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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INDEX

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A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

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THURSDAY, MAY 5, 1887.

LOOMIS'S CONTRIBUTIONS TO METEOROLOGY.

Contributions to Meteorology. By Elias Loomis, LL.D., Professor of Natural Philosophy and Astronomy in Yale College, &c. Revised Edition. Chapter II. (New Haven, Conn., 1887.)

IN Chapter I. of the revised issue of these valuable contributions to meteorology, Prof. Loomis summarised, and in some directions materially extended, the laborious investigations on which he had been engaged for the previous ten years, with reference to areas of low atmospheric pressure—their form, magnitude, direction, and velocity of movement (NATURE, vol. xxxiii. p. 49). In Chapter II. he similarly deals with the allied problems of meteorology which group themselves round areas of high atmospheric pressure, investigates their form, magnitude, direction, and velocity of movement, and concludes by tracing some of the more important relations of areas of high to areas of low atmospheric pressure.

It is premised that areas of high pressure exhibit characteristics quite unlike those which attend areas of low pressure. A typical example, contrasting the two sets of phenomena, is given in the great storm of February 5, 1870, where, in the centre of the Atlantic, pressure fell to 27·33 inches, whilst at the same time, and contiguous to this great cyclone, there occurred a strongly-marked anticyclone in Europe, where, over an extensive region, pressure rose to upwards of 31·00 inches. Near the centre of the cyclone the winds blew with hurricane force, but near the centre of the anticyclone the atmosphere was well-nigh calm. In the cyclone the winds circulated about the low centre in a direction contrary to the hands of a watch, with, at the same time, a decided tendency inwards upon the centre of low pressure; whereas in the anticyclone the winds circulated about the high centre in the same direction as that of the hands of a watch, with a decided tendency outwards from the centre of high pressure.

As regards their form, cyclones nearly always approximate closely to the circular, elliptical, or oval forms; but on the other hand, the departures of anticyclones from the circular form are nearly always quite palpable; indeed, we rarely find an isobar about a centre of high pressure which does not clearly deviate from the figure of an exact circle. The isobars are generally elongated, and are thus elliptical rather than circular, and the elongation is sometimes very great.

In order to ascertain the general form of the isobars of anticyclones, Prof. Loomis has examined and measured with care 238 well-defined cases, shown on the United States weather-maps. The result indicates that the average ratio of the longest to the shortest diameter was 1·91. Whilst the longest diameter may be turned in any direction, 72 per cent. of the cases occurred in the azimuth from 0° to 90°, the direction of maximum frequency being N. 44° E. It is noteworthy that these results intimately agree with those found for low-pressure areas, showing, together with other results, how intimately cyclones and anticyclones are connected.

A similar investigation has been carried through as regards these two great systems of pressure for the Atlantic and Europe. In this part of the inquiry the number of cases examined was 252, which showed that the average ratio of the longest to the shortest diameter was 1·84; and as respects the direction of the longest diameter, 69 per cent. of the cases occurred in azimuth from 40° to 130°, the direction of maximum frequency being N. 75° E. Thus, while there is a remarkable correspondence between the results for these two great regions of the globe as respects the form of anticyclonic isobars, there is not the same accord as to the prevalent direction of the longest diameter. It may be well to emphasise here this vital difference between the anticyclones of North America, and those of the much larger Europasian continent. About three-fourths of these anticyclones occurred during the six colder months of the year.

When an area of high pressure is situated between two areas of low pressure not far apart from each other, the anticyclone generally takes a very elongated form, and sometimes the isobars surrounding this high area extend a distance of several thousand miles, with but little curvature. The isobars for January 14, 1876, show between one widespread low-pressure area over the North Atlantic and Arctic Oceans, and another over the Mediterranean, a belt of high pressure, with nearly straight and parallel isobars of 30·3 and 30·5 inches, extending from south-west to north-east for about 3000 miles.

With the view of throwing some light on the origin of anticyclones, all those cases in the States in which the barometer rose to 30·85 inches or upwards during the twelve years ending with 1884 were examined. Of such anticyclones there were fifty-two cases, distributed as follows: in October one, November ten, December eight, January fourteen, February sixteen, and March three; the earliest having occurred on October 26, and the latest on March 12. The majority of the whole number of cases occurred north of lat. 46°, only two being south of lat. 40°; and the influence of longitude was equally strongly marked: 82 per cent. of the cases occurred west of long. 90° W., and nearly half of the number west of long. 100° W. These areas of high pressure indicate a general movement towards the south-east, the prevalent direction being S. 40° E. at a mean velocity of twenty-one miles an hour.

An instructive point in the history of anticyclones is the low temperature which accompanies them. It is shown from a somewhat exhaustive examination that, when pressure is unusually high, temperature is generally very much below the mean, and that the amount of the depression increases with the height of the barometer; and it is further shown that with a given barometric pressure the temperature depression is greatest in the neighbourhood of Manitoba, or approximately in the centre of the continent. These relations are sometimes shown on the weather-maps with a surprising closeness, of which an instructive instance is figured on Plate XXV., which gives the isobars and thermic isabnormals for December 24, 1872. In the case of the more strongly pronounced anticyclones, the depression of the temperature below the mean for the season is on the average 38°·7. The maximum temperature depression may occur in any direction from the place of highest pressure, but it is generally about 400 miles distant from it, and most frequently on its northern side. Indeed, in only 12 per cent. of the cases examined did the place of greatest temperature depression correspond exactly with the place of highest pressure. Similar relations exist as regards the summer anticyclones, with the important differences that their maximum pressure is generally about half an inch less than that of winter cyclones, and their temperature depression below the mean of the season only about 8°·0.

The average breadth of the American anticyclones is 2587 miles, which is nearly equal to the breadth of the continent in lat. 40°, and an examination of the maps of the *International Bulletin* show that a large number of high-pressure areas are 4000 miles in length. The average distance from the centre of an anticyclone to the centre of the cyclone on its east side is 2371 miles, and to the centre of the cyclone on its west is 2381 miles, being thus nearly equal. Since, however, the average value of the lowest isobars of the cyclones on the east side is 29·190 inches, and on the west 29·570 inches, it follows that the gradients on the east side of the United States anticyclones are about twice as great as those on the west side. This is an extremely valuable result, as showing the powerful influence of the Atlantic in lowering atmospheric pressure, and thus intensifying storms as they near its seaboard in their course eastwards. Indeed, as regards those cyclones which show a pressure not exceeding 29·00 inches at their centre—in other words, the more important storms—five-sixths of the number occurred near the Atlantic. The cases examined numbered 131, and were distributed through the months as follows, beginning with January: 14, 16, 25; 10, 4, 1; 0, 2, 2; 7, 26, and 24; there having thus occurred 105 during the five months from November to March, and only 5 during the four months from June to September.

It is shown that, while an area of extraordinarily high pressure uniformly has an area of low pressure both on its east and west sides, the barometer in these areas seldom sinks very low, and when it does sink very low the centre of the low pressure is very remote; and on the other hand that an area of very low pressure has an area of high pressure both on its east and west sides, but in these areas of high pressure the barometer seldom rises very high.

As regards anticyclones, Prof. Loomis makes it abundantly clear that the height to which pressure rises in their centres is approximately proportioned to the depression of temperature over the region at the time. In these circumstances, the air, being condensed by the cold, sinks more and more into the lower regions of the atmosphere, thus leaving a less pressure in the upper regions than prevails all round at these heights, and consequently upper currents set in all round towards and upon the anticyclonic region, by which its high pressure is further increased. Hence it follows that it is over one and the same region, viz. the more inland portions of the Europasian continent, where depressions of temperature are greatest, most widespread, and most persistent, and where the best-pronounced anticyclones occur in the colder months of the year. This state of things is entirely due to the great extent of the Old continent, by which a very large portion of it is remote from the disturbing influence of the ocean. The same cause explains a singular feature of the anticyclones of this part of the globe, viz. the absence they frequently show, sometimes for weeks together, of any movement of translation over the earth's surface, being so different in this respect from the anticyclones of the comparatively narrow continent of America, which have a distinctly progressive movement towards the south-east, already referred to.

On the other hand, low pressures are distinguished by the relatively high temperature which accompanies them. The highest temperature occurs about 300 miles on the south or east side of the low centre of the cyclone, the average excess above the mean temperature being 22°·3. No inconsiderable part of the low pressure of cyclones is occasioned by the high temperature and excessive moisture which accompanies them. But, while all, or nearly all, of the high pressure of anticyclones may be accounted for by the very low temperatures which overspread the same region at the same time along with the resulting upper currents concentrating upon them from adjoining cyclonic regions, it is quite different with the low pressures of cyclones. In the case of cyclones the problem is complicated by the strong winds, the copious precipitation, and the ascending currents, which affect the results in ways which no physicist has yet been able to explain.

The problem of the weather would be immensely simplified if it could be explained, first, how it is that air highly heated and approximately saturated with aqueous vapour comes to overspread a definite well-marked region, usually a very extensive one, while the atmosphere over contiguous regions remains relatively cold and dry; and, secondly, how the low temperature which is so characteristic a feature of anticyclones and of the rear of cyclones has its origin. The complete changes which weather-maps show us to take place in these respects over enormous tracts of the earth's surface within even such brief spaces of time as twenty-four hours, point to volumes and velocities of translation of masses of air through the upper currents which meteorologists have yet scarcely taken cognisance of. Prof. Loomis's paper is a well-worked-out and valuable contribution towards the solution of this all-important practical problem.

THE GAME OF LOGIC.

The Game of Logic. By Lewis Carroll. (London: Macmillan and Co., 1887.)

MR. "LEWIS CARROLL'S" new book has both the merit and the sterilitywhich might be expected from a fresh and rather independent system of diagrammatic or visual logic. That is to say, it is ingenious and closely worked out, but cannot be said to advance either our theoretic knowledge of reasoning processes or the more practical craft of dealing with assertions and arguments as found in ordinary life. Perhaps so trying a standard ought not in fairness to be applied to the work before us, which is intended—so the preface and the title hint—to amuse those who would otherwise play with some less instructive puzzle. But it is because it seems unlikely that "The Game of Logic" will be patiently studied as a game, or would reward such patience by providing "an endless source of amusement," that one is inclined to consider it rather as a contribution to visual logic than to any other form of literature.

Technically speaking, the propositions contemplated are "extensive" and "existential." That is to say, all assertions are supposed to be concerned with things as members of classes, and to be translatable into the form "Of the class *u x* there are none (or 'some') which are *u y*"; so that, for example, "I feel much better" becomes (pp. 50, 70, and 9) "Of the class of persons who are I, there are none who do not feel much better, and there are some who do"; and "There is no one in the house but John" becomes "Of the class of persons who are not John, none are in the house." Of course these are here chosen as examples of difficulty in translation: a great many propositions can be much more naturally treated as expressing class-relations.

Every assertion is thus supposed to perform two functions: it provides a certain number of labelled compartments, and it tells us that one or more of these is either empty or "occupied." If, for example, we regard a given proposition as containing only two terms, *x* and *y*, the compartments provided are four in number: *x y*, *x non-y*, *non-x y*, and *non-x non-y*. (Mr. Lewis Carroll however adopts Mr. Maccoll's neater notation for the negative terms.) And if two such propositions have one term in common—say, if the first speaks of *x* and *m*, and the second of *y* and *m*—the two can be taken together as one complex assertion providing eight compartments, and giving, under certain conditions, more information about *x* and *y* than is to be found in either proposition when taken singly.

As regards the division of the universe into compartments, there is little to distinguish Mr. Lewis Carroll's system from others which, like Boole's or Jevons', make use of a similar framework; and nothing to distinguish it from Mr. Venn's, except the form of picture employed. But the special features consist in (1) making all affirmative propositions assume the real existence of the subject, and (2) providing for the expression of certain forms of proposition which are usually found difficult to put into diagrams. For example, by means of the restricted sense given to the assertion that a compartment is *occupied*, the "particular" proposition, which often causes trouble, becomes easily expressed. In Mr. Venn's scheme, propositions either tell us that a compartment is empty or else tell us nothing about it, whereas here the information that a compartment is *occupied* (meaning merely "not empty") can also be distinctly given; so that to mark the compartment *x y* as "occupied" expresses "some *x* are *y*" and "some *y* are *x*" together. Again, "some *x* exist," "no *x* exist," and "*only* some *x* are *y*" are readily and neatly represented; and a distinction, due to the assumption of real existence, is drawn between "all *x* are *y*" and "no *x* are *non-y*," and similarly between "no *x* are *y*" and "all *x* are *non-y*."

The author is in one or two instances not quite fair to the more old-fashioned logicians. It is not the case, as stated on p. 30, that those who regard the universal negative as asserting incompatibility of attributes would therefore regard the assertion "No policemen are eight feet high" as *false*. They might rather be inclined to consider any such *a priori* treatment of it as a case of *petitio principii*, since, if the assertion be supposed to intend giving information at all, the question whether or no the attributes *are* compatible is supposed to be at issue. Again, there are probably few logicians in existence who would simply turn away with scorn from the premises given on p. 35. Even the more pedantic would rather suggest that a very slight verbal alteration performed on the minor premise by recognised processes (conversion and obversion) would give a legitimate syllogism in *celarent*. But no doubt Mr. Lewis Carroll's method deals with such premises more directly.

It is held by some who ought to know, that logic might be taught at a much earlier age than is now the fashion; and possibly the book should be regarded as an attempt to make a beginning in this direction. If so, the advantages and disadvantages of the scheme seem about evenly balanced. Certainly the diagram is simple to draw, full in its information, and easily read; but these good qualities are gained at some expense. The difficulty of forcing all assertions into the forms of class-inclusion is not indeed peculiar to Mr. Lewis Carroll's system; but the assumption of the real existence of the subject leads, in certain cases, to additional troubles of interpretation.

For example, the whole range of hypothetical and abstract assertions would require to undergo some preliminary torture: wherever a sentence intends to assert that one fact conditions another, without expressing an opinion as to the actual fulfilment of the condition, we should have to contraposit the sentence and restrict it to the negative form. Thus "policemen seven feet high would attract a crowd" seems to require reading: "Things that would fail to attract a crowd are not policemen seven feet high"—a form which most children would think unnatural. Indirectly the child might learn, by this system as well as by any other, that the real difficulty of avoiding logical blunders lies more in translating ordinary language into carefully-defined symbols, than in the operations afterwards performed by merely mechanical rules. But teachers who desire rather to show by diagrams the direct binding force of deductive reasoning would do well to select for illustration those propositions which can be most simply and naturally regarded as expressing the "extensive" comparison of classes. It is only fair to Mr. Lewis Carroll to add that the examples he provides for exercise will not perhaps do more to keep alive the notion that logic is trivial than many of those that are given in perfectly sober text-books. As things are, the junior student seems, not unnaturally, to believe that the safest plan of answering logical conundrums is to find out the most ingenious and roundabout way of avoiding the answer that would be dictated by common-sense. It is worth considering whether the correction of this tendency is not a more pressing need in the teaching of elementary logic than even the best new variations on the old surprise of finding that absurdity in matter is no bar to legitimacy in form. ALFRED SIDGWICK.

OUR BOOK SHELF.

Nitrate of Soda: its Importance and Use as a Manure. A Prize Essay. By A. Stutzer, Ph.D., re-written and edited by P. Wagner, Ph.D. (London: Whittaker and Co., 1887.)

IN the spring of 1885 a committee of South American nitrate of soda manufacturers offered a prize for the best popular essay on the above subject. The judges were Profs. Grandeau (France), Adolf Mayer (Holland), Petermann (Belgium), Thoms (Russia), P. Wagner (Germany), and Mr. Warington (England). The prize was divided between two of the competitors, Dr. A. Stutzer, President of the Bonn Agricultural Experiment Station, and Prof. Adolphe Damseaux, of Gembloux.

The book now presented embodies the main points of Dr. Stutzer's essay, combined with the views expressed by the committee of judges, and important matter contained in the second prize essay. The subject is divided into two parts, in the first of which theoretical questions as to the advantages to be derived from the use of nitrate of soda are ably and thoroughly sifted, and the error of many popular prejudices is exposed. The important question of the impoverishment of the soil is carefully discussed, and the conclusion arrived at that it causes an increased consumption of nutrient substances only in proportion to the increase of crop, and does not increase the percentage of potash and phosphoric acid in the crop, and even that a larger crop is produced with proportionally smaller use of the two latter materials. It is also shown that, although nitrate of soda does cause a large increase of straw, yet it quite as certainly causes an increase in the quantity of grain.

The second portion of the book contains very clear instructions for the use of nitrate of soda with various crops, and will prove a capital practical guide for farmers.

A. E. T.

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. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Units of Weight, Mass, and Force.

IN a letter to NATURE, dated March 29, I stated that we have "no names for units of velocity, acceleration, impulse, momentum, &c." This cannot be said now. Through the kindness of Messrs. Macmillan and Co., I have, this morning, received a copy of "Dynamics for Beginners," by the Rev. J. B. Lock, M.A. In this book the units of velocity and of acceleration are named a velo, and a celo, respectively. Other units also have received names, "the use of which" (as the author justly observes) "will be found to simplify considerably the language of the subject." The preface to this book is dated April 1887.

Bardsea, April 23. EDWARD GEOGHEGAN.

Earthquake in the Western Riviera.

AN interesting fact in connexion with the late disastrous earthquake which did such damage along the Western Riviera of Liguria on the morning of February 23 last, and with which I only lately became acquainted through my friend Dr. Bellotti, of Milan, who was at Nice at the time, is that during the days immediately following the catastrophe quite a large number of deep-sea fish were taken dead or half dead in shallow water or found stranded on the beach. This happened more especially in the immediate neighbourhood of Nice, whose sea, as that of Messina, has long been well-known for its richness in deep-sea fish.

I have since taken the pains to inquire more fully into the subject, which has a very special interest for me, and through Gal frères I have learnt that the following species were taken: *Alepocephalus rostratus* (mostly dead and floating), numerous; *Pomatomus telescopium*, several; *Tetragonurus cuvieri*, one specimen; *Dentex macrophthalmus*, many; *Scopelus elongatus*, several; *Scopelus humboldti*, several; *Spinax niger*, abundant.

Alepocephalus rostratus is a typical deep-sea form, only found as yet, and rarely, during the summer along the Western Riviera by deep-sea liners.

Several of the fish above mentioned are in my possession.

Firenze, April 20. HENRY H. GIGLIOLI.

The Boiling-Point and Pressure.

A VERY convenient lecture experiment to show that the boiling-point of liquids is lowered by diminishing the pressure of the surrounding medium, may be made with one of Ducretet's carbon-dioxide tubes. The lower part of the tube contains the CO_2 in the liquid condition, while the upper part is, of course, filled with the same body in the gaseous state. By subjecting this upper half of the tube to a jet of ether spray, the pressure of the inclosed gas will gradually diminish, and after a few seconds the liquid below will enter into brisk ebullition.

The experiment is readily adaptable for projection on a screen. M. F. O'REILLY.

St. Joseph's College, Clapham.

A Sparrow chasing Pigeons.

MR. J. JENNER WEIR (NATURE, vol. xxxv. p. 584) says that he has never observed a sparrow to chase a pigeon *except when on the wing*. I wish to say that I have frequently witnessed the occurrence, having kept pigeons for a number of years. I have often seen sparrows chase pigeons on the house-tops, particularly when the former have had their nests in the vicinity. One season, about ten years ago, I well remember the annoyance these impudent birds caused to the pigeons, for whenever the latter were let out in the morning to air themselves the sparrows would immediately attack them and continue the harassment without intermission all day long. On one occasion, during the same year, I recollect a very amusing scene between a cock sparrow and a cock tumbler pigeon. The former had just flown on to the house-top with a large piece of bread in his bill, when the pigeon advanced rapidly towards him with the intention of seizing the dainty morsel. The plucky sparrow, however, was in readiness for him, and dropping the bread he made a bold onslaught on his larger feathered relation, pecking vigorously at his rear quarters. The pigeon not expecting an attack from such an insignificant foe, and being utterly unprepared for a "round," did not attempt to use its wings as weapons, as is its custom, but contented itself with cooing, and aiming a few rapid pecks with its beak, and then ignominiously took to flight, leaving a few feathers behind in its hurry. No sooner, however, had it quitted the field than about half a dozen other sparrows, who had been sitting on the chimney-pots in the neighbourhood watching the affray, dashed after it and chased it round and round the house, about half a dozen times, before they considered that it had had sufficient punishment. Such is an example of the annoyance my pigeons were sometimes subjected to by the sparrows. I may add that I have frequently observed sparrows and starlings fighting, and also on one occasion saw a sparrow chase a jackdaw which had evidently been attempting to take the sparrow's eggs or young ones. The records of a few other curious instances of birds fighting which I will briefly extract from my diary may perhaps be interesting. "1880 May. Saw a starling attack and drive away two jackdaws which had gone in quest of its eggs. 1880 June. Saw a cloud of redpolls chase a cuckoo. 1883 June. Witnessed a desperate fight between a flock of jackdaws and a heron, in mid air. 1885 June. Saw a skylark chase a cuckoo and drive it away. 1885 August. Saw a mistletoe thrush chase a sparrow-hawk and drive it away. 1886 July. Witnessed a great battle between a large flock of starlings and a flock of rooks, the former having invaded the feeding-grounds of the latter." W. HARCOURT BATH.

Ladywood, Birmingham, April 25.

THE ROYAL SOCIETY SELECTED CANDIDATES.

THE following is the list of the fifteen candidates selected by the Council of the Royal Society, at their meeting last Thursday, to be recommended for election into the Society. The ballot will take place on Thursday, June 9, at 4 p.m. We print with the name of each of the candidates the statement of his qualifications.

JOHN YOUNG BUCHANAN, M.A. (Glasgow),

F.R.S.E., F.C.S. Chemist and Physicist to the *Challenger* Expedition, 1872–76. Invented and improved apparatus and methods for collecting and analysing ocean water. Author of "Report of the Specific Gravity of Ocean Water" (Part 2, vol. i., of "The Voyage of H.M.S. *Challenger*"). Since the return of the *Challenger*, Mr. Buchanan has continued his investigations in the steam-yacht specially fitted up by him for the purpose. The following are the titles of some of the papers contributed by him to scientific Transactions and Journals: "Sur l'acide chloropropionique" (*Comp. Rend.*, lxv., p. 417); "On the Formation and Decomposition of some Chlorinated Acids" (Proc. Roy. Soc. Edin., vii., p. 419); "On the Absorption of Carbonic Acid by Saline Solutions" (Proc. Roy. Soc., xxii., pp. 192 and 483); "On the Specific Gravity of Ocean Water" (Proc. Roy. Soc. Edin., ix., 283); "On the Compressibility of Glass" (Trans. Roy. Soc. Edin., 1880); "On a Solar Calorimeter, and some Observations made with it in Upper Egypt" (Proc. Roy. Soc. Edin., xi., 827).

J. THEODORE CASH, M.D., C.M.,

Has devoted himself to physiological and pharmacological science, and has made discoveries in these as described in the following works, of which he is the author:—"On the Relationship between the Muscle and its Contraction" (*Journ. Anat. Phys.*, vol. xv.); "Ueber den Antheil des Magens und des Pancreas an der Verdauung des Fettes" (*Arch. für Physiol.*, 1880); "Description of a Double Cardiograph for the Frog's Heart" (*Journ. of Physiol.*, vol. iv.). With Dr. Lauder Brunton:—"Contributions to our Knowledge of the Connexion between Chemical Constitution, Physiological Action and Antagonism" (Phil. Trans., 1884); "Action of Alkaloids on Oxidation" (St. Barth. Hosp. Rept., vol. xviii.); "Influence of Heat and Cold on Muscles poisoned by Veratria" (*Journ. of Physiol.*, vol. iv.); "On the Valvular Action of the Larynx" (*Journ. Anat. Phys.*, vol. xvii.); "On the Effect of Electrical Stimulation of the Frog's Heart, and its Modification by Heat, Cold, and the Action of Drugs" (Proc. Roy. Soc., vol. xxxv.); "Ueber Vorbeugende Gegengifte" (*Centralb. für d. Med. Wiss.*, 1884); and several other papers. With Dr. Yeo:—"The Effects of certain Modifying Influences on the Latent Period of Muscle Contraction" (Proc. Roy. Soc., vol. xxxiii.); "The Variations of Latency in certain Skeletal Muscles" (Proc. Roy. Soc., vol. xxxv.); "On the Relationship between the Active Phases of Contraction and the Latent Period of Skeletal Muscle" (*Journ. of Physiol.*, vol. iv.).

SIR JAMES NICHOLAS DOUGLASS,

Civil and Mechanical Engineer. Engineer-in-Chief to the Hon. Corporation of Trinity House. Is a Member of the Smeatonian Society of Civil Engineers and a Member of the Council of the Inst. Civ. Eng. Member Inst. Mech. Eng. Has been attached to coast signalling since 1847, and the development of electricity for coast lighting since 1862. Has designed and erected several rock lighthouses on the coasts of this country and abroad; some of these works being of exceptional difficulty, for which he received on two occasions a testimonial from the Hon. Corporation of Trinity House. He designed and erected the new Eddystone Lighthouse. On the completion of this work he received the honour of knighthood. He has very materially improved the optical apparatus, lamps, lanterns, and fog-signal apparatus of lighthouses and light-vessels. Is the author of several papers. For one on "The Wolf Rock Lighthouse," and another on "The Electric Light applied to Lighthouse Illumination" (Proc. Inst. Civ. Eng., vols. xxx. and lvii.), he received the Telford and Watt Gold Medals of the Institution of Civil Engineers.

PROF. J. A. EWING, B.SC. (Edin.),

Professor of Engineering, University College, Dundee. Author of papers contributed to the Royal Society treating of the Thermo-electric quality of metals; the Electric effect due to twisting Iron and Steel wire when magnetised; and various papers on the Magnetic Qualities of Iron and Steel, all of which have been published (see Proc. Roy. Soc., Nos. 205, 210, 214, 216, 220, and the present volume of the Transactions). Well known for his work in connexion with the observation and recording of Earthquake Phenomena, having contributed numerous papers on this subject to the Seismological Society of Japan; is also joint author with the late Prof. Fleeming Jenkin of several papers communicated to the Royal Society of Edinburgh, and published in the Transactions of that Society. Was for five years in Japan as Professor of Engineering, and has held his present Chair (Dundee) for two and a half years.

PROF. GEORGE FORBES, M.A.,

F.R.S. Edin., F.R.A.S. Member of the Astronomische Gesellschaft, of the Electro-technical Society of Vienna, and of the Society of Telegraph-Engineers. Associate of the Inst. Civil Engineers. Chevalier of the Legion of Honour. Formerly Professor of Natural Philosophy at Anderson's College, Glasgow. Consulting Engineer (Electrical). Author of the following amongst other papers:—"On the Meteoric Shower of November 14, 1866" (*Phil. Mag.* 1867); "On the Meteor-Shower of August 1867;" "On Astronomical Refraction;" "On certain Connexions between the Molecular Properties of Metals;" "On Irradiation;" "On Thermal Conductivity;" "On an Instrument for Indicating and Measuring the Fire-damp in Mines," &c. (Brit. Assoc. Rep., 1867, 1872, 1873, 1878, &c.); "Note on the Zodiacal Light;" "On the After-glow of Cooling Iron at a Dull Red Heat;" "On Diamagnetic Rotation;" "On Comets;" "On the Theory of the Telephone;" "On an Ultra-Neptunian Planet, &c." (Proc. Roy. Soc. Edin., vols. viii., ix., x.); "On the Velocity of White and Coloured Light" (in conjunction with the late Dr. James Young, F.R.S.—Phil. Trans., 1882). Also author or joint author of the following separate works, published by Macmillan:—I. "The Transit of Venus;" II. "Rendu's Theory of Glaciers."

WILLIAM RICHARD GOWERS, M.D. (Lond.),

F.R.C.P. Fellow of University College, London. Physician to University College Hospital, and to the National Hospital for the Paralysed and Epileptic. Distinguished as a Physiologist and Physician. Attached to science and anxious to promote its progress. Contributor of papers on:—"The Automatic Action of the Sphincter Ani" (Proc. Roy. Soc., 1877); "The Decupation of the Optic Nerves" (*Centralb. für d. Med. Wiss.*, 1878); "The Enumeration of Blood-Corpuscles" (*Practitioner*, 1878); "The Estimation of Hæmoglobin" (Trans. Clin. Soc., 1878); "The Nature of the So-called Tendon-reflex Phenomena" (Trans. Roy. Med. Chir. Soc., vol. lxii.); "The Mechanism of the Movements of the Eyelids" (*ibid.*); "A Reflex Mechanism in the Fixation of the Eyeballs" (*Brain*, vol. ii.); "The Relation of the Fifth Nerve to Taste" (*Journ. of Phys.*, 1883); "The Origin of the Sixth Nerve" (*Centralb. für d. Med. Wiss.*, 1878); "Unilateral Lesion of the Spinal Cord" (Trans. Clin. Soc., vol. xi.). Author of treatises:—"Manual and Atlas of Medical Ophthalmoscopy," second edition; "Epilepsy and other Chronic Convulsive Diseases;" "Diagnosis of Diseases of the Spinal Cord," second edition; "Diagnosis of Diseases of the Brain;" "Pseudo-hypertrophic Muscular Paralysis;" "Diseases of the Walls of the Heart;" "Leucocythæmia;" "Hodgkin's Disease;" Reynold's System of Medicine;" "Path. Anat. of Hydrophobia;" "Mode of Development of Spindle Cells," &c., &c.

ALEXANDER BLACKIE WILLIAM KENNEDY, M.I.C.E.

Civil Engineer. Professor of Engineering and Mechanical Technology, University College, London. Has rendered considerable services to Engineering Education by the establishment of an Engineering Laboratory at University College, and by his investigations there made into the strength of materials (Journ. Soc. Arts, 1875). The students are there taught systematic experimental work, and the plan first introduced by Prof. Kennedy has been generally followed. Tests were there made on strength and elasticity for the Indian Government, and accounts of other tests are given in the reports on riveted joints for Institution of Mechanical Engineers, and in a paper on mild steel written for Roy. Inst. Brit. Architects. He translated and edited "Theoretische Kinematik." He is the author of numerous papers connected with Engineering, as the "Critical Description of the Steam-Engines, &c.," in the Vienna Exhibition, 1873 *Engineering*, June, December, 1873); "Air-Engines" (*Engineering*, 1875); "Geometrical Solutions of some Statical Problems" (Proc. Lond. Math. Soc., vol. ix.). He has also designed the iron and concrete framework and roof of the new Alhambra Theatre.

GEORGE KING, M.B.,

F.L.S. Superintendent of the Royal Botanical Gardens, Calcutta, and of the Government Cinchona Plantations of Darjeeling. Formerly Superintendent of the Botanical Gardens of Saharunpur. Author of "Notes on the Lion of Aboo" (Proc. Asiat. Soc. Beng., 1868); "On the Birds of the Goona District" (Journ. Asiat. Soc. Beng., 1868); "Notes on the Vegetable Products and Farm Foods of Rajpootana and Marwan;" "Observations on the genus *Ficus*, and on the Fertilisation of *F. hispida*;" "A Monograph of Indian Fici" (in course of publication). Eminent as an Indian Botanist and Quinologist, and for the services he has rendered to Botanists and Naturalists in India.

SIR JOHN KIRK, G.C.M.G., M.D.,

F.L.S. H.M. Agent and Consul-General, Zanzibar. Chief Officer and Naturalist of Dr. Livingstone's Government Expedition to the Zambesi, Nyassa Country (1858-63), during which he made large collections, observations, and drawings of great scientific value. Author of numerous contributions to the Botany, Zoology, and Geography of Eastern Tropical Africa, published in the Journals of the Linnean and Zoological Societies, the *Ibis*, &c. During Sir John Kirk's residence, of nearly twenty years, in Zanzibar, he has rendered most important services to the various Expeditions despatched by English and Foreign Governments and by private bodies for the exploration of Central Africa, directing their routes, superintending their equipments, and encouraging them in the formation and transmission of Zoological, Botanical, and Ethnological Collections.

OLIVER JOSEPH LODGE, D.SC. (Lond.),

Professor of Physics in University College, Liverpool. Distinguished for his acquaintance with the science of Physics. Author of numerous papers on Physics published in the *Philosophical Magazine*, Proceedings of the Physical Society of London, Reports of the British Association, and elsewhere, including (among many others) the following:—"On some Problems connected with the flow of Electricity in a Plane;" "On a Model illustrating mechanically the passage of Electricity through Metals, Electrolytes, and Dielectrics according to Maxwell's Theory;" "On a Mechanical Illustration of Thermo-electric Phenomena;" "On a Modification of Mance's Method of measuring Battery Resistance;" "On a Method of measuring absolutely the Thermal Conductivity of Crystals;" "An attempt at a Systematic Classification of some of the Forms of Energy;" "On Intermittent Currents and the Theory of the Induction Balance;" "On the Dimensions of a Magnetic Pole in the Electrostatic System of Units;" "On the Phenomena exhibited by Dusty Air in the neighbourhood of strongly Illuminated Bodies" (jointly with the late J. W. Clark); "On the seat of Electromotive Force in the Galvanic Circuit;" "On the Identity of Energy;" "On Electrolysis;" author of a book on "The Elements of Mechanics" (W. and R. Chambers).

PROF. JOHN MILNE,

F.G.S., Associate and Hon. Fellow of King's Coll. Lond. Royal Exhibitioner at Royal School of Mines, Lond. Professor of Mining and Geology in the Imperial College of Engineering, Department of Public Works, Japan. Studied in Freiberg; travelled in Iceland; engaged, in 1873-74, in mining in Newfoundland; accompanied Dr. Beke as geologist to North-West Arabia; travelled across Russia, Siberia, Mongolia, and China to Japan; visited the Kurile Islands, the Corean frontier, California, &c. Author of "Travelling Notes" (*Geol. Mag.*, 1877, pp. 237, 289, 389, 459, 511, 557; 1878, pp. 29, 62); "The Volcana of Oshima" (*op. cit.*, 1877, p. 193); "On the Form of Volcanoes" (*op. cit.*, 1878, p. 337, pl. IX.; and 1879, p. 506); "The Volcanoes of the Kurile Islands" (*op. cit.*, 1879, p. 337, pl. IX.); "On the Cooling of the Earth" (*op. cit.*, 1880, p. 99); "Physical Features and Mineralogy of Newfoundland" (Quart. Journ. Geol. Soc., 1874, vol. xxx., p. 722); "Geological Notes on the Sinaitic Peninsula and N.W. Arabia (*ibid.*, vol. xxxi., pp. 1-28); "Seismic Science in Japan" (Trans. Seismol. Soc., Yedo, vol. i., pp. 1-34); "The Earthquake of February 22, 1880" (*op. cit.*, vol. i., part ii., pp. 1-116); "The Earthquakes of Yedo Plain" (*ibid.*, vol. ii., pp. 1-38); "The Peruvian Earthquake of May 9, 1877" (*ibid.*, vol. ii., pp. 51-96); "Experiments in Seismology" (*ibid.*, vol. iii., pp. 12-64); "Notes on the Great Earthquakes of Japan" (*ibid.*, vol. iii., pp. 65-102); "The Earthquake of March 8, 1881" (*ibid.*, pp. 127-136); "Distribution of Seismic Activity in Japan" (vol. iv., pp. 1-30); "The Systematic Observation of Earthquakes" (*ibid.*, vol. iv., pp. 87-117; joint papers with Mr. T. Gray, B.Sc., F.R.S.E.); "On Seismic Experiments" (Phil. Trans., 1882, and Proc. Roy. Soc., 1881); "Earthquake Observations in Japan" (*Phil. Mag.*, November 1881); "Elasticity and Strength, Constants of Certain Rocks" (1882); "Reports on Investigation of the Earthquake Phenomena of Japan" (Brit. Assoc. Rep., 1881 and 1882). Intrusted with three grants from British Association for the investigation of earthquake phenomena. Author of numerous other papers on geology, mineralogy, mining, &c. Has specially devoted himself to the study of earth-movements.

REV. OCTAVIUS PICKARD-CAMBRIDGE, M.A.,

Rector of Bloxworth, Dorset. Distinguished for his acquaintance with Zoology, particularly Arachnology. Has published the following works:—"A History of British Spiders to 1881," entitled "Spiders of Dorset," with Preface and General Introduction in two vols., pp. 1-625, plates 1-6; numerous papers in the Proc. Zool. Soc. of London, especially "On the Spiders of Palestine and Egypt from 1869 to 1883;" article "Arachnida" in "Encyclop. Brit.," 1874; articles "Arachnida" in "Zoological Record," 1870-1883; numerous papers "On Arachnida of various Countries" in *Ann. Mag. Nat. Hist.*, 1860-83; articles "On British Spiders" in Trans. Linn. Soc., 1868-70; articles "On British and Exotic Spiders" in Proc. Linn. Soc., vols. x. and xi.

GEORGE JAMES SNELUS,

A.R.S.M., F.C.S., Memb. I.M.E. Metallurgist. Is distinguished as a chemist and metallurgical engineer. He was the first to make pure steel from phosphoric pig iron in a Bessemer converter lined with basic materials, a discovery of national importance, for which he was, in 1883, awarded the Bessemer Gold Medal of the Iron and Steel Institute. He has developed points of much scientific interest in connexion with the mode of existence of carbon and silicon in iron and steel, and has specially studied the liquidation of fluid bodies during the solidification of steel. He has also conducted important researches on the relation between the chemical composition and mechanical properties of steel used for rails. Is the author of the following papers :—"On the Condition of Carbon and Silicon in Iron and Steel"; "On the Composition of the Gases evolved from the Bessemer Converter"; "On the Scientific Features of the Danks' Puddling Furnace"; "On the Manufacture and Use of Spiegeleisen"; "On Fire-Clay and other Refractory Materials"; "On the Direct Process of Steel Making"; "On the Removal of Phosphorus and Sulphur in the Bessemer Process"; "On the Distribution of Elements in Steel Ingots"; "On the Chemical Composition and Testing of Steel Rails."

THOMAS LORD WALSINGHAM.

Trustee of the British Museum. A practical Entomologist and ardent student and collector of the Microlepidoptera, of which he has an extensive private collection. In 1872, Lord Walsingham made a special expedition to Northern California and Oregon, and besides forming a large series of the Microlepidoptera of that district, many of which were unknown, obtained a good collection of other Natural History specimens, which he presented to the Cambridge Museum.—[ADDENDUM.—Author of "Catalogue of North American Tortricidæ in the British Museum," 4to., being Part 4 of "Illustrations of Typical Specimens of Lepidoptera Heterocera," 1879, and published by the Trustees. "On some Probable Causes of a Tendency to Melanic Variation in Lepidoptera of High Latitudes" (Presidential Address, Trans. Yorksh. Nat. Union, 1885); and various papers in the Trans. Ent. Soc. and Proc. Zool. Soc., 1880–84.]

WILLIAM WHITAKER, B.A. (Lond.),

F.G.S. Assoc. Inst. C.E. District Surveyor on the Geological Survey of England and Wales. Contributor to, or author of, ten of the Memoirs of the Geological Survey, among these being—"The Geology of the London Basin;" "The Geology of the N.W. part of Essex, and the Geology of the country round Ipswich;" of Papers in the Quarterly Journal of the Geological Society on the "Western end of the London Basin;" "The Lower Tertiaries of Kent;" "On some Borings in Kent, and other subjects;" and of many other papers on geology, more especially of the Cretaceous and Tertiary Beds, and on the subject of water-supply. Has also drawn up many lists valuable to the student of geological literature, and was for several years Editor of the *Geological Record*. Awarded the Murchison Medal of the Geological Society, 1886.

THE PARIS ASTRONOMICAL CONGRESS.

WE have now received a considerable instalment of the *procès-verbaux* of the sittings of the Conference, and although those of the final meeting are not yet to hand, we think it important to give, in continuation of our article in a recent number, an account of the work done, so far as the information has reached us, the Conference having sat every day, except Sunday, from the 16th to the 25th.

The meetings, which took place in the large hall of the Paris Observatory, were attended by the following astronomers and physicists, the names being given alphabetically :—Auwers, Berlin; Baillaud, Toulouse; Bakhuyzen, Leyden; Bertrand, Paris; Beuf, La Plata; Bouquet de la Grye, Paris; Brunner, Paris; Christie, Greenwich; Cloué, Paris; Common, Ealing; Cornu, Paris; Cruls, Brazil; Donner, Helsingfors; Dunér, Lund; Eder, Vienna; Elkin, America; Faye, Paris; Fizeau, Paris; Folie, Brussels; Gautier, Geneva; Gill, Cape of Good Hope; Gyldén, Stockholm; Hasselberg, Pulkova; Henry (Brothers), Paris; Janssen, Meudon; Kapteyn, Gröningen; Knobel, London; Krueger, Kiel; Laussedat, Paris; Liard, Paris; Lœwy, Paris; Lohse, Potsdam; Mouchez, Paris; Oom, Lisbon; Oudemans, Utrecht; Pechüle, Copenhagen; Perrier, Paris; Perry, Stonyhurst; Peters, Clinton; Pujazon, Cadiz; Rayet, Bordeaux; Roberts, Liverpool; Russel, Sydney; Schönfeld, Bonn; Steinheil, Munich; Struve, Pulkova; Tacchini, Rome; Tennant, Ealing; Thiele, Copenhagen; Tisserand, Paris; Trépied, Algiers; Vogel, Potsdam; Weiss, Vienna; Winterhalter, Washington; Wolf, Paris. With a few exceptions we have in this list all the men engaged in astronomical photography.

In our former article we gave an account of the first general meeting, held on April 16. The second took place on the 19th. The names of those present were as follow :—

President, M. Struve; Vice-President, Mr. Christie; Messrs. Auwers, Baillaud, Bakhuyzen, Beuf, Bigourdan, Bouquet de la Grye, Common, Cornu, Cruls, Donner, Dunér, Eder, Elkin, Fizeau, Gautier, Gill, Gyldén, Hasselberg, Paul Henry, Prosper Henry, Janssen, Kapteyn, Knobel, Krueger, Lœwy, Lohse, Mouchez, Oom, Oudemans, Pechüle, Perry, Peters, Pujazon, Rayet, Roberts, Russell, Schönfeld, Steinheil, Tacchini, Thiele, Tisserand, Trépied, Vogel, Weiss, Winterhalter, and Wolf.

M. Struve commenced the proceedings by giving an account of the resolutions adopted by a small technical Committee, which resolutions had been prepared by M. Lœwy. They had been suggested by the desire that a great number of Observatories should participate in the work; that the price of the instruments should be moderate; and that the work should be completed in the smallest time possible compatible with thoroughness.

The following conclusions of the Committee were adopted without discussion, and agreed to unanimously :—

(1) The instruments employed shall be exclusively refractors, and may be made locally provided the conditions laid down by the Conference are fulfilled.

(2) The stars shall be photographed as far as the fourteenth magnitude inclusively; this magnitude being indicated provisionally by the scale actually in use in France, and with the reservation that the photographic value shall be definitely fixed afterwards.

The third conclusion gave rise to a considerable amount of discussion, and was finally approved as follows :—

(3) The aperture of the object-glasses shall be 0·33 metre, and the focal length about 3·43 metres, so that a minute of arc shall be represented approximately by 0·001 metre.

The division of the Congress into sections for the study of special questions was next considered, and it was determined that there should be two sections, one to deal with purely astronomical questions, and the other with those involving photography.

It was arranged that these sections should not meet at the same time, and that each section should appoint its officers at the first meeting.

We next come to the work of the Photographic Section, which held two meetings on April 20 and 21.

M. Auwers, the Vice-President of the Conference, took the chair, and proposed that M. Janssen should be appointed President. This proposition was adopted with acclamation. The bureau of this section was composed as follows :—

President, M. Janssen; Secretaries, MM. Dunér and Trépied; and MM. Auwers, Bakhuyzen, Christie, Cloué, Common, Cornu, Cruls, Eder, Fizeau, P. Gautier, Gill, Hasselberg, Paul Henry, Prosper Henry, Kapteyn, Knobel, Krueger, Laussedat, Lœwy, Mouchez, Oudemans, Pechüle, Peters, Pujazon, Roberts, Steinheil, Tacchini, Thiele, Vogel, Weiss, Winterhalter, and Wolf.

The proceedings of the section were opened by M. Cornu in a long speech which gave rise to a very lengthened discussion.

The three following resolutions were finally arrived at:—

(1) All the plates to be used should be prepared according to an identical formula to be subsequently determined.

(2) A permanent control of these plates from a point of view of their relative sensibility to the different radiations shall be instituted.

(3) The aplanatism and achromatism of the object-glasses employed shall be calculated for the wave-lengths near Fraunhofer's G.

At the second meeting the last resolution was re-discussed, and another one was passed modifying it as follows:—

"The resolution adopted at the last meeting relating to the aplanatism and achromatism of the object-glasses shall be understood in the sense that the minimum focal distance shall be that of a ray near G with a view to obtain the maximum sensibility of the photographic plates."

An important letter was read from Mr. Vogel, the Director of the Potsdam Observatory, relating to the preparation of the plates and suggesting the construction of a sensitometer.

The Committee then passed on to consider the extent of the photographic field.

With regard to the question of the distortion of images of stars away from the centre of the field, the Astronomer-Royal gave the results of calculations which he had made to determine the dimensions of the elliptic images of stars at different distances from the centre of the field, supposing that at the centre the image is reduced to a point.

Calling the axes of the ellipses *a* and *b*, the Astronomer-Royal's results are as follows:—

Angular distance from the centre of the field.	*a*.	*b*.
°	"	"
0·0	0·0	0·0
0·5	3·0	1·3
1·0	11·0	5·0
1·5	24·0	11·0
2·0	44·0	20·0

The results of these calculations agree very satisfactorily with measurements actually made on some of the Brothers Henry's star photographs.

It was suggested that, by placing the centre of the plate slightly inside the focus, one might be able by a sort of compensation to diminish the distortions of the more distant parts of the field, and thus to augment the usable extent of field.

Finally, the following resolution was passed:—

"The object-glasses shall be constructed in such a manner that the field to be measured shall extend at least 1° from the centre."

The decision was almost unanimous, three only voting against it.

Dunér then raised the question of duplicating the observations on the same or different plates.

There was a unanimous feeling that, although more than 10,000 plates would be required if four square degrees of surface were agreed upon for the field, two series of negatives must be obtained for the whole heavens; the plates being so arranged that the star at the corner of one plate shall be at the centre of another.

The work of the meeting was terminated by suggesting the appointment of two special Commissions to consider questions relating to the safe keeping and reproduction of the negatives, and also to the determination of photographic magnitudes.

We next come to the meetings of the Astronomical Section, which met on April 20, 21, and 22.

The first part of the programme of this section was the examination of the methods and instrumental details which will enable the orientation of the plates and the value of the scale to be precisely determined. The question discussed was whether the actual plates recording the stars down to the fourteenth magnitude could be used for the determination of fundamental positions. It was ultimately determined to have two series of photographs, but no resolution was arrived at at the first meeting.

At the second meeting this matter was settled by the following resolution, which was carried unanimously, with one exception:—

"Besides the negatives giving the stars down to the fourteenth magnitude, another series should be made with shorter exposures, to assure a greater precision in the micrometrical measurement of the fundamental stars, and render possible the construction of a catalogue."

At the third meeting the President remarked that, as the section had finished its deliberations, it was desirable to arrive at final resolutions on the questions referred to it.

M. Auwers proposed the following resolution:—

"The supplementary negatives destined for the construction of the catalogue shall contain all the stars down to the eleventh magnitude inclusive. The Executive Committee shall determine the steps to be taken to insure that this condition is fulfilled."

This was carried by 25 votes to 6; some members being in favour of tenth magitude only.

The second resolution, which was unanimously agreed to, ran as follows:—

"The photographic plates to be used in formation of the catalogue shall be accompanied by all the data necessary to obtain the orientation and the value of its scale; and as far as possible these data shall be written on the plate itself.

"Each plate of this kind shall show a well centred copy of a system of cross-wires to insure the determination of errors of the field, and to eliminate those which may be produced by a subsequent deformation of the photographic film.

"Further details of this nature shall be determined by the Executive Committee."

The following resolutions were next carried:—

"In the negatives destined for the map the number of cross-wires to be used in their control and reduction shall be reduced to a minimum."

"The tubes of the photographic instruments shall be constructed of the metal most likely to give an invariable focal plane, and shall carry a graduation for the determination and regulation of the position of the plate."

"The Executive Committee shall choose the reference stars to be used."

"The question of the methods of measurement and the conversion of the numbers obtained in right ascension and declination for the equinox of 1900 is left to the Executive Committee."

"That Committee shall first occupy itself with the study, and methods of use, of measuring-instruments giving either rectangular or Polar co-ordinates, and based upon the simultaneous use of scales for the larger distances and micrometer eye-pieces for scale subdivisions."

Although we are unable to give, this week, a complete account of the final doings of the Conference, what was done at the last meeting is partly known, and it is clear that the Conference has been a great success, and that much solid work has been accomplished by the forty or fifty astronomers who attended one meeting or the other.

At the final general meeting Admiral Mouchez announced that all necessary arrangements had been made with the French Government to enable the Observatories of Paris, Algiers, Bordeaux, and Toulouse, to accept at once the conditions proposed by the Conference. M. Cruls, Director of the Rio Observatory, and M. Beuf, Director of the La Plata Observatory, also accepted at once the same conditions; their instruments being already ordered, and all expenses provided for. Most of the official astronomers had not as yet obtained the necessary grants of money from their respective Governments; but among those who expressed their readiness to take a share in the work, if funds can be provided, were Struve, of Pulkova; Weiss, of Vienna; Auwers, of Berlin; Christie, of Greenwich; Pujazon, of San Fernando; Oom, of Lisbon; Gill, of the Cape; and Russell, of Sydney, who answered also for Ellery, of Melbourne. Prof. Peters, speaking for the United States, said that ten of their Observatories were anxious to join in this undertaking, but he did not know whether they would accept the conditions proposed by the Conference. Omitting, therefore, these ten doubtful Observatories, we see that already four Observatories in the North and two in the South have given in their adhesion, and these will probably soon be joined by six other leading Observatories in the northern hemisphere and by three in the southern. Henceforward we need have no time or money spent on stellar photographs which will not find their place in a well-thought-out and general scheme.

The estimated cost for each Observatory, including instruments, extra assistant, plates, measurements, &c., is about £4000.

We believe that among the resolutions arrived at at the last meeting was one recommending the erection by France of an Observatory at Réunion, and by England of one in New Zealand.

The Permanent Committee appointed consists of all the Directors of the Observatories taking part in the work, with a certain number of members not necessarily actually engaged in this work. The names of the Committee are Christie, Dunér, Gill, Prosper Henry, Janssen, Lœwy, Pickering, Struve, Tacchini, Vogel, Weiss; and, as Directors of Observatories who have decided already to join, Cruls, Beuf, Mouchez, Trépied, Baillaud, and Rayet.

The Conference nominated Janssen and Common as a Committee to consider the application of photography to other celestial bodies not included in the scheme of a photographic chart.

The hospitality of the French Government and men of science to the members of the Conference can be judged from the following list of festivities provided in their honour. In addition to the welcome of the Conference by the Minister of Foreign Affairs already mentioned, Admiral Mouchez gave a *soirée* at the Observatory on Tuesday evening, April 19. The Bureau of the Conference were presented to the President of the Republic on Wednesday, the 20th. A banquet was given by the Minister of Foreign Affairs at his official residence, on Thursday, the 21st. M. Janssen invited the Conference to his Observatory at Meudon on Friday afternoon, the 22nd. On Saturday there was a ball at the Hôtel Continental and a special performance of "Hamlet" at the Comédie Française on Sunday. A banquet was given by Admiral Mouchez at the Observatory, on Sunday, to all the members of the Conference and many of the leading French *savants*, including MM. de Lesseps, Frémy, Becquerel, Hébert, and others. On Saturday, the 23rd, the English astronomers gave a dinner to their French *confrères*; the Astronomer-Royal presided, Mrs. Christie was also present.

ON ICE AND BRINES.[1]

II.

THE second part of the paper is occupied by the study of the melting of pure ice in sea-water and other saline solutions. A large number of experiments were made with solutions of concentration comparable with that of sea-water, and in one or two cases the experiments were extended to low temperatures and strong solutions. As a rule, from 50 to 100 grammes of solution, cooled to 0° C., were mixed with an equal weight of pounded ice, also at 0° C. The thermometer used for all these determinations was one of Geissler's normal ones, divided into tenths of a degree Centigrade; and its zero-point was verified almost daily. Along with the thermometer, a pipette of suitable capacity was immersed in the beaker, and used with the thermometer for keeping the mass well mixed. Its upper aperture was closed with a small cork, which was removed from time to time to permit of some of the brine being sucked up and allowed to run back again. The inside of the pipette was thus kept constantly moistened with the slowly altering solution in the beaker. The temperature was read after very thorough mixing and the sample thereupon immediately removed and preserved for analysis.

As a rule samples were taken for analysis at intervals of 0°·4 C. The results for three classes of salt in dilute solutions are arranged in Tables IV., V., and VI.

TABLE IV.—*Giving the percentage of chlorine in solutions of various chlorides in which ice melts at given temperatures.*

Temperature of melting ice.	Chloride in solution.						
	HCl.	NaCl.	KCl.	(Sea-Water.)	$MgCl_2$.	$CaCl_2$.	$BaCl_2$.
	Per cent. chlorine in solution.						
°C.							
−3·5	3·06	3·50	—	—	4·12	—	—
−3·0	2·68	3·02	3·00	—	3·62	3·70	—
−2·5	2·28	2·53	2·50	—	3·12	3·20	—
−2·0	1·85	2·02	2·00	—	2·62	2·70	2·72
−1·5	—	1·50	1·50	1·500	1·19	2·15	2·10
−1·0	—	1·02	1·02	1·034	1·51	1·50	1·47
−0·5	—	0·50	0·52	0·588	0·87	—	—

TABLE V.—*Giving percentage of K in solutions of various potassium salts in which ice melts at given temperatures.*

Temperature of melting ice.	Salt in Solution.			
	KCl.	KI.	$K\frac{Cl + I}{2}$	KOH.
	Per cent. K in Solution.			
°C.				
−3·0	3·29	3·02	3·15	—
−2·5	2·79	2·59	2·68	2·60
−2·0	2·28	2·13	2·17	2·08
−1·5	1·74	1·63	1·66	1·57
−1·0	1·18	1·13	1·12	—
−0·5	0·59	0·60	0·57	—

[1] Paper read before the Royal Society of Edinburgh, by J. Y. Buchanan, on March 27 last. Continued from vol. xxxv. p. 611.

TABLE VI.—*Giving percentage of hydrogen in solutions of various hydrogen salts in which ice melts at given temperatures.*

Temperature of melting ice.	Salt in solution.			
	H_2SO_4.	HCl.	HNO_3.	HKO.
	Per cent. H in solution.			
° C.				
− 3·0	0·144	0·076	0·077	—
− 2·5	0·119	0·065	0·065	0·066
− 2·0	0·097	0·052	0·052	0·053
− 1·5	0·073	—	0·042	0·041
− 1·0	0·048	—	0·032	—

On considering them, it was at once evident that the lowering of the melting-point of ice followed the concentration of the solution, but the law deviated in all cases from that of strict proportionality to the amount of salt dissolved, in some cases to a greater extent than in others. In comparing the effects of different salts in solution on the melting-points of ice, no simple connexion could be traced between their absolute weights and the effects produced; but on comparing chemically equivalent weights, a very close connexion was discovered. This will be evident from the inspection of the tables. In each the first column contains the temperatures at which pure ice melts; and in the parallel columns, the percentages of chlorine, potassium, or hydrogen in the solutions of the salts indicated at the head of each column, when ice melts in them at the temperature indicated. The figures thus give numbers proportional in each table to the chemically equivalent weights of the different salts. They show at first that, whereas the presence of equal absolute weights in solution produces very different effects, the presence of chemically equivalent weights produces very similar effects. On closer inspection, it is seen that the effects are almost identical where the elements to which the common constituent is united belongs to the same group of the periodic series, and differ sharply where these elements belong to different groups. In the case of the chlorides of sodium and potassium the number expressing the percentage[1] of chlorine in the solution expresses equally the depression of the melting-point of ice in terms of the Centigrade scale. The same depression of melting temperature is produced by 10 per cent. less of chlorine united to hydrogen, and by 30 to 35 per cent. more of chlorine when united to magnesium, calcium, or barium.

The results obtained with sea-water are also given, for comparison. It will be seen that it behaves very approximately as a solution of chloride of sodium containing the same amount of chlorine.

It is perhaps not very astonishing that unit weight of potassium in saline solution should produce the same effect in lowering the melting-point of ice, whether it is united to Cl, to I, or to OH; but it shows clearly how independent this action is of the character of the body in solution when we find the effect produced by unit weight of hydrogen identical whether it is united to such opposite radicles as Cl or OK. Table VI. shows further the effect of valence. While a given weight of hydrogen produces the same effect in solution whether it be united to the very different but both univalent radicles Cl and OK, its effect is reduced by one-half when united to the bivalent SO_4. That valence is not the only factor is shown by comparing the effects of hydrogen and potassium when united to the common element, chlorine. Hydrochloric acid in solution produces a markedly more powerful lowering effect on the melting-point of ice than the equivalent amount of chloride of potassium. Of all the substances that I have experimented on, hydrochloric acid is the most energetic in reducing the melting-point of ice, and with ordinary strong acid and pounded ice there is no difficulty in producing temperatures as low as the freezing-point of mercury. In the case of hydrochloric acid, sulphuric acid, chloride of sodium, and chloride of calcium, I have carried my experiments to low temperatures and great concentration. But before passing to them it is well to consider the more dilute solutions with regard to their density.

[1] All percentages are *by weight.*

That the mere density of the solution in which the ice is melting has no direct connexion with the lowering of its melting-point is shown by the following table, in which the specific gravities (at 15° C.) are given of the solutions of different salts which give the same depression of melting-point.

Temperature of melting.	Specific gravity of solutions of				
	NaCl.	KCl.	$MgCl_2$.	$CaCl_2$.	$BaCl_2$.
° C.					
− 2·86	1·03370	1·03850	1·03893	1·04756	—
− 1·8	1·02174	1·02535	1·02715	1·03262	1·06633

There are many similarities in the effects produced by greatly increasing the pressure upon pure water and by dissolving salts in it. First, there is an absolute diminution in the volume of the solution as compared with the sum of the volumes of its components; second, in virtue of this compression by molecular forces it has become less compressible by mechanical means; third, the temperature of maximum density and the freezing temperature are lowered; and fourth, the former of these two temperatures is lowered more rapidly than the latter. All these effects are produced *in kind* by increasing the pressure on pure water. Whether, or in how far, they agree in degree must be decided by future experiments.

Experiments with Concentrated Solutions.—Several series of experiments have been made with hydrochloric acid, chloride of sodium, and chloride of calcium, and also with sulphuric acid. Table VII. gives the results, in the same form as preceding tables, for the chlorides:—

TABLE VII.

Temperature of melting ice.	Salt dissolved.		
	HCl.	NaCl.	$CaCl_2$.
	Per cent. Cl in solution.		
° C.			
− 35	15·26		
− 30	13·98		15·97
− 25	12·60		14·47
− 20	11·00		12·65
− 15	9·17	11·10	11·29
− 10	7·02	8·40	8·93
− 5	4·15	4·72	5·65

It will be seen that, in proportion as the solution becomes more concentrated, further additions of salt produce a greater effect in lowering the melting-point of ice, and at a temperature of −15° C. equivalent weights of NaCl + $CaCl_2$ produce identical results. In Table VIII. the results for hydrochloric acid and sulphuric acid are given in terms of the percentage of hydrogen in the solution.

TABLE VIII.

Temperature of melting ice.	Acid dissolved.	
	HCl.	H_2SO_4.
	Per cent. H in solution.	
°C.		
− 25	0·355	0·538
− 20	0·310	0·487
− 15	0·258	0·418
− 10	0·198	0·332
− 5	0·117	0·205

The temperatures given in these tables are all in terms of the same thermometer, which has not been verified for this part of its scale by comparison with a standard or with the air thermometer.

It is exceedingly difficult, as a rule, to ascertain the trustworthiness of a thermometer at low temperatures. This difficulty would be removed if the temperature at which ice melts in solutions of some very soluble salts of different concentrations were carefully and accurately determined with a good air thermometer. If, for instance, this were done for chloride of calcium solution, which in many ways would be a particularly convenient one, there would be no difficulty in verifying a thermometer at any moment at temperatures as low as −30° C. by mixing pounded ice with the strong solution, immersing the thermometer in it, taking a series of readings of the instrument, while a series of samples of the liquid is taken and in them the chlorine determined. There are considerable advantages in this method of verification of thermometers by chemical means, especially as it obviates the use of the air thermometer, which is always inconvenient. The experiments of Pfaundler and Schnegg on the freezing of aqueous solutions of sulphuric acid can be used for this purpose. But it would be better to have a series of observations made for the purpose with the more convenient chloride of calcium solution.

Freezing Mixtures.—The results obtained in examining the melting-point of ice in saline solutions affords data for mixing freezing baths of any degree of cooling power. With chloride of sodium, for instance, a rough rule is to have such an amount of salt dissolved in the brine that the percentage of chlorine shall give the desired temperature in Centigrade degrees below the freezing-point. In my experiments in freezing sea-water in quantities of 300 grammes, I usually made up the bath of 500 grammes pounded ice, 400 grammes water, and 45 grammes common salt. When mixed, the liquid contained about 4 per cent. Cl, and gave a temperature a little below −4° C. In the course of an hour the liquid would contain 3 per cent. to 3·25 per cent. Cl, and the temperature have risen to −3° C. By using such baths freezing operations can always be kept completely in hand.

Summary.—Owing to its peculiar physical properties it is impossible to prepare the crystalline solid which separates from sea-water and analogous saline solutions in a condition to enable the question, whether the salt does or does not form part of the solid matter of the crystals, to be solved directly by chemical analysis.

So far as chemical analysis is applicable, it is in favour of the salt belonging exclusively to the adhering brine. When sea-water is carefully frozen artificially, the ratio between the chlorine and the sulphuric acid is the same for the solid contents of the original water, the crystals, and the mother-liquor. It is exceedingly unlikely if part of the salt went into the crystals, leaving the remainder in the brine, that there would be no selective separation of its constituents.

It has been shown (and the whole of the second part of the paper is taken up with this subject) that snow or pure lake ice, which, when melting by itself or immersed in pure water at atmospheric pressure, melts at the constant temperature called 0° C. or 32° Fahr., changes its melting temperature when immersed in a saline solution. The altered melting temperature, however, is the same for solutions of the same composition (no doubt with some allowance for pressure) and different for solutions of different composition.

The temperature at which pure ice melts in a solution is identical with that at which ice separates from the same solution on being sufficiently cooled.

When sea-water is frozen to the extent of 15 per cent. of its mass, and the crystals so formed are allowed to melt in the liquid in which they have been produced, they melt exactly as they have been formed. If snow or pure ice be immersed in the brine formed by partially freezing sea-water, it melts at the same temperature as the ice which had been formed by freezing the sea-water, so long as the chemical composition is the same in each case.

When saline solutions are cooled for a sufficient length of time at a sufficiently low temperature, there arrives a certain concentration at a certain temperature, when further removal of heat causes solidification of the brine as a whole (cryohydrate).

The concentration necessary for the solidification of even the cryohydrate of highest melting temperature is such that in the *primary* freezing of sea-water in nature no such body can be formed. It would follow from this consideration alone that the first ice formed on the sea in Arctic regions consists of pure ice, and it is also certain that it would retain a large quantity of the residual sea-water in its interstices. During the winter this inclosed liquor would solidify in the interstices of the crystals to ice and cryohydrates, in so far as the temperature and the nature of the salts in solution would permit. From my experiments with chloride of calcium, and the existance of brines observed to remain liquid at −30° C. at the winter-quarters of the *Vega*, it is unlikely that sea-water, as a whole, can ever be completely solidified in nature. The presence of unfreezable or difficultly freezable brine in freshly-formed sea-water ice explains its eminently plastic character even at very low temperatures. The presence of similar unfrozen brine in natural land ice at temperatures neighbouring to 0° C. explains its slightly plastic character, which is sufficient to account for the slow fluid motion of glaciers under the long-continued pressure of their own weight.

The fact that cryohydrates of different salts solidify and melt at different temperatures, sufficiently explains the various composition of different specimens of *old* sea ice.

The physical phenomena observed in freezing sea-water and saline solutions of moderate concentration, are all easily and simply explained on the hypothesis that the crystalline body formed is pure ice. Thus, the heat removed in freezing sea-water to the extent of 15 per cent. of its mass accounted for the production of the same amount of ice as was given by calculation on the basis of the chlorine found in the mother-liquor.

The apparent expansion, near the melting-point, of ice formed by the freezing water which contains any salt at all is perfectly explained on the hypothesis that in the act of freezing the water rigidly excludes all saline matter from participation in its solidification.

The same applies to the latent heat of water containing salt in matter. Pettersson made two determinations of the latent heat of sea-water containing 1·927 per cent. Cl and 3·53 per cent. salt. The freezing took place in the one case between the temperatures −9°·0 and −7°·47 C., and in the other between −8°·35 and −6°·94 C., and the results he found were 52·7 and 51·5. The mean initial temperature in these two experiments is − 8°·7 C., and the mean final temperature − 7°·2 C. At − 7°·2 C., ice would form on cooling, and would melt on warming a solution of chloride of sodium containing 6·48 per cent. Cl, which represents 11·87 per cent. of the sea salt. In order to concentrate a brine containg 3·53 per cent. salt to one containing 11·87 per cent., 70 per cent. of the water in it must be removed. Hence in sea-water freezing at a final temperature of − 7°·2 C., there is formed 70 per cent. of ice, and there remains liquid 30 per cent. of brine. Freezing began at the mean temperature − 8°·7 C., and the latent heat of pure ice at this temperature is 75. Calculating the latent heat of this mixture from the heat liberated in the calorimeter during freezing, and assuming that the whole mass had solidified, Pettersson's results give the mean latent heat of this sea-water as 52·1. Calculating the apparent latent heat on the assumption that 70 per cent. of the mass solidifies into pure ice and that 30 per cent. remains liquid, we get the number 51·5. On all grounds therefore we must conclude that pure ice is the primary product in freezing sea-water and saline solutions of moderate concentration.

The fact that ice melts in sea-water at a temperature of − 1°·6 C. to − 1°·8 C., according to its saltness, explains the anomalous distribution of temperature in Antarctic waters, and furnishes an account of the motive power which draws the surface water of cold temperate regions into the deeper layers, and after dilution with the melted matter of the icebergs, to the surface layers of Antarctic latitudes. Forming as it does an important factor of oceanic circulation, this part of the subject was treated in a separate paper, of which an account has already been given in NATURE (vol. xxxv. p. 516).

THE CLASSIFICATION OF SPIDERS.[1]

ALTHOUGH Dr. Thorell's paper is nominally only a criticism of Dr. Bertkau's views, it is really a masterly sketch of the literature on the subject of the classification of spiders, and a review of the methods of various authors from Lister downwards. The two leading views at present held on this subject appear to be represented by the author (Dr. Thorell) and Dr. Bertkau. These two views may be stated generally as *anatomical* v. *biological*, the former being the basis of Dr. Bertkau's classification, the latter (combined with considerations of external structure) that of Dr. Thorell. Dr. Thorell successfully, as it appears to us, defends the classification provisionally adopted in 1869, in his work "On European Spiders," from the sweeping charge that it is neither natural nor equal, nor based on characters of sufficient importance and distinctly expressed, though at the same time he freely admits its inevitable imperfection. He shows that in no branch of Nature are the subordinate groups of exactly equal value, nor should it be expected that the same equal systematic value would be found in the subordinate groups of the class Arachnida. It is well to remember that zoologists have to form their groups out of such materials as have come to their hands ready provided for them by Nature. They cannot expect to advance natural science by constructing out of limited materials a perfectly symmetrical system, and then insisting that all the diverse forms of Nature shall, *nolens volens*, be stuffed somewhere or other into it. Equality, therefore, of systematic value in the various groups into which spiders (Araneidea) may be divided can at best be only approximate; and it seems evident that as our knowledge of structure, whether external or internal, or of habits as dependent on and arising out of structure, becomes more extensive and exact, so some further modifications may become necessary in the primary divisions of spiders. After subjecting Dr. Bertkau's classification (which is based principally on the breathing-organs) to a minute and destructive criticism, Dr. Thorell modifies his former views by reverting, in some measure, to the Latreillian division of spiders into (1) those possessing four air-sacs, *Tetrapneumones*; (2) those with two, *Dipneumones*; still, however, retaining the old Latreillian biological divisions, *Territelariæ*, *Tubitelariæ*, *Orbitelariæ*, &c., based on habits, because the groups so divided may yet be thoroughly and scientifically differentiated by important and trustworthy structural characters. These divisions (now called by Dr. Thorell "tribus") are, as is well known, seven in number, and each is subdivided into families, which, with few exceptions, include only European species, Dr. Thorell's opportunities for the study of exotic groups not enabling him to construct a more complete subdivision of all known spiders. Dr. Bertkau's primary division of the Araneidea is into two groups, called suborders—*Tetrasticta*, with four breathing-apertures, and *Tristicta*, with three. Dr. Thorell shows conclusively that in the present state of our knowledge of the respiratory system of spiders (though this is far advanced beyond what it was in Latreille's days, and in a great degree the advancement is admitted to be due to Dr. Bertkau's labours) these two suborders are artificial rather than natural; as are also his subdivisions of the *Tristicta*, which are based on the undoubtedly remarkable characters to which such great prominence was given by the late Mr. Blackwall, that is, the possession or absence of a *cribellum* and *calamistrum*, the use of these in primary subdivision bringing together spiders not closely allied by any other natural characters. Dr. Thorell attributes a certain amount of classificatory importance to these organs by his intercalation of the families of his largest "tribus" possessing them, in a kind of osculant or collateral way, and of the other "tribus" in which they are found, in a linear arrangement, guided, however, in both cases by their possessing such other characters as, in all instances, fully warrant the position assigned to them. The anatomical study of the *tracheæ* (properly so called) of spiders seems to be yet in its infancy. Certainly at present these organs of respiration do not appear to warrant the importance attributed to them by Dr. Bertkau; and although Dr. Thorell's primary subdivisions are, in their *names*, strictly speaking, based on only biological characters, yet in reality they severally enshroud the most important structural ones, and hold all known spiders in a fairly natural system. They are, moreover, well known, and appear likely to be adopted for some time yet to come, with more or less modification, by the majority of araneologists.

O. P. CAMBRIDGE.

[1] "On Dr. Bertkau's Classification of the Order Araneæ, or Spiders," by Dr. T. Thorell (*Ann. and Mag. N.H.*, April 1886, pp. 301–26). (See especially the following works by Dr. Bertkau:—"Versuch einer natürlichen Anordnung der Spinnen," *Archiv. für Naturgeschichte*, xliv. 1, pp. 351 *et seq.*, 1878; and "Ueber das Cribellum und Calamistrum; ein Beitrag zur Histologie, Biologie, und Systematik der Spinnen," *ibid.*, xlviii. 1, pp. 316 *et seq.*, 1882.)

CHRISTMAS ISLAND.

THE following account of the little-known Christmas Island, situated in the Indian Ocean, south of Java, may be of interest. Capt. Maclear and his officers collected a variety of specimens, which have been forwarded to the Museum of Natural History and to the Royal Gardens, Kew, but they do not seem to have succeeded in making their way through the dense jungle to the upper part of the island, to ascertain the geological character of the mountain originally protruded from the depths. It is a little remarkable that, in a sea so calculated to encourage coral growth, no new reefs should have formed round the island since the ancient ones were elevated above the surface The Cocos or Keeling

Islands, 500 miles to the westward, are a well-known example of thriving coral life.

W. J. L. WHARTON.

H.M. Surveying-Vessel "Flying-Fish," January 31, 1887.

Christmas Island is 190 miles from the nearest point of Java, from which it is separated by a depth of 2450 fathoms. It is formed of coral limestone, has no fringing reef, but rises abruptly from the sea in cliffs about 30 feet high, very much underworn, and in many places hollowed out in caverns; the shore is steep-to: generally a depth of 100 fathoms is found at one to two cables from the cliffs.

In appearance it is somewhat saddle-shaped, rising from a long back in the middle, 700 to 800 feet high, to hills at the north-eastern and at the western sides; the western summit is double, and is the best-defined mark: its height is 1580 feet. The shape is irregular quadrilateral; it extends through 12′ of latitude, and about the same in longitude.

The island is densely wooded all over, except where the cliffs are too steep to allow anything to grow. From the northern side the ascent is gradual to the highest parts; but on the southern side, after rising gradually for half a mile from the sea cliffs, a second wall of limestone cliffs is met, estimated at 200 to 300 feet high, and thence slopes gently again to the top.

The shore cliffs are almost continuous, making the island inaccessible except at a few places. These cliffs are split by deep fissures extending several feet below water; where these have become enlarged, and the adjacent cliffs have fallen in, a small white beach of fragmentary rock is thrown up, and at such places on the lee-side landing can be effected.

From the blown direction of the trees on the south side, and from the weather-worn aspect of rocks exposed to the southward, it is manifest that the south-eastern is by far the prevailing wind.

The north side of the island forms a large bight, in which the water is quite smooth, so that a boat can go close up to the cliffs, but on the southern and eastern sides a heavy sea dashes against the rocks.

The *Flying-Fish* steamed close round the island looking for anchorage, but found none, except in a small cove two miles to the westward of the north point of the island—this has been named Flying-Fish Cove; here she anchored in 22 fathoms, with her stern secured by hawsers to the trees, to prevent slipping off the bank.

The hill rises nearly perpendicularly at the head of the cove in the form of a horseshoe, and slopes gradually down to the two arms forming the cove. The bare beach is not more than 20 yards wide, and, from the look of the fragments that compose it, must be thrown up in northerly gales; the upper part of the beach to the foot of the hill, a distance of some hundred yards, is of just the same material, viz. fragments of coral rock and coral limestone, but it has a covering of mould from fallen leaves, and is thickly wooded, many of the trees on it being forest trees of 12 feet girth and of great height, apparently hundreds of years of age, showing that a very long time must have elapsed since that beach was raised from the water.

One very large tree had something like the letters **W W** cut inside a scroll, and nearly illegible from time; this was the only sign of the island having been visited before; but one of our officers heard at Batavia that a Dutch vessel was wrecked on the south-east point of the island in a calm about fifteen years ago, and that the crew escaped and lived many months on the island before they were taken off, but I have no other details about the affair.

No running water was seen, but the droppings from the leaves during rain and dew must be great, as holes in the rocks and cup-shaped leaves were filled with water. As it was raining over some part of the island (generally the western) great part of the time the *Flying-Fish* was in the neighbourhood, and clouds were continually being formed over the island from the moist air driven up the side by the south-east wind, a great deal of water must be deposited, and probably be absorbed by the soil. At the eastern end of the cove, among the trees, where had seemed at first the most likely place for a watercourse, a few volcanic stones were found, but everywhere else the only rock seen was coral limestone; the cliffs above from which detached pieces had fallen to the beach were the same; the soil under the trees was a rich moist mould, apparently formed from decaying vegetation.

Landing was also effected at another small beach in the northern bight near the north-west point; the general features were the same, but there was no anchorage at half a cable from the shore. A few cocks and hens were landed here, but as the crabs immediately began to chase them, I doubt if they will survive and produce.

No large animals were seen, nor marks of any. An iguana, said to be 4 feet in length, was seen in a tree, high up, but was not captured. Rat-holes were numerous, and one rat was secured, also a large bat. Several insects, spiders, flies, beetles, and butterflies, were collected; there were sand-flies, but no mosquitoes. Large crabs were very plentiful, and appeared equally at home running over the sea-cliffs and climbing up the trees; they were very ravenous, pouncing quickly on a dead gannet and devouring other injured crabs, and they must be terrible enemies to the birds generally.

Gannet and frigate-birds frequent the island, and evidently breed there, but it was not the breeding-season, and very few eggs were found; the young birds were nearly grown. Besides the sea-birds there was the large green Torres Strait pigeon: one was shot, with three large red berries in his crop. These pigeons seemed to frequent the higher trees well up the hill. Also a ground-thrush, of a sooty-brown colour, just the colour of the fallen leaves among which it ran nimbly, apparently looking for insects; and a little fly-catcher of the same sombre colour. As evening advanced, a small swift appeared, which flew about the jungle on the margin of the beach, fly-catching: none of these three last were secured. No bones were found on the beach, nor remnant of any animal; not even turtle-remains.

The flora appeared to be the same as that of the neighbouring islands, the Moluccas. As before stated, the island is densely wooded, and many of the trees attain great size. Chief amongst them I recognized two iron-wood trees, one with straight stem and round trunk, and the other with strong buttresses from the roots; both are natives of Celebes. Creepers were as thick as in the Moluccas, and covered the top branches of the trees.

Two palms—one I take to be the sago-palm, growing to a great height; and the pandanus—were abundant: cocoanut trees were not seen, though husks were found on the beach, apparently washed up from elsewhere. At a small beach on the eastern side there appeared to be banana-trees, but they looked withered and there were no signs of fruit.

No mangroves were seen: the flora of the coast was generally such as is found in all tropical islands.

I regret to say that nearly all the botanical specimens that were collected were destroyed by insufficient drying in the exceedingly damp weather we experienced.

(Signed) J. P. MACLEAR, Captain.

NOTES.

ON March 9, on the invitation of the Chief Justice of Queensland, a public meeting was held at Brisbane, to consider the establishment of a University for that colony. A resolution was passed inviting ministers of religion, the various professions, and every representative body to petition Parliament to establish a University for Queensland in perpetual commemoration of the Jubilee year of the Queen's reign. A Committee was appointed to prepare a petition and make arrangements for united action.

ACCORDING to the *Calcutta Englishman*, the Indian Government has arranged a scheme for the complete and systematic botanical survey of India. The country will be divided into four great districts, the first under Mr. Duthie, Superintendent of the Government Botanical Gardens at Saharunpur; the second under Surgeon-Major King, Superintendent of the Royal Botanical Gardens at Calcutta; and the third and fourth under the Madras and Bombay Government Botanists respectively.

THE rich flora of the Philippine Islands has hitherto been most imperfectly known. In fact, it has been practically only represented in European herbaria by the collections of Cuming, which, though rich, were made in a limited area. It was only, therefore, to be expected that the explorations made by Dr. Sebastian Vidal, of Soler, Director of the Botanic Garden at Manilla, and of the Commission for studying the forest flora, would add to our knowledge a profusion of new and interesting species. Dr. Vidal has on two occasions visited Kew with his collections, which have quite realised the expectations that had been formed of them. There was some reason to fear that the work might, on financial grounds, have to be interrupted. But from a communication made to Kew by the Spanish Minister, we are glad to learn "that although the Botanical Survey Commission intrusted to Dr. Sebastian Vidal had been at one time suppressed in the Budget of 1887–88, it was afterwards re-established in view of the great importance of the work."

THE thirty-sixth meeting of the American Association for the Advancement of Science will be held in New York during the week beginning August 10, 1887. The New York Academy of Sciences has appointed a Committee to secure concert of action among those who are anxious that adequate preparations may be made for the meeting.

IN his speech at the Royal Academy banquet, Prof. Huxley offered some suggestive and interesting remarks on the relations between science on the one hand and art and literature on the other. "I imagine, he said, "that it is the business of the artist and of the man of letters to reproduce and fix forms of imagination to which the mind will afterwards recur with pleasure; so, based upon the same great principle by the same instinct, if I may so call it, it is the business of the man of science to symbolize, and fix, and represent to our mind in some easily recallable shape, the order, and the symmetry, and the beauty that prevail throughout Nature. I am not sure that any of us can go much further from the one to the other. We speak in symbols. The artist places his colours upon the wall; the colours have no relation to the actual objects, but they serve their purpose in recalling the emotions which were present when the scenes which they depict were acted. I am not at all sure that the conceptions of science have much more correspondence with reality than the colours of the artist have; but they are the symbols by which we are constantly recalling the order and beauty of Nature, and by which we by degrees force our way further and further into her penetralia, acquiring a greater insight into the mystery and wonder which are around us, and at the same time, by a happy chance, contributing to the happiness and prosperity of mankind." Referring to the fact that in these days scientific men are in danger of becoming specialists, occupied with a comparatively small field, Prof. Huxley maintained that the remedy lies in the recognition of "the great truth that art and literature and science are one, and that the foundation of every sound education and preparation for active life in which a special education is necessary should be some efficient training in all three." He concluded as follows:—"I sincerely trust, Sir, that, pondering upon these matters, understanding that which you so freely recognise here, that the three branches of art and science and literature are essential to the making of a man, to the development of something better than the mere specialist in any one of these departments—I sincerely trust that that spirit may in course of time permeate the mass of the people, that we may at length have for our young people an education which will train them in all three branches, which will enable them to understand the beauties of art, to comprehend the literature at any rate of their own country, and to take such interest not in the mere acquisition of science, but in the methods of inductive logic and scientific inquiry as will make them equally fit, whatever specialised pursuit they may afterwards take up. I see great changes; I see science acquiring a position which it was almost hopeless to think she could acquire. I am perfectly easy as to the future fate of scientific knowledge and scientific training; what I do fear is, that it may be possible that we should neglect those other sides of the human mind, and that the tendency to inroads which is already marked may become increased by the lack of the general training of early youth to which I have referred."

THE first edition of "Scenery of Scotland viewed in Connexion with its Physical Geology," by Mr. Archibald Geikie, was published twenty years ago. It was one of the first books in which the origin of the scenery of a country was traced out in some detail with reference to geological structure. Since the appearance of the work, the author has extended his experience by journeys all over Europe and through the western territories of America, and he is engaged, we understand, upon a general treatise on the origin of the surface features of the land. In the meantime, in response to repeated requests, he has prepared a new edition of his first work on the subject—the "Scenery of Scotland." The book has been thoroughly revised and in great part re-written. The illustrations are almost all new. It is expected that the volume will be ready in time for the visitors who crowd into Scotland in the summer and autumn.

MESSRS. KEGAN PAUL, TRENCH, AND CO., will publish immediately "Three Lectures on the Anatomy of Movement: a Treatise on the Action of Nerve Centres and Modes of Growth," delivered at the Royal College of Surgeons by Dr. Francis Warner, Hunterian Professor of Comparative Anatomy and Physiology.

REGULARLY during twenty-five years, on the first of each quarterly month, Mr. Van Voorst published a part of Hewitson's "Exotic Butterflies," containing coloured figures of new species. The work was completed a few years ago. Since that time, material for its continuation has accumulated in the collection of Mr. Henley Grose Smith, who will now, with the assistance of Mr. W. F. Kirby, bring out another series under the title of "Rhopalocera Exotica." Part I will be published by Mr. Van Voorst's successors, Messrs. Gurney and Jackson, in July.

In continuation of Hooker and Baker's "Synopsis Filicum," a hand-book of the other orders of Vascular Cryptogamia, by Mr. J. G. Baker, will be published shortly by Messrs. G. Bell and Son. It will include Equisetaceæ, Fycopodiaceæ, Selaginellaceæ, and Rhizocarpeæ, in which, excluding the fossil types, there are eleven genera and about 700 species.

MR. ARTHUR DENDY, B.Sc. of the Victoria University, and Associate in Science of Owens College, has been appointed by the Trustees of the British Museum an Assistant in the Department of Zoology in the vacancy occasioned by the resignation of Mr. Stewart O. Ridley, whose work in connexion with the sponges and corals Mr. Dendy will continue. The vacancy in the Botanical Department occasioned by Mr. Fawcett's appointment to the Curatorship of the Botanical Gardens in Jamaica has been gained, after competitive examination, by Mr. Edmund Gilbert Baker, a son of the well-known botanist of Kew Gardens.

THE Council of the British Medical Association have recently appointed Mr. Watson Cheyne and Dr. Sidney Martin as Science Scholars for one year. The former proposes to continue his research of Bacteria in relation to disease, and the latter to carry on researches on the vegetable albuminoses, especially with relation to their alleged toxic action.

ON Monday last, Mr. J. M. Thomson delivered the first of the concluding course of Cantor Lectures at the Society of Arts. The remaining lectures of the course will be delivered on May 9, 16, and 23. The subject is the "Chemistry of Substances taking Part in Putrefaction and Antisepsis."

ANOTHER synthesis of a natural product has just been added to the long score of successes which have followed Wöhler's initiative. About a dozen years ago it was observed that the outer coatings of walnuts collected at the end of June became covered with small yellow needle-shaped crystals, of a substance which was found by Vogel and Reischauer in the expressed juice of the same, and named by them nucine or juglon. Bernthsen and Semper have very recently (*Ber. Deut. Chem. Ges.* 1887, No. 6) proved conclusively that this substance is an α-hydroxy-α-naphthoquinone, $C_{10}H_5O_2(OH)$, and to complete the proof have actually built up the same substance directly from naphthalene. They first prepared α_1 α_3 dihydroxynaphthalene, $C_{10}H_6(OH)_2$, by Armstrong's method, which was then oxidized by chromic acid; the brown precipitate obtained was afterwards digested with warm ether, and after removal of the ether by distillation, crystallization from chloroform yielded beautiful acicular crystals identical in all respects with juglon, of nutshell odour, producing violent sneezing. As naphthalene itself can be built up from its elements, it follows that juglon, undoubtedly a product of vegetable growth, has been synthetized by artificial means.

SOME important observations on the structure and origin of the gelatinous sheath which invests the filaments of many Algæ, and also some Flagellata, have recently been published by Herr G. Klebs. In the Zygnemaceæ this sheath is composed of a substance entirely independent of the cell-walls. It consists of two portions: a homogeneous substance which is but slightly refringent, and which is indifferent to the action of staining reagents; and a portion which absorbs pigments with avidity, and which is composed of minute rods at right angles to the cell-wall. This substance does not exhibit the reactions of the ordinary mucilage of vegetable cells; it is not dissolved by alkalies. The author maintains that the substance of the sheath is derived directly from the cytoplasm of the cell through the cell-wall; it is always quite distinct from the cell-wall, and must be formed by apposition rather than by intussusception. Similar results were obtained from the gelatinous sheath of the Desmidieæ and of some other Algæ. A gelatinous sheath can be detected in nearly all the Flagellata by the use of sufficiently dilute staining materials; and here, also, the sheath is due directly to the activity of the protoplasm. In *Euglena sanguinea* it is secreted in the form of more or less curved filamentous bodies. In the social forms the gelatine consists of a fundamental substance, immersed in which are denser granular corpuscles. The brown or black colour is due to the deposition of oxide of iron.

THE preparations for the making of a canal between the Baltic and the German Ocean are so far advanced that the construction of the earthworks will be begun on June 18.

ON March 17 we stated in a note that the Berlin Academy of Sciences had granted a sum of money "for the printing of some important zoological works," among which we mentioned Dr. Taschenberg's "Bibliothek." Herr Engelmann, the publisher of the "Bibliothek," writes to us that the grant was made to Dr. Taschenberg personally, in recognition of his labours as editor, and that it does not in the slightest degree diminish the publisher's responsibilities in connexion with the work.

THE Council of the Parkes Museum believe that there are many medical men who would be glad to make use of the Museum under the guidance of someone able to point out the object and advantages of the various appliances exhibited. They have therefore arranged, for the month of May, three demonstrations, which will be open to all members of the medical profession on presentation of their cards. Prof. W. H. Corfield has consented to give a demonstration on Monday, May 9; Mr. Rogers Field on Monday, May 16; and Mr. Percival Gordon Smith on Monday, May 23. The demonstrations will begin at 5 p.m.

SIX years ago a seaside laboratory for the study of biology was started at Annisquam, near Cape Ann, by the American Woman's Education Association. The Society, which does not give permanent support to any of its enterprises, has always been anxious that this institution should be placed on a secure basis; and accordingly a circular letter was lately sent to teachers of science in different parts of the United States, giving an account of the work done, and asking for opinions as to the need of such an establishment. The answers were so satisfactory that a number of naturalists met to consider the question; and this meeting appointed a Committee with full power to establish a new and greatly improved laboratory. An appeal for 15,000 dollars has now been issued, and if the response is liberal, the laboratory may be opened in the summer of the present year.

THE Italian Meteorological Society reports that its observer at Patagones (lat. 40° 49′ S., long. 62° 45′ W.), while taking observations at 2 a.m. on December 1 last—observations being then taken every two hours—was surprised by a continuous shower of innumerable shooting-stars proceeding from all visible parts of the sky. They were of varying brilliancy, the majority appearing to be of the brightness of stars of the second and third magnitude. He was unable to take an exact observation, for want of necessary materials; but during the fifteen or twenty minutes that he stood observing them, the stupendous display constantly maintained the same intensity.

M. L. TEISSERENC DE BORT has published in *Ciel et Terre* a summary of his charts showing the mean amount of cloud over the surface of the globe, presented to the Académie des Sciences, Paris, on February 7. The paper is of interest from the fact that up to the present time the amount of cloud has not been treated in the same general way as the other meteorological elements, except for limited areas. The charts in question are based on observations made at 700 stations, and on an immense number of observations collated by the Meteorological Office in Paris. The following are the principal conclusions arrived at: (1) there is a marked tendency in all months towards a distribution of cloud in zones parallel to the equator; (2) when disturbing influences are eliminated it is seen that there is a maximum amount of cloud near the equator, that there are two belts of slight nebulosity from 15° to 35° of north and south latitude, and two zones of greater cloudiness between latitudes 45° and 60°, and that beyond this (so far as can be judged from observations in the northern hemisphere) the sky appears to become clear towards the Poles; (3) these zones have a marked tendency to follow the march of the sun's declination; they are transferred towards the north in spring, and towards the south in autumn; (4) if the charts of nebulosity are compared with isobaric and wind charts it will be seen that the zones of clear sky correspond to the regions of high pressure which lie on each side of the equator, and which give rise on the one hand to the trade winds, and, on the other, to the westerly winds which prevail towards the temperate regions of the two hemispheres. The zones of greater cloudiness extend above the regions of low pressures, viz. near to the equator, on the one hand, and near to 60° of north and south latitude. The distribution of cloud, taken as a whole, appears therefore to be a direct result of the march of the winds, and is regulated by the distribution of the atmospheric pressures.

THE new Industrial Institute at Bromley, Kent, was opened on Saturday last by Sir Lyon Playfair. Hitherto, he said, the country had prided itself upon the practical knowledge of its artisans, but it had relied too entirely upon that knowledge, and the consequence had been that countries which nurtured the intellects of their people had stepped in, and with their superior mental education had shown the world that the competition of the day was not one of local advantages, such as the possession of raw material applicable to industries, but a competition of intellect. England was realizing her position now, and training her sons by technical schools to compete intellectually with the countries round her, from whom she had learnt her lesson.

Science lately invited certain eminent American authorities on education to discuss in its columns the question, "What industry, if any, can profitably be introduced into country schools?" Mr. Samuel G. Love writes enthusiastically on the effects which may be produced on children by industrial or manual training. It "opens a way," he thinks, "to interest them, to develop and employ their perceptive faculties, and to make the otherwise unattractive experiences of school life cheerful and pleasant." As for the particular kinds of industry that may be most advantageously introduced, he contents himself with the general statement that "there are many things that can be done with profit in any and all schools; and, as soon as the pupil enters upon school life, one of them should be taken up, and each carried forward one after the other, just as the subjects of study are taken up and completed." Mr. Francis A. Walker is more precise. He proposes that approved methods of the Kindergarten should be carried upwards through the primary grades; that at the age of twelve, or thereabouts, there should be semi-weekly exercises with tools, preferably wood-working tools, and in clay-modelling; and that at the age of about fourteen, exercises in metal-working should be begun. Mr. Charles H. Ham takes a wholly different view. He objects to the introduction of "industrial features" into courses of popular education in rural districts, partly because industrial training is very costly, but chiefly because children in the country learn so many things in their ordinary work and play that they do not seem to him to need any special industrial training at school.

THE additions to the Zoological Society's Gardens during the past week include two Green-winged Doves (*Chalcophaps indica*) from Penang, presented by Mr. S. A. Clarke; two Alpine Newts (*Molge alpestris*) from Algiers, presented by Mr. Alban Doran; twenty Ruffe or Pope (*Acerina cernua*) from British fresh waters, presented by Mr. T. E. Gunn; a Whinchat (*Pratincola rubetra*), British, two White-faced Tree-Ducks (*Dendrocygna viduata*) from Brazil, purchased; two White-necked Storks (*Dissura episcopus*) from West Africa, two Demoiselle Cranes (*Grus virgo*) from North Africa, received in exchange; a Gayal (*Bibos frontalis* ♂), a Persian Gazelle (*Gazella subgutterosa* ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE ORBIT OF THE MINOR PLANET EUCHARIS.—Dr. L. de Ball has published, in *Mémoires de l'Académie Royale de Belgique*, tome xlix., an investigation of the orbit of *Eucharis* (No. 181), deduced from all the available observations made during the years 1878 (the year of its discovery) to 1886 inclusive. The discussion of the orbit of this minor planet is of considerable interest, as in part of its path it approaches Jupiter, and its consequent perturbations will afford material for a determination of the mass of that planet. To attempt such a determination at present would be premature, but a necessary preliminary to it is the determination of a sufficiently accurate orbit for the perturbed planet, and this is furnished by Dr. de Ball in the paper before us. The perturbations due to Jupiter and Saturn, using Bessel's values of the masses, have been taken into account, and great pains have been taken to reduce the places of the comparison-stars used to a uniform system—that of Auwers's Fundamental Catalogue. The great mass of observations discussed in this paper are equatorial observations; a considerable number of meridian observations made with the Washington transit-circle in 1878 are, however, also discussed. These do not harmonize very well with the equatorial observations, and Dr. de Ball is led to the conclusion (for which he is unable to account) that the corrections to reduce the Washington meridian observations to the system of the Fundamental Catalogue deduced from fundamental stars are not applicable to the observations of *Eucharis*, and gives the latter consequently a very small combining weight. But this want of harmony it doubtless due to the circumstance that the observations of fundamental stars are made in a bright field, whilst those of the planet must have been made in a dark field with illuminated threads—a difference which is quite sufficient to account for such a systematic discordance as Dr. de Ball has found to exist.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 MAY 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 May 8.

Sun rises, 4h. 22m.; souths, 11h. 56m. 20·1s.; sets, 19h. 31m.; decl. on meridian, 17° 5′ N.: Sidereal Time at Sunset, 10h. 36m.

Moon (at Last Quarter on May 14) rises, 7h. 24m.*; souths, 0h. 26m.; sets, 5h. 19m.; decl. on meridian, 13° 41′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	3 56	10 43	17 30	8 29 N.
Venus	6 1	14 30	22 59	25 10 N.
Mars	4 16	11 44	19 12	16 2 N.
Jupiter	17 30	22 44	3 58*	9 42 S.
Saturn	8 5	16 13	0 21*	22 17 N.

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

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·3	81 16 N.	May 9,	3 19 *m*
			,, 14,	2 59 *m*
U Monocerotis	7 25·4	9 33 S.	,, 9,	*M*
W Virginis	13 20·2	2 48 S.	,, 12,	2 0 *m*
δ Libræ	14 54·9	8 4 S.	,, 14,	3 36 *m*
U Coronæ	15 13·6	32 4 N.	,, 14,	19 50 *m*
S Libræ	15 14·9	19 59 S.	,, 13,	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 9,	3 22 *m*
			and at intervals of	20 8
U Sagittarii	18 25·2	19 12 S.	May 8,	20 0 *m*
R Scuti	18 41·3	5 50 S.	,, 13,	*M*
η Aquilæ	19 46·7	0 43 N.	,, 8,	0 0 *M*
S Sagittæ	19 50·9	16 20 N.	,, 14,	1 0 *m*
U Cygni	20 16·1	47 32 N.	,, 13,	*m*
W Cygni	21 31·8	44 52 N.	,, 9,	*M*
S Cephei	21 36·6	78 7 N.	,, 13,	*m*
δ Cephei	22 25·0	57 50 N.	,, 10,	0 0 *m*

M signifies maximum; *m* minimum.

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

May.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
8	γ Libræ	$4\frac{1}{2}$	1 4	near approach	168 —
13	π Capricorni	5	3 35	4 57	93 274

GEOGRAPHICAL NOTES.

IN the new number (iv.) of *Petermann's Mitteilungen* we find an interesting note by General Tillo on what he calls the chief water-partings of the earth. He points out that it is usual to indicate for each region the great water-partings, and to come down from that to what are considered as smaller or subordinate water-partings. But it seems to him that, from a general consideration of the earth's surface, the idea of a great world-water-parting may be worked out. This he illustrates by a Polar projection map in which the Old and the New World continents are brought face to face. On this he lays down one continuous line, broken only by Bering's Straits, extending from the south point of America, north along the west side of South and North America, in an irregular diagonal across Asia to the Isthmus of Suez, and down Eastern Africa to the Cape. No doubt there is much to be said for this general conception, especially as General Tillo admits that there are special continental water-partings which do not quite conform to the line of the great parting, though, as a matter of fact, nearly all the great rivers of the world are divided by this parting into two directions. It brings out strikingly the fact that the greater portion of the land-mass of the Old World, and by far the larger portion of the New World are drained into the Polar Atlantic basin.

AMONG the other papers in the new number of *Petermann* is one on the caravan routes between Suakim and Kassala, by Josef Menges. Dr. Ed. Petri contributes a paper of great interest containing some fresh and curious data on the Yakuts, of whose persistence, activity, and culture, he, like others, has formed a high estimate. Dr. Posewitz contributes a first paper on the geological condition of the Island of Billiton, with special reference to mining.

M. CHAFFAUJON has completed his exploration of the Orinoco and returned to Ciudad Bolivar. He states, in a letter dated March 15 last, that he has discovered the sources of the Orinoco, and found that the River Cassiquiari is only a branch of that river, uniting its basin with that of the Amazon; which seems pretty much what we knew before. M. Chaffaujon has sent a complete report of his exploration to the French Minister of Public Instruction, as well as some ethnographical curiosities, and a fairly complete collection of the fishes of the Orinoco.

AT a recent meeting of the Hungarian Geographical Society, Herr Moriz Von Déchy gave an account of his exploration of the Caucasus last year, in company with the geologist Dr. Schafurzik. The exploration has been rich in scientific results. Besides taking numerous observations on glaciers, measurements of heights, and many fine photographs, the explorers have brought back with them large collections, which have been deposited in the Museum of the Society, the University of Buda-Pesth, and the Hungarian Geological Institute. There are eighteen boxes of rocks and minerals. There is a small collection of beetles, and several highly interesting and valuable macrocephalic skulls. These, with the large collection of plants obtained in the Expedition of 1885, will be of the highest value to writers on the geology and natural history of this interesting region.

IT is stated in Copenhagen that an Expedition will be despatched late this summer by Herr A. Gamil, the equipper of the *Dijmphna* Expedition of 1882, to the north-east coast of Greenland. It is hoped that the explorers may reach a higher latitude than that attained by Lieut. Holm in 1884, and discern the "Sound" described by the East Greenlanders as running from the east to the west coast, somewhere in latitude 78° N. The Expedition will, if it starts, be commanded by Lieut. Hovgaard, who in 1882 commanded the *Dijmphna*.

FRANCE has succeeded in moving the eastern boundary of her Congo territory from the somewhat uncertain River Licona to the magnificent Mobangi. According to the *Times* Paris Correspondent, the *Thalweg* of the Mobangi is to be the boundary between the French territories and the Congo Free State; but how far up the Mobangi the boundary goes we are not informed. As, by the Berlin Treaty, we understand France cannot go farther north than 4° N. lat., we do not see that she gains much by this new boundary, should the Mobangi turn out, as is probable, to be identical with the Wellé. Had the Paris Correspondent taken the trouble to look at a map, he would not have told us that by this new advance France becomes mistress of the greater part of the Congo basin; the statement is absurd.

WE are glad to learn of the safety of Mr. Carey, to whose extensive journeys in Central Asia we referred in a previous number. He has wintered at Hami, and is by this time probably well on his way to India.

THOSE interested in Dr. Junker will find a very full statement of his work, with a map, in the new number of the Proc. R.G.S., by Mr. J. T. Wills. Dr. Junker's travels in the Soudan and Central Africa have lasted from the spring of 1876 to the end of 1886, with the exception of about a year and a half in 1878–80. In his first journey he found the sources of the Wellé-Makua near Lake Albert Nyanza. In 1880–83 he explored the basin of the Makua and Kuta (probably the Upper Mobangi).

THE Government of Victoria are preparing to send out a well-equipped expedition to explore the Owen Stanley Range of New Guinea, from Port Moresby, and have, we learn from the Proc. R.G.S., offered the leadership to the man of all others best able to carry out so difficult an undertaking to a successful issue—the Rev. J. Chalmers.

A FURTHER step has been taken in promotion of an expedition towards the South Pole, by the colony of Victoria. Acting on an offer made by Sir Allen Young to lead such an expedition, it is stated that Sir Graham Berry has brought the question of a Government grant towards the cost of the enterprise before the Victorian Cabinet, and that the matter is being urged forward with a view to the expedition starting from Hobson's Bay in October or November next.

THE WORK OF THE IMPERIAL INSTITUTE.[1]

II.

WHILE extolling the comprehensive and well-organised sytems of technical education existing in all parts of the Continent and the United States, let us not undervalue the great progress which has been made in recent years in Great Britain in the advancement and extension of technical instruction. The Royal Commission on the Depression of Trade and Industry state, as the result of evidence collected by them, that "It would be difficult to estimate the extent to which our industries have been aided in various ways by the advance of elementary, scientific, and technical education during the last twenty years."

The important influence exercised by the admirable work which the organisation of the Science and Art Department has accomplished, upon the intellectual and material progress of the nation, is now thoroughly recognised. Prof. Huxley, the Dean of the Normal School of Science, in his recent important letter "On the Organisation of Industrial Education," has reminded us that "the classes now established all over the country in connexion with that Department, not only provide elementary instruction accessible to all, but offer the means whereby the pick of the capable students may obtain in the schools at South Kensington as good a higher education in science and art as is to be had in the country," and "that it is from this source that the supply of science and art teachers is derived, who in turn raise the standard of elementary education" provided by the School Boards. The extension of facilities for the education of those engaged in art-industries is constantly aimed at, as was recently demonstrated by the creation of free studentships for artisans in the Art Schools at South Kensington.

The necessity which has gradually made itself felt in the manufacturing towns of the United Kingdom for encouraging the study of science in its application to industries, by those who intend to devote themselves to some branch of manufacture or trade, has led to the establishment in about twenty-five towns in England and Scotland, and in two or three in Ireland, of colleges of science corresponding more or less to the Continental

[1] Lecture (abridged) delivered at the Royal Institution, on Friday, April 22, by Sir Frederick Abel, C.B., F.R.S.; H.R.H. the Prince of Wales, K.G., F.R.S., Vice-Patron, in the Chair. Continued from vol. xxxv. p. 621.

polytechnic schools, and accomplishing important work in training students in the different branches of science in their applications to manufactures and the arts.

The wealthier of the City Companies, some of which had long been identified with important educational establishments, associated themselves with the Corporation of the City of London nearly ten years ago to establish an organisation for the advancement of technical education, which has already carried out most important work. The Society of Arts, which initiated the system of examinations, afterwards so successfully developed by the Science and Art Department, set on foot and conducted for several years examinations of artisans in a few branches of technology. This useful work was relinquished in 1879 to the City and Guilds Institute, and its gradual extension since that period has been attended with most satisfactory results. The beneficial influence exercised by the examinations upon the development and extension of technical instruction in the manufacturing districts throughout the country being already very marked. The adoption of the system, originated by the Science and Art Department, of contributing to the payment of teachers in proportion to the successes attained by their pupils, is operating most successfully in promoting the establishment and extension of classes for instruction in technical subjects, in connexion with Mechanics' Institutes and other educational establishments in various centres of industry.

The Technical College at Finsbury was the first great practical outcome of the efforts made by the City and Guilds Institute to supplement existing educational machinery, by the creation of technological and trade schools in the metropolis, and the results, in regard to number and success of students at the day and evening schools of that important establishment, have afforded conclusive demonstration of the benefits which it is already conferring upon young workers who, with scanty means at their command, are earnest in their desire to train themselves thoroughly for the successful pursuit of industries and trades. The evening courses of instruction are especially valuable to such members of the artisan classes as desire, at the close of their daily labour, to devote time to the acquisition of scientific or artistic knowledge.

Another department of the City and Guilds Institute, of a somewhat different character, is the South London School of Technical Art, which is also doing very useful work, while the chief or Central Institution for Technical Education, which commenced its operations about three years ago, if it but continue to be developed in accordance with the carefully matured scheme which received the approval of the City and Guilds Council, and with that judicious liberality which has been displayed in the design and arrangement of the building, bids fair to become the Industrial University of the Empire.

As one of the first students of that College of Chemistry which became part-parent of our present Normal School of Science, and the creation of which (forty-two years ago) constituted not the least important of the many services rendered towards the advancement of scientific education in this country by His Royal Highness the Prince Consort, most vividly I remember the struggling years of early existence of that half-starved but vigorous offspring of the great school of Liebig, born in a strangely unsympathetic land in the days when the student of science in this country still met on all sides that pride of old England, the practical man, inquiring of him complacently: *cui bono? quo bono?* That ardent lover of research and instruction, the enthusiastic and dauntless disciple of Liebig—my old master—Hofmann, loyally supported through all discouragement, and in the severest straits, by a small band of believers in the power of scientific research to make for itself an enduring home in this country, succeeded in very few years in developing a prosperous school of chemistry which soon made its influence felt upon British industry; and it is not credible that less important achievements should be accomplished, and less speedily, in days when the inseparable connexion of science with practice has become thoroughly recognised, by an Institution created, and launched under most auspicious circumstances, by those powerful representatives of the commercial and industrial prosperity of the Empire, who, before all others, must realise the vital necessity for ceaseless exertions, even for much self-sacrifice in the immediate present, to recover our lost ground in the dominions of industry.

One of the most important functions of the Central Technical College should consist in the thorough training of teachers of applied science. The statistics furnished by the technological examinations show that, while their successful organisation has led to the establishment of classes of instruction, supplementary to the general science teaching in every large manufacturing centre, the increase in the number of candidates examined has been accompanied by an increase in the percentage of failures to pass the examinations, and that the supply of a serious deficiency in competent teachers was essential to a radical improvement in technical education. The work of the City and Guilds Institute in this direction has already been well begun, and it is in the furtherance of this, by the organisation of arrangements for facilitating the attendance of science teachers for sufficient periods at the Central Institute, or at more accessible provincial technical colleges, that the Imperial Institute may hope to do good work.

Without taking any direct part in the duty of education, it is contemplated that the Imperial Institute will actively assist in the thorough organisation of technical instruction, and its maintenance on a footing, at least of equality, with that provided in other countries, by the system of intercommunication which it will establish and maintain between technical and science schools; by the distribution of information relating to the progress of technical education abroad, to the progressive development of industries, and the requirements of those who intend to pursue them; by the provision of resources in the way of material for experimental work, and illustrations of new industrial achievements, and by a variety of other means.

The provision of facilities to teachers in elementary schools to improve their knowledge of science and their power of imparting information of an elementary character to the young, constitutes another direction in which important progress may be made towards establishing that continuity between elementary and advanced education which is so well developed on the Continent. The organisation of facilities, combined with material aid, to be provided to young artisans who shall afford some legitimate evidence of superior natural intelligence, and a striving after self-improvement, to enable them to abandon for a time the duty of bread-winning, and to work at one or other of the technical schools in London or the provincial centres, will be another object to which the resources of the Imperial Institute should be applied very beneficially. Not only will the intelligent workman's knowledge of the fundamental principles of his craft or trade be thereby promoted; his association in work and study with others who are pursuing the acquisition of knowledge in different directions, which at first seem to him alien to his personal pursuits and tastes, but come in time to acquire interest or importance in his eyes, will bring home to him the advantages of a wider and more comprehensive scope of instruction, and the enlargement of his views regarding the value and pleasure of knowledge will, in turn, exercise a favourable influence in the same direction upon those with whom he afterwards comes into contact. The cramping influence which the great subdivision of labour, resulting from the development of mechanical, physical, and chemical science, is calculated to favour, must thus become counteracted, and the workman will realise that if he is to rise above the level of the ordinary skilled labourer, mere dexterity in the particular branch of that trade which he has made his calling must be supplemented by an acquaintance with its cognate branches, by some knowledge of the principles which underlie his work, and by some familiarity with the trades allied to his calling.

The importance of bringing technical instruction within the reach of the needy scholars of the lower middle class need not be dwelt upon, and there can be no question that one of the most powerful means of promoting the extension of technical education will be the well-organised administration of a really comprehensive system of scholarships, to be judiciously utilised in connexion with the well-established colleges and schools of science and technics throughout the country, in such proportions as to meet local requirements and changing conditions. That a good foundation for such a system of scholarships is likely ere long to emanate from the resources of the Royal Commission of 1851, has already been officially indicated in one of its reports; may we not also hope that many will be found in our Empire ready to follow the example of the late Sir Joseph Whitworth, and to act in emulation of the patriotism of those men who, by munificent donations or endowments in aid of the work of bringing industrial education within the reach of all classes in the United States, have helped to place our cousins in the position to hold their own and aspire to victory, in the war of industry? The thoroughly representative character which it is intended to maintain for the governing body of the Imperial Institute will secure the wise administration by it of funds of this kind, dedicated to the extension and perfection of national establishments for technical education, and to the encouragement of its pursuit, in the ways above indicated, by those whose circumstances would otherwise prevent them from enjoying the advantages secured to their fellow-workers in other countries. Several other directions readily suggest themselves in which the judicious administration of resources in aid of the technical training of eligible men of the the artisan class could well form part of the organised work of the Imperial Institute.

By the establishment of an Education branch of the Intelligence Department, which will form a very prominent section of the Imperial Institute, the working of the colleges and schools of applied science in all parts of the United Kingdom will be harmonised and assisted, and the information continuously collected from all countries relating to educational work and the application of the sciences to industrial purposes and the arts will be systematically distributed. A well-organised Inquiry Department will furnish to students coming to Great Britain from the colonies, dependencies, and India, the requisite information and advice to aid them in selecting their place of work and their temporary home, and in various other ways. The collections of natural products of the colonies and India, maintained up to the day by additions and renewals at the central establishment of the Institute, will be of great value to students in the immediately adjacent educational institutions, and will, moreover, be made subservient to the purposes of provincial industrial colleges by the distribution of thoroughly descriptive reference catalogues, and of specimens. Supplies of natural products from the Colonies, India, or from other countries, which are either new or have been but imperfectly studied, will be maintained, so that the material may be readily provided to the worker in science or the manufacturer, either for scientific investigation or for purposes of technical experiment.

The existence of those collections and of all information relating to them, as well as of the libraries of technology, inventions, commerce, and applied geography, in immediate proximity to the Government Museums of Science and Inventions, Art, and Natural History, to the Normal School of Science, and to the Central Technical Institute, present advantages so obvious as to merit some fair consideration by those who have hitherto declined to recognise any reason in favour of the establishment of the Imperial Institute at South Kensington.

In the powerful public representations which have of late been made on the imperative necessity for the greater dissemination and thorough organisation of industrial education, the importance of a radical improvement in commercial education, as distinguished from what is comprehended under the head of technical training, has scarcely received that prominence which it merits. It is true that, in some of our colleges, there are courses of instruction framed with more especial reference to the requirements of those who propose to enter into mercantile houses, or in other ways to devote themselves to commercial pursuits; but as a rule the mercantile *employés*, embraced under the comprehensive title of clerks, begin their careers in life but ill prepared to be more than mechanical labourers, and remain greatly dependent upon accident, or upon their desire for self-improvement which directs them in time to particular lines of study, for their prospects of future success in commercial life.

This impressed itself strongly upon the Royal Commission on the Depression of Trade and Industry, who state as the result of evidence collected by them that our deficiency in the matter of education as compared with some of our foreign competitors relates "not only to what is usually called technical education, but also to the ordinary commercial education which is required in mercantile houses." The ordinary clerk in a merchant's office is too often made to feel his inferiority to his German colleague, not merely in regard to his lamentable deficiency in the knowledge of languages, but in respect to almost every branch of knowledge bearing upon the intelligent performance of his daily work and upon his prospect of advancement. The preliminary training for commercial life on the Continent is far more comprehensive, practical, and systematic than that which is attainable in this country, and the student of commerce abroad has, afterwards, opportunities for obtaining a high scientific and practical training at distinct branches of the polytechnic schools and in establishments analogous to the technical colleges such as the High Schools of Commerce in Paris, Antwerp, and Vienna.

It will be well within the scope of the Imperial Institute, as an organisation for the advancement of industry and commerce, to promote a systematic improvement and organisation of commercial education by measures analogous to those which it will bring to bear upon the advancement of industrial education.

The very scant recognition which the great cause of technical education has hitherto received at the hands of our administrators has, at any rate, the good effect of rousing and stimulating that power of self-help which has been the foundation of many achievements of greatest pride to the nation, and we may look with confidence to the united exertions of the people of this country, through the medium of the representative organisation which they are now founding, for the early development of a comprehensive national system of technical education, of the nature foreshadowed not long since by Lord Hartington, in that important address which has raised bright hopes in the hearts of the apostles of education.

In connexion with some of the views which have been of late put forward regarding the possible scope of the Imperial Institute, the antagonism which has been raised and fostered against its location in the vicinity of some of our national establishments most intimately connected with the educational advancement of the Empire, has developed a tendency to circumscribe its future sphere of usefulness, and to place its functions as a great establishment of reference and resort for the commercial man in the chief foreground. I have endeavoured to indicate directions in which its relations to the Colonies and India, to the great industries of the country, and to the advancement of technical and commercial education, cannot fail to be at least as important as its immediate connexion with the wants of the commercial section of the community, and those are most certainly quite independent of the particular locality in which it may be placed, excepting in so far as the command of ample space, and the advantages to be derived from juxtaposition with the great national establishments to which I have referred, is concerned. At the same time, there is not one of the directions in which the development of the resources and activity of the Institute has been thus far indicated, which has not an immediate and important bearing upon the advancement of the commerce of the Empire. There are, however, special functions to be fulfilled by the Institute, which are most immediately connected alike with the great commercial work of the City of London and with that of the provincial centres of commerce. The provision, in very central and readily accessible positions, of commercial museums or collections of natural or import products, and of export products of different nations, combined with comprehensive sample-rooms and facilities for the business of inspection, or of commercial, chemical or physical examination, is a work in which the Institute should lend most important aid. The system of correspondence with all parts of the Empire which it will develop and maintain will enable it to collect and form a central depot of natural products from which local commercial museums can be supplied with complete, thoroughly classified economic collections, and with representative samples of all that, from time to time, is new in the way of natural products from the Colonies and Dependencies, from India, and from other countries. In combination with this organisation, the distribution to commercial centres of information acquired by a central department of commercial geography will constitute an important feature in the work of the Institute, bearing immediately upon the interests of the merchant at home, in the Colonies, and in India.

The formation of specially commercial institutions, of which inquiry offices, museums, and sample-rooms with their accessories, will form a leading feature, and which will supply a want long since provided for by the nations with whom we compete commercially, is already in contemplation in the Cities of London and Newcastle; other great commercial centres will also doubtless speedily take steps to provide accommodation for similar offshoots from the central collections of the Institute. So far as the Indian Empire is concerned, the organisation of correspondence by provincial committees which already exists in connexion with economic and geological museums established in the several Presidencies, affords facilities for the speedy elaboration of the contemplated system of correspondence in connexion with the Institute, and the establishment of similar organisations in the different Colonies will, is is hoped, be heartily entered upon and speedily developed.

The system of correspondence to which I have more than once alluded in indicating some of the work of the Institute, in relation to technical education and industry, and which will form a most important part of the main groundwork of its organisation, is not in the least theoretical in its character. Its possible development has suggested itself to many who have given thought to the future sphere of action of the Institute in connexion with commerce and industry; to myself, who for many years have been, from time to time, officially cognisant of the work performed by what are called the Intelligence Departments of the Ministries of War abroad and at home, the direct and valuable bearing of such a system upon the work of the Institute, suggested itself as soon as I gave thought to the possible future of this great conception, and to Major Fitzgerald Law belongs the credit of suggesting that the well-tried machinery of the War Office Intelligence Department should serve as a guide for the elaboration of a Commercial Intelligence Department. This Department, which will, it is hoped, ere long commence its operations by establishing relations with the chief colonies and India, will be in constant communication with the Inquiry Offices to be attached to the local commercial establishments and to other provincial representations of the work of the Institute, systematically distributing among them the commercial information and statistics continually collected. It will be equally valuable to the colonies and India by bringing their requirements thoroughly to the knowledge of the business men in the United Kingdom, and by maintaining that close touch and sympathy between them and the people at home which will tend to a true federation of all parts of the Empire.

In no more important direction is this system destined to do useful work than in the organisation of emigration, not only of labour, but also of capital. The establishment of emigration inquiry offices at provincial centres in connexion with a central department at the Institute, will be of great service to the intending emigrant, by placing within his reach the power of acquiring indispensable information and advice, and by facilitating his attainment of the special knowledge or training calculated to advance his prospects in the new home of his choice. Similarly, the capitalist may be assisted in discovering new channels for enterprise in distant portions of the Empire, the resources of which are awaiting development by the judicious application of capital and by the particular class of emigration which its devotion to public works or manufacturing enterprise in the Colonies would carry with it. The extent to which the State may aid in the organisation of systematic emigration, and the best mode in which it may, without burden to the country, promote the execution of such public works in the Colonies as will open up their dominions to commerce and at the same time encourage the particular class of emigration most advantageous to the Colonies themselves, are subjects of great present interest; but, in whatever way these important questions may be grappled with, such an organisation as the Institute should supply cannot fail to accelerate the establishment of emigration upon a sound and systematic footing, and to co-operate very beneficially in directing private enterprise into the channels best calculated to advance the mutual interests of the capitalists and the colonies.

I have already indicated that it is not only in connexion with purely commercial matters that the Intelligence Department of the Institute will occupy itself. The prospects of its value to the Colonies and to India in promoting the development of their natural resources and the cultivation of new fields for commercial and industrial activity are well illustrated by the valuable work which has been accomplished upon similar lines by the admirably directed organisation at Kew.

By the systematic collection and distribution of information relating to industries and to education from all countries which compete with ourselves in the struggle for supremacy in intellectual and industrial development, the Institute will most importantly contribute to the maintenance of intimate relationship and co-operation between educational, industrial, and commercial centres, between the labourer in science and the sources through which his work becomes instrumental in advancing national prosperity; between the Colonies and the Mother Country, between ourselves and all races included in the vast Empire of Her Majesty.

In conclusion, I venture to express the belief that the organisation which the Imperial Institute will have the power of developing, with a wisely constructed governing body at its head, may accomplish, and at no distant date, other most useful work, which has been already publicly indicated as destined to have an immediate bearing upon the federation of England and her colonies. Prof. Huxley, in his last Presidential Address to the Royal Society, uttered most suggestive words, indicative of the value and the possibility of a *scientific* federation of all English-speaking peoples; and this subject is now receiving the careful consideration of that Society. It is firmly believed by leading men of science that such a federation of at any rate the Colonies and Dependencies with us will be brought about, and it is in harmony with that belief that the Imperial Institute should be expected, through its organisation, to afford important aid in the application of the principle of federation to the geological and topographical survey of the Colonies, in the establishment of a system of interchange of meteorological and scientific observations, and in the promotion, in various ways, of thorough co-operation between particular Colonies, or groups of Colonies, for applying the results of scientific research to the mutual development of their natural resources.

It may be that the programme of which I have given a very imperfect exposition, as indicative of the work which the Imperial Institute may be called upon to accomplish, will be regarded by some as almost too ambitious in its scope for practical fulfilment. The outline of this programme has been drawn by a combination of abler hands than mine; I have but ventured to sketch in some of the details as they have presented themselves to my mind, and to the minds of others who have given thought to this great subject; but I dare to have faith in its realisation, and to believe that, if the work be taken in hand systematically and progressively, the nucleus being first thoroughly established from which fresh lines of departure will successively emanate, the Imperial Institute is destined to become a glory of the land. And, as one whose mission it has been, through many years of arduous work, to assist in a humble way in the application of the resources of some branches of science to the maintenance of the countıy's power to defend its rights and to hold its own, I may perhaps be pardoned for my presumption in giving expression to the firm belief that, by the secure foundation and careful development of this great undertaking, and by its wise direction, by a government truly representative of its founders—all Nations and Classes composing the Empire—there will be secured in it one of the most important future defences of the Queen's dominions; one of the most powerful instruments for the maintenance of the unity, the strength, and the prosperity of her realms.

THE LOCOMOTOR SYSTEM OF STAR-FISH.

PROF. PREYER, of Jena, has recently concluded an elaborate research "Ueber die Bewegungen der Seesterne." This paper, which contains over thirty illustrations, appears in the Publications of the Zoological Station at Naples (vii.), where the investigation was carried on during a period of nearly five months. This investigation was exclusively physiological, and confined to the star-fish—the Holothurians, Echini, &c., not having fallen within its scope. Considering that Prof. Preyer thus selected a line of experimental inquiry which had been already pretty well worked out, he deserves to be congratulated on the everywhere interesting and frequently novel character of his results. The most important of these results, in so far as they are new, appears to me—as also to himself—to be his demonstration that a severed ray of a star-fish exhibits much more co-ordination in the management of its tube-feet, if the section has been arranged so that two or more of the central ganglia in the disk are left in connexion with the ray, than if only one of these ganglia be so left. It was previously known that under any circumstances the severed ray of a star-fish would not only crawl about, seek the light, &c., but also right itself when turned over on its back. In order to execute this manœuvre highly co-ordinated action on the part of the tube-feet is required, and therefore the interest attaching to Prof. Preyer's observation consists in its having shown that this co-ordination cannot be nearly so well effected by one of the central ganglia as it can be by two or more of them. Or, in his own words, "Also leisten 2 functionell gleichwerthige Theile des Nervensystems zusammen qualitativ mehr als jeder für sich. Man kommt auf die Vermuthung, dass auch bei den höheren Thieren, und vielleicht auch dem Menschen, es nicht allein die qualitative Beschaffenheit der Ganglienzellen, sondern auch ihre Anzahl und Verbindung ist, welche höhere psychische Leistungen ermöglichen."

Highly interesting also are the results of numerous ingenious experiments devised with a view of testing whether the adaptive movements of star-fish can be explained as due to mechanical reflexes alone, or require us to suppose something of the nature of a rudimentary intelligence. These experiments consisted in placing the animals in various unnatural circumstances, and observing the means which they adopted in order to extricate themselves. For instance, a piece of narrow tube was pushed over one of the rays of a brittle-star, so as to tightly inclose that ray from its base to within an inch or two of its apex. In order to get rid of such an obstruction the star-fish did not always adopt the same method, as we should have expected if the adaptive actions were of a purely reflex kind. Sometimes they rubbed the tube off by friction on the ground; but if the tube were too closely fitting to admit of this mode of removing it, they would adopt sundry other devices—such as holding the tube firmly down by the other rays while drawing the imprisoned ray through its cavity; or by means of the serrated edges of the two adjacent rays progressively pushing the tube upwards over the end of the imprisoned ray; or, lastly, failing every other means, by amputating the imprisoned arm. Various other experiments were tried in the way of pinning down the star-fish in unnatural positions, and the expedients to which they resorted in order to regain their liberty appeared to Prof. Preyer amply to prove the presence in them of psychical as distinguished from merely physiological functions.

Although these are the results of most importance, many others are full of interest to the working physiologist. To me individually this is especially the case, seeing that the research has everywhere proceeded upon the same lines as those which Prof. Ewart and myself adopted while working out the physiological part of our inquiry concerning the locomotor system of Echinodermata. It is satisfactory to note that in almost every particular Prof. Preyer has corroborated our results. There are, however, four or five points—mostly of subordinate importance—with regard to which he expresses disagreement with these results. I have, therefore, carefully considered these points, and have come to the conclusion that the discrepancies admit of being explained, either (1) by our not having worked with the same species of star-fish; (2) where we did work with the same species, by our not having employed precisely the same methods of stimulation; or (3) by the temperature of the water at Naples being higher than that with which we worked in the north of Scotland. This explanation refers to the few, and comparatively unimportant, disagreements upon matters of fact. But Prof. Preyer's principal disagreement with us is upon a matter of inference. He objects to our over-caution in expressly refusing to credit the Echinodermata with any psychical faculties, remarking that many of our own results are sufficient to show that there must be something more than simple reflex mechanism concerned in the adaptive movements of these animals. Here, however, Prof. Preyer has misunderstood our meaning. We did not "expressly declare" that the star-fish are destitute of any psychical faculty: we merely excluded the question from our paper as one very difficult to answer, and as not strictly appertaining to a physiological research. But if Prof. Preyer will turn to a subsequent publication of my own, where this question does require to be considered, he will find that my views upon the subject are in very much closer agreement with his than he at present supposes.[1] Indeed, although I am perhaps less confident in attributing to them any psychical faculties other than that of a short-lived memory (which I argue admits of being proved), I think that the level in the psychological scale to which I do assign them in my book is just about the level to which, in his opinion, they ought to be assigned.

It only remains to add that for my own part I hope Prof. Preyer will next extend his researches to the Echini, which present even more abundant material for physiological investigation than the star-fish, and out of which, therefore, his observant mind may be expected to evolve even more interesting results.

George J. Romanes.

[1] "Mental Evolution in Animals," pp. 76, 342, 348–49.

THE REPORT OF THE SELECT COMMITTEE ON ENDOWED SCHOOLS.

During the present week the Report of the Committee, to the main results of which we were able to refer in our last issue, has been printed. It is a document of first-rate importance. Reserving a more detailed examination of some parts of it for a future occasion, we give this week an extract from the general conclusion of the Report, and also a summary of some of the opinions formed, and recommendations made.

Conclusion.

A pressing need now seems to be that we should not forget, in the search for more immediate advantages of an obvious nature, the importance of preserving, even at some cost, a high ideal of secondary education, both on its own account and in its connexions either with the Universities or with the excellent Colleges which have been recently established in our large towns with the special object of education in relation to the needs of manufacturing and commercial communities. Your Committee find that the work done by the Charity Commissioners under the Endowed Schools Acts, while it has not lost sight of this ideal, has done much to bring higher instruction, in popular and necessary forms, within the reach of classes which otherwise would have been shut out from it. It has thus fulfilled a double function: to promote in all classes the creation of trained intelligence, and to build up a system under which, when created, it may find a free and prosperous scope. With such improvements as your Committee have recommended in future schemes, it is to be hoped that the intelligence of the working-classes will be trained in a direction which, while it develops their intellectual faculties, will at the same time enable these faculties to be more readily applied to the needs of productive industry.

Summary.

The great extension in elementary education under the Education Acts having, to a certain extent, altered the position and objects of elementary endowed schools, in any scheme for remodelling them, special attention should be directed to providing, as far as possible, for the children of the working-classes a practical instruction suitable to their wants in the particular circumstances of each locality.

The policy of the Commissioners has been to establish scholarships in elementary schools and exhibitions from them to schools of secondary education. On the whole these have worked well in large towns, but they are less adapted to the circumstances of a scattered rural population; and in any case scrupulous care should be taken where endowments have been appropriated to the poor, that the paramount interests of the poor should be secured in the application of scholarships or exhibitions provided out of the trust funds.

The abolition of gratuitous education in elementary endowed schools is generally opposed to the wishes of the poorer classes in the localities. It is only justifiable when the imposition of fees gives a higher and more useful character of education to the working-classes than they formerly enjoyed, and after provision made for payment of school fees of children whose parents stand specially in need of such assistance.

The application of non-educational endowments to educational purposes under Section 30 of the Act of 1869 has been beneficial, but the veto now possessed by the trustees of such endowments is, in some cases, a hindrance to reforms and an inadequate protection for the poor. It would be expedient to substitute, for the consent of the trustees, the concurrence of some local representative body.

The diversion of educational endowments from one locality, decreasing in population, to a neighbouring populous locality, is sometimes necessary, but should only take place after the requirements of the locality have been met.

The diversion of an endowment, partially or entirely, from the education of boys to that of girls, has been successful in numerous instances, but when opposed by the localities it requires discretion in its exercise.

The extension of technical and higher commercial education has risen to much importance since the Act of 1869, and should be carefully kept in view by the Commissioners in framing their schemes. When the value of the endowment is too small to provide laboratories and workshops for technical or scientific teaching, the local authorities might be empowered to initiate and aid them by local rates. But before applying local rates in aid of technical or scientific teaching, endowments, the purposes of which have failed, should, as far as practicable, be utilised.

The examination of endowed schools and inspection of the state of the buildings and apparatus, and of the discipline and general working, are subjects of urgent importance. Reports upon the actual condition of the schools should be periodically laid before Parliament. Those reports should be published in the locality in a cheap and convenient form.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Applications to occupy the Cambridge table at the Naples Zoological Station should be sent to Prof. Newton on or before May 26.

Mr. S. F. Harmer has been approved by the Senate, on the recommendation of the Special Board of Medicine, as a teacher of Comparative Anatomy for the purposes of medical study.

The reports of Mr. H. Gadow, M.A., of King's College, and Mr. M. C. Potter, M.A., of Peterhouse, to whom grants were made from the Worts Travelling Scholars Fund last year, have just been published.

Mr. Gadow states that he began his researches on July 2, 1886, with the exploration of several caves on the Monte Junto and in the Serra de Athouguia, Province of Estremadura. In the caves were found a considerable number of human and other bones, many of which show unmistakable signs of being worked and cut by prehistoric man; they are now in the Museum of Zoology, awaiting further investigation. Fourteen celts, some worked flakes, and a flint arrowhead, collected in the caves or in the neighbourhood thereof, are now in the Museum of Archæology.

Mr. M. C. Potter joined Dr. Gadow on August 14 at Porto, and immediately went in search of *Clemmys caspica* (the water tortoise which bears the Alga); finding this tortoise was scarce in the North of Portugal, they went to Santarem, where it also was not procurable in sufficient numbers. They therefore proceeded to the Eastern Alemtejo to the mines of São Domingos; here, during several successful expeditions, they succeeded in obtaining a great number of *Clemmys caspica*, and with them a good supply of the parasitical Alga. At the mines of São Domingos, Mr. Potter was able to carry on his investigations through the kindness of his friend Mr. T. Warden, who placed his house at his disposal. The results have already been published in a preliminary form in the Proceedings of the Cambridge Philosophical Society, and will probably be published in full by the Linnean Society of London. To this Alga, hitherto undescribed, he has given the name of *Epiclemmidia lusitanica*, thus describing its nature and to some extent its geographical distribution. The expedition was of great value in enabling him to study the geographical distribution of many plants, and to collect specimens for the Botanical Museum, especially at Coimbra, where the Scientific Staff of the University presented both gentlemen with many valuable specimens.

SCIENTIFIC SERIALS.

Botanische Jahrbücher (A. Engler), vol. viii. Part 3.—On the history of development of form in the Roburoid Oaks, by Franz Krasan (two plates). The author points out, among other conclusions drawn from a comparison of ancient and modern forms, that the developmental series of forms of Oak extending continuously over immeasurable periods of time is compendiously summarised before our eyes in the development of the individual, *i.e.* that the ontogeny is an epitome of the phylogeny.—On *Eria choneana*, a new species, by Fr. Kränzlin.—Descriptions of Lehmann's collections in Guatemala, Costa Rica, and Columbia: Cyperaceæ, by O. Böckeler; Liliaceæ, Hæmodoraceæ, Amaryllidaceæ, Dioscoreaceæ, and Iridaceæ, by J. G. Baker; Passifloraceæ and Aristolochiaceæ, by Maxwell T. Masters; Lythraceæ, by E. Kœtner.—The Hungarian species of *Inula*, especially those of the *Enula* group, by Vincentius de Borbás.—The remainder of this number is taken up by the continuation of Dr. Winter's excellent epitome of the recent literature on the classification and geographical distribution of fungi, and by Dr. F. von Herders' article on new contributions to the geographical botany of Russia.—Notice is also given in this number of the joint work by Profs. Engler and Prantl, to be entitled "Die natürlichen Pflanzenfamilien." This will be a very comprehensive, profusely-illustrated work, while the names under which it is to be issued will be sufficient guarantee of its excellence.

THE principal article in the current number (vol. v. Part ii.) of the *Folk-Lore Journal* is the continuation of Miss Courtney's paper on Cornish folk-lore, which is very exhaustive. Mr. Kirby calls attention to five tales in the "Arabian Nights," which, though differing greatly from each other, are all based upon two simple fundamental ideas, viz. a door which it is forbidden to open, and the hero falling in love with a woman seen from a house-top. The five tales which are examined lead by curious gradations from the simplest form of the story to the most complex. In response to an appeal issued by the local Secretary in Hong Kong to dwellers in the Far East, we get several Chinese and Japanese contributions. The most important of these relates to the folk-lore of aboriginal Formosa, and is written by Mr. G. Taylor, whose papers on Formosa and its aborigines in the *China Review* were noticed several times last year in these columns. From Formosa, as elsewhere in the world, the cry comes that the aborigines are either disappearing, or are becoming sophisticated by their contact with civilised races. "Come quickly, or you will be too late," says Mr. Taylor to inquirers. He is certainly losing no time in making the most of his opportunities as a resident, and it is to be hoped he will continue his researches. Mrs. Mansfield supplies some interesting Chinese superstitions respecting children; and Mr. Hartland writes on the somewhat hackneyed subject of Japanese New Year decorations. The late Mrs. Chaplin Ayrton almost exhausted this subject in a paper read about ten years ago before the Asiatic Society of Japan, and reproduced by her a few years later in a charming book on child-life in Japan. The other papers, dealing with Negro songs in Barbados, and American song-games and wonder-tales, show that this interesting Society is extending the area of its activity so as to include all parts of the globe. Cornwall, Arabia, Formosa, Barbados, the United States, Japan, do not form a bad assortment for a single number of this journal.

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, April 23.—Prof. W. G. Adams, Vice-President, in the chair.—The following papers were read:—On delicate calorimetrical thermometers, and on expansion of thermometer-bulbs under pressure, by Prof. Pickering. The reading of a delicate mercurial thermometer, when placed in a bath at constant temperature, is found to depend on whether the thermometer was at a higher or lower temperature than the bath, before immersion. Capillarity was suggested as an explanation, but experiment showed that the effect was not always greatest at the narrow parts of the tube, and hence this idea was discarded. By using the same tube with different bulbs attached, the differences varied, and eventually the effect was found to be caused by exposing the inside of the tube to air and moisture; for when bulbs were attached to new tubes, without being so exposed, the differences between the rising and falling readings disappear. Hence, for very delicate thermometers great care should be taken not to expose the bore of the tube, and calibration of a tube before attaching the bulb must not be attempted. Even in the best tubes, after every possible precaution has been taken, the author finds some parts about which the mercury appears to stick, and in delicate observations these parts of the tube are to be avoided. He also finds it necessary to gently tap the top of the tube to relieve any friction, and has devised a clockwork arrangement for performing the operation uniformly. In the second part of the paper the author describes the want of concordance between the thermometers which have been compared with the same standard, and finds it due to the expansion of the bulbs not being in all cases proportional to the difference of pressure between the inside and outside. Thermometers with large thin bulbs show greatest discrepancies, and the remedy is found to be in making the bulbs more rigid. This is done by having a double bulb, making them from a cylindrical tube instead of by blowing, and increasing the thickness of the walls of the bulb. A knife-edge arrangement in the upper part of a thermometer is described, by which the same part of the graduated tube can be used, whatever the temperature (about which small changes are to be observed) may be. The proper amount of the mercury column can be cut off with the greatest nicety by its use. Mr. Whipple remarked that phenomena similar to those described in the paper were constantly coming under his notice, and mentioned the pressure-corrections they were applying to thermometers used *in vacuo*, during some pendulum experiments at present being carried out. He also described the Kew method of determining the pressure-correction in deep-sea thermometers, which are protected by an outer glass jacket filled with alcohol. Mr. Lant Carpenter described the first comparison experiments made at sea with protected and unprotected bulb thermometers. In answer to questions, Prof. Pickering said the range of pressure used was rom 0 to 3 atmospheres, and in his most delicate thermometer, where 200 millimetres correspond to 1° C., the difference between readings taken in horizontal and vertical positions amounts to 30 millimetres.—Note on magnetisation; on sequences of reversals, by Mr. R. H. M. Bosanquet. Some experiments have recently been made on an iron bar whose magnetic properties under reversals with ascending values of current were first determined some years ago. The magnetic resistances have again been determined, first with ascending values of current, and afterwards with descending values. In all cases the induction was measured by reversing the current. The results generally show a greater magnetic resistance for descending values of current, except for small inductions where the resistance was less, when the experiments were performed in the above order. The paper concludes with a molecular hypothesis to explain the above results.—On a thermo-dynamical relation, by Prof. Ramsay and Dr. S. Young. The paper is an extension of one presented to the Society on February 26, and of which an abstract was read by the Secretary. The numerical results are given, from which the authors deduce the relation $p = bt - a$, for constant volume, and additional reasons are given for believing acetic acid (whose vapour-density at ordinary temperatures is abnormal) to be a mixture of $C_2H_4O_2$ and $C_4H_8O_4$, the former preponderating as the temperature rises. The authors ask the Society for a name to designate lines connecting pressure and temperature at constant volume, and for which they suggested "*isochor*" in their previous paper.

Zoological Society, April 19.—Mr. Osbert Salvin, F.R.S., Vice-President, in the chair.—The Secretary called attention to a set of eleven photographs representing the principal objects of natural history collected by the celebrated traveller Prjevalsky, during his four expeditions into Central Asia, and to an accompanying catalogue of them which had been presented to the Society's library by Dr. A. Strauch, of the Imperial Museum, St. Petersburg.—Mr. T. D. A. Cockerell exhibited and made remarks on some specimens of rare British slugs taken at Isleworth, Middlesex.—The Secretary read some extracts from a letter addressed to him by Mr. A. A. C. Le Souef, giving an account of a successful attempt to keep the duck-billed *Platypus*, or water-mole, alive in captivity in the Zoological Gardens at Melbourne.—Mr. J. Bland Sutton exhibited some specimens of diseased structures taken from mammals that had died in the Society's Gardens, and made comments thereon.—Mr. J. Bland Sutton read a paper on the singular arm-glands met with in various species of the family Lemuridæ.—Mr. F. E. Beddard read a paper on the anatomy of earthworms, being a further contribution to his researches on that subject.—A communication was read from Mr. A. D. Bartlett, Superintendent of the Society's Gardens, containing remarks upon the mode of moulting of the Great Bird of Paradise (*Paradisea apoda*), as observed in a captive specimen.—A communication was read from Mr. J. Douglas Ogilby, of the Australian Museum, Sydney, containing the description of a rare Australian fish (*Girella cyanea*).—A second paper by Mr. Ogilby contained the description of an undescribed fish of the genus *Prionurus*, obtained in Port Jackson, which was proposed to be called *Prionurus maculatus*.

Chemical Society, March 30.—Annual General Meeting.—Dr. Hugo Müller, F.R.S., President, in the chair.—The President delivered an address, some extracts from which we have already printed.—Prof. Odling proposed that the thanks of the meeting be given to the President for his address, and that he be requested to allow it to be printed. This motion was seconded by Dr. Gladstone, and accepted with acclamation by the Fellows present. The President acknowledged the compliment.—Dr. A. K. Miller and Dr. Rideal were appointed scrutators, and a ballot having been taken, the following were declared elected as Officers and Council for the ensuing year:—President: W. Crookes, F.R.S. Vice-Presidents who have filled the office of President: Sir F. A. Abel, C.B., F.R.S.; Warren De La Rue, F.R.S.; E. Frankland, F.R.S.; J. H. Gilbert, F.R.S.; J. H. Gladstone, F.R.S.; A. W. Hofmann, F.R.S.; H. Müller, F.R.S.; W. Odling, F.R.S.; W. H. Perkin, F.R.S.; Sir Lyon Playfair, K.C.B., F.R.S.; Sir H. E. Roscoe, F.R.S.; A. W. Williamson, F.R.S. Vice-Presidents: J. Dewar, F.R.S.; David Howard; H. McLeod, F.R.S.; Ludwig Mond; C. Schorlemmer, F.R.S.; W. A. Tilden, F.R.S. Secretaries: H. E. Armstrong, F.R.S.; J. Millar Thomson. Foreign Secretary: F. R. Japp, F.R.S. Treasurer: W. J. Russell, F.R.S. Ordinary Members of Council: Messrs. T. Carnelley, M. Carteighe, A. H. Church, Frank Clowes, P. F. Frankland, R. J. Friswell, E. Kinch, R. Messel, H. F. Morley, J. A. R. Newlands, W. Ramsay, Thomas Stevenson.

April 7.—Mr. William Crookes, F.R.S., President, in the chair.—The following papers were read:—Researches on the constitution of azo- and diazo-derivatives; II. Diazoamido-compounds (continued), by Mr. R. Meldola, F.R.S., and Mr. F. W. Streatfeild.—Conjugated sulphates and isomorphous mixtures of the copper-magnesium group, by Mr. P. C. Roy.—Suboxide of silver, Ag_4O, by Mr. G. H. Bailey and Mr. G. J. Fowler.—Action of trimethylenebromide on the sodium compounds of ethylic acetoacetate, benzoylacetate, paranitrobenzoylacetate, and acetonedicarboxylate, by Dr. W. H. Perkin, Jun.

Institution of Civil Engineers, April 19.—Mr. Edward Woods, President, in the chair.—Four papers were read on the subject of obtaining water-supply from wells, namely, chalk springs in the London Basin, by Mr. J. W. Grover; borings in the chalk at Bushey, Herts, by Mr. William Fox; on a borehole in Leicestershire, by Mr. T. S. Stooke; and the wells and borings of the Southampton Waterworks, by Mr. William Matthews.

Paris.

Academy of Sciences, April 25.—M. Janssen, President, in the chair.—Remarks on M. Colladon's note of April 18, by M. Faye. In reply to M. Colladon's statement that his observations had reference to whirlwinds and waterspouts and not to cyclones or tornadoes, the author points out the great analogy that exists between these two orders of phenomena, both being descending vortices with vertical axis originating in the upper atmospheric regions. The essential difference is that the cyclones are much larger, and that their movement takes its rise at a much higher elevation; but both are subject to the same laws, while it is quite impossible to separate waterspouts from tornadoes.—Experiments for determining the coefficient of nutritive and respiratory activity of the muscles at work and in repose, by M. A. Chauveau and M. Kaufmann. Here a solution is attempted of the physiological problem, to determine for a given weight of living muscular tissue and for all the normal and regular physiological conditions of such tissue (1) the quantity of blood flowing through it in a given time for purposes of nutrition; (2) the weight of oxygen absorbed by this tissue, and of the carbonic acid secreted by it in the same time; (3) the weight of the substances which supply the carbon contained in the carbonic acid gas.—On a new species of truffle, by M. Ad. Chatin. It is shown that the truffle produced in Champagne and Burgundy is not the common species known as *Tuber rufum* and *T. æstivum*, but another hitherto undescribed variety here specified and named *Tuber uncinatum*.—Remarks on a thunderbolt of an unusually destructive character, by M. Daniel Colladon. An electric discharge is described which occurred on April 7 at Schoren in the Canton Bern, and which, after striking a large poplar, spread havoc for some hundreds of metres around, comparable to the effects caused by the explosion of a powder magazine. The shock was felt in Langenthal, three-quarters of a mile off, where several windows in a house were smashed.—On acute pneumonia, by M. Jaccoud. The observations here described establish the fact that true pneumonia is due not to the accidental penetration of specific microbes into the system, as is usually supposed, but to the development under favourable conditions of microbic germs permanently present in the system. A chief condition of such development is a sudden chill, which explains the frequent coincidence of lung affections with abrupt changes of temperature.—Note on the method of research for determining the correlation between two orders of facts, by M. de Montessus. The reference is to M. de Parville's recent paper on the correlation between earthquakes and lunar declination. The difficulty of correlating such phenomena is commented upon, which sufficiently accounts for the failure of the numerous attempts hitherto made to establish a distinct relation between the movements of the moon and those of the terrestrial crust. Such a relation would be equivalent to an experimental demonstration of the hypothesis which assumes that the centre of the earth is in a fluid state.—On the earthquake of February 23, 1887, by M. Albert Offret. With the data supplied from the various localities affected, an attempt is here made accurately to determine the moment when the shock reached the different points in the central part of the seismic area. The results are shown in two separate tables for France and Italy.—Expansion and compressibility of water, and displacement of the maximum of density by pressure, by M. E. H. Amagat. The author has carried his experiments on water as far as 3200 atmospheres, operating between 0° and 50° C. as limits of temperature, with the general result that a sufficient increase of pressure and temperature tends to bring water within the normal condition of other fluids. Towards 3000 atmospheres the last traces disappear of the perturbations of the general laws resulting from the existence of the maximum of density.—Isogonic magnetic curves, by M. C. Decharme. The author endeavours to show by a series of diagrams the double magnetic influence to which the needle is subjected in the vicinity of a magnet.—A study of the alkaline vanadates (continued), by M. A. Ditte. Here are examined the vanadates of lithine, to which is appended a general table of the well-defined crystallized salts yielded by potassa, soda, ammonia, and lithine.—Artificial production of magnetite, by M. Alex. Gorgeu. By the process here described a magnetic oxide is obtained apparently identical with natural magnetite. It is attracted by the magnet, shows a metallic lustre, and affects opaque octahedral forms, sometimes modified by minute facets of the rhomboidal dodecahedron, with hardness from 6 to 6·5, and density 5·21 to 5·25.—Qualitative study of the sulphites in the presence of the hyposulphites and sulphates, by M. A. Villiers. A convenient and rapid process is described for the research of the sulphites in the presence of the hyposulphites, which, like the former, liberate sulphurous acid by the action of the acids.—On the various sulphurous waters of Olette, Eastern Pyrenees, by M. Ed. Willm. A tabulated analysis is given of these waters on the assumption that all the carbonic acid is combined under the form of bicarbonates.—On synthetic acetonitril, by M. Louis Henry. The acetonitril obtained by the process here described is in every respect identical with that yielded by the dehydratation of acetamide.—On some cases of morphinomania in animals, by M. Ludovic Jammes. Several instances are mentioned of cats, and especially monkeys, acquiring a decided taste for the fumes of opium through association with opium-smokers in Camboja and China.

BERLIN.

Meteorological Society, April 5.—Prof. von Bezold, President, in the chair.—Prof. Upton, of Providence (U.S.A.), spoke on meteorological observations during eclipses of the sun. After discussing the phenomena which may theoretically be expected during an eclipse, he gave a full account of his own meteorological observations, already known to the readers of NATURE, which he made during the eclipses of May 6, 1883, on the island of Carolina, and which have already been published in full. He then discussed an explanation of the barometric variations during the period of totality which had been appended to the report of his observations as published in the *Zeitschrift für Meteorologie*, and expressed his dissatisfaction with the same. He is rather inclined to believe that the very evident fall in the atmospheric pressure before the period of totality is due to an outrush of air which is becoming cooled in the moon's shadow, and that the rise of pressure which is observed shortly after the period of totality is due to a compensating inrush of air. In conclusion Prof. Upton pointed out the importance of making meteorological observations in Prussia, especially as regards the variations of barometric pressure, during the total eclipse of August 19, along the line of total eclipse, and more particularly along the boundaries of the area of totality. In the discussion which followed, Prof. Spörer gave a description of the dense clouds of mist which he observed close to the earth during the eclipse of August 18, 1868, in India. Several days previously to the eclipse there had been a heavy fall of rain; the unclouded sun which rose on the 18th heated the surface of the earth and the dark stones, and then, as soon as the totality began, long, dense bands of mist made their appearance, and with the sharp breeze which was blowing gave rise to a very obvious sensation of coolness. Dr. Zenker then pointed out that, inasmuch as in Prussia the period of totality would occur in the very early morning hours, it would be out of place to expect any very marked variations of either temperature or barometric pressure. On the other hand, he considered that the meteorological observations should be directed more especially to an investigation of Bishop's rings, and of the alternating light and dark bands which precede and follow the stage of totality; they are probably interference fringes, and could be most efficiently recorded by means of photography. Prof. von Bezold laid stress on the importance of observations on the twilight which occurs during the eclipse, pointing out that specially favourable conditions for such observations will present themselves in Germany during this year. It is to be hoped that exact observations of the umbra, the penumbra, and the colours which simultaneously make their appearance will throw considerable light on the phenomenon. Prof. von Bezold intends to provide at the time for an adequately numerous participation in carrying out these observations.

STOCKHOLM.

Royal Academy of Sciences, March 9.—Sir Lowthian Bell, Bart., was elected a foreign corresponding member of the Academy.—On the species and varieties of the Coniferæ found in Scandinavia, by Prof. Wittrock. He also exhibited *Viola Sueciæ exsiccatæ*, prepared by Messrs. L. M. Neuman, L. J. Wahlstedt, and S. Murbeck.—Report of a visit to some lakes and fresh-water basins in Sweden for the purpose of studying the flora, by Dr. N. A. Lundström.—Remarks on the fishes of the Mediterranean and the Sea of Japan, by Prof. F. A. Smitt.—A description of the collection of Japanese fishes in the zoological museum of the University of Upsala, by Herr E. Nyström.—Researches on the volume and composition of the gases resulting from the solution of iron in acids, by Herr H. Bäckström and Herr G. Paykull.—On the effects of hardening on the volume and composition of the gases evolved on the solution of steel in acids, by Herr G. Paykull.—On the number and congruences of the roots of the second order, by Dr. A. Berger.—On the integration of the differential equations for a material point movement, by Dr. G. Kobb.—The Secretary announced the acceptance of the following papers for publication in the Academy's Proceedings:—On the application of a numerical-theoretic formula for the transformation of a definite double integral, by Dr. A. Berger.—Contributions to the theory relating to the undulating movement in a gaseous body, by Prof. A. V. Bäcklund.—On allaktite from the Långban Mines, by Herr A. Sjögren.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Problem of Evil: G. D. Thompson (Longmans).—Die Natürlichen Pflanzenfamilien, Lief. 1 and 2: A. Engler and K. Prantl (Engelmann, Leipzig).—A Popular History of Astronomy during the Nineteenth Century, 2nd edition; A. M. Clerke (Black, Edinburgh).—Bulletin de la Société Impériale des Naturalistes de Moscou, No. 4, 1886, and No. 1, 1887 (Moscou).—Botany Notes, 2 parts, 3rd edition: A. Johnstone (Livingstone).—Organic Materia Medica: R. Bentley (Longmans).

CONTENTS

PAGE

THURSDAY, MAY 12, 1887.

THE AINOS.

The Language, Mythology, and Geographical Nomenclature of Japan viewed in the Light of Aino Studies. By Basil Hall Chamberlain. Including an Ainu Grammar by John Batchelor, and a Catalogue of Books relating to Yezo and the Ainos. (Memoirs of the Literature College, Imperial University of Japan, No. 1, 1887. Published by the Imperial University, Tōkyō.)

THE Ainos have long been a puzzle to the philologist and ethnologist. Their place amongst the races of the world, living or extinct, has been, and remains, unsettled; and their relations to the present inhabitants of the Japanese archipelago, though the subject of frequent discussion in recent years in Japan, form an unsolved problem. Like fragments of races still existing in various other lands, the Ainos refuse to fit into any ethnological scheme, and are waifs and strays in science much as they are in the world around them. The main cause of this, no doubt, is that so little has really been known about this curious race. A certain amount of knowledge has been repeated by one writer after another, but that invaluable instrument of investigation, the Aino language, has stood outside the pale of philology. The writer of this very important and interesting monograph does not attempt to answer the questions arising out of the presence of the Ainos in Yezo, the Kuriles, and Southern Saghalien; his object is, "by comparing the language and mythology of the Ainos with the language and mythology of the early Japanese, to ascertain what sort of relationship, if any, exists between the two races, and to shed light on the obscure problem of the nature of the population of the Japanese archipelago during late prehistoric times." His equipment for this interesting task is a profound knowledge of the Japanese language and mythology—"which, in the absence of a thorough practical knowledge of Aino itself, is the first condition of the successful investigation of any subject connected with the Island of Yezo"—and travel and investigation, especially in regard to their myths, amongst the Ainos themselves. He has also associated with him in the work the Rev. John Batchelor, of the Church Missionary Society, who has published in the present monograph a grammar of the Aino language, and whose "five years' intercourse with the Ainos in their own homes, and close study of the language as it falls from the lips of the people, enable him to speak with an authority belonging to no other investigator." The result of this co-operation is the work before us, and perhaps the best method of reviewing it is to explain the method followed by Prof. Chamberlain in his investigation, and the results at which he has arrived.

First, then, he compares the Aino language with the Japanese. The close and intimate resemblance between the two is only superficial, and vanishes as soon as they are carefully compared. "The paradox of two races so strongly contrasted speaking related languages has no foundation in fact." Then follows a list of fifteen salient points of difference between the Japanese and Aino linguistic systems. Some of these, the writer says, may not be appreciated at their true value by scholars accustomed exclusively or chiefly to the study of the Aryan family of languages, whose looser structure allows of such wide divergences between the various members of the family. "But the Altaist, knowing the iron rule which forces all the Tartar tongues into the same grammatical mould, however widely their vocabularies may be separated, will hold the opinion of fundamental want of connexion between Japanese and Aino, until very strong arguments shall have been brought forward on the other side," and he proceeds to point out that on thirteen of the fifteen points of difference there is absolute identity between Japanese and Corean. As for the points of similarity between Japanese and Aino—such as the same construction of the sentence, and nearly the same phonetic system—Prof. Chamberlain suggests the long contact between the two peoples; but the borrowing, if borrowing there be, must have been on the side of the Ainos. On the whole, he is inclined to accept the theory of Von Schrenck, in his work on Amur Land, that Aino is to be regarded as a language altogether isolated at the present day, and "when it is remembered that the Aino race is isolated from all other living races by its hairiness and by the extraordinary flattening of the tibia and humerus, it is not strange to find the language isolated too." He treats with ridicule the suggestion that the Aino may be an Aryan tongue.

Next he comes "to that of which language is the vehicle—to the religion, the traditions, the fairy-tales of the two nations. Do the Ainos account for the origin of all things after the manner of their Japanese neighbours? Do Aino mothers and Japanese mothers lull their little ones to sleep with the same stories?" Japanese mythology is almost all to be found in the *Kojiki*, a work of the early part of the eighth century of our era, a literal translation of which, by Prof. Chamberlain himself, was noticed in NATURE a few years ago. With regard to the Aino myths, as there are no Aino books of any sort, these have to be obtained orally, by a tedious process of listening to successive story-tellers, for the brain of the Aino soon tires. The writer gives the results side by side: on one side the Japanese account of the Creation, of their origin, of the origin of civilisation, of the aborigines, of heaven and hell, the sun and moon; and then the Aino myths on similar subjects. In addition, a large number of stories of both peoples, relating to such subjects as Rip Van Winkle, the Isle of Women, a visit to the underworld, various beast-myths, stories about monsters, the causes of the peculiarities of natural objects, &c., are related—sometimes side by side for purposes of comparison; sometimes only the Aino version is given, the corresponding Japanese tale being readily accessible elsewhere. It will be seen from this bare outline of the contents of this section that a new world of folk-lore is here opened to the study of inquirers into this branch of research. The general conclusion at which Prof. Chamberlain arrives after this comparison of the two mythologies is that there is even less connexion between them than between the two languages. The stories could scarcely be more divergent in general complexion. The Japanese stories "are myths pure and simple, airy phantoms of the imagination," and have no moral tendency whatever. Japanese commentators on their own myths, struck with the absence of morality in them, account for it by saying that their countrymen needed no moral teaching, because they were perfect already, and not depraved like the Chinese and foreign nations generally. The tales of the Ainos, on the other hand, generally point a moral or account for some natural fact. Birds and beasts are the characters; those of the Japanese are generally men, or gods who are the counterparts of men.

The third line of investigation adopted by Mr. Chamberlain is that of place-names in Yezo and in Japan. His first process was to make a catalogue of the names of the principal places in Yezo, with their Japanese corruptions, and the Chinese characters used by the Japanese in writing them. From this he compiles a kind of key, composed of Aino words contained in the names of places in the previous list, and common Aino designations for features of the landscape, such as are likely to occur in the names of places, their meanings, and the Japanese pronunciation. This he applies to the place-names of Japan proper in order to test whether they are of Aino origin. By this ingenious, elaborate, and toilsome method, Prof. Chamberlain examines numbers of Japanese geographical names in various parts of the country, with an amazing profusion of learning. We can do no more here than give the broad results, which are sufficiently clear and striking. He can say with certainty that names, as to the Aino origin of which there can scarcely be a question, may be traced right through the main island of Japan into the two great southern islands. They are fairly abundant even in the extreme southern province. The inference to be drawn from this is that the Ainos were the true predecessors of the Japanese all over the archipelago. "The dawn of history shows them to us living far to the south and west of their present haunts; and ever since then, century by century, we see them retreating eastwards and northwards, as steadily as the American Indian has retreated westwards under the pressure of the colonists from Europe."

It will be observed that Prof. Chamberlain comes to this conclusion, after a comparison of the languages, mythologies, and place-names of the Japanese and the Ainos; it is likewise the conclusion at which Prof Milne arrived a few years ago along a different line of investigation. The latter gentleman compared the kitchen-middens, stone implements, and other prehistoric remains found in numerous parts of Japan with those of undoubted Aino origin—some of the middens being even now in course of construction in Yezo. Prof. Milne's papers on the subject will be found in the Proceedings of the Anthropological Society for 1881, and of the Asiatic Society of Japan (vol. viii. Part 1, and vol. x. Part 2). Hence it may be taken as established sufficiently for all practical purposes that the pre-Japanese inhabitants of the Japanese islands were the Ainos. Beyond this conclusion we are not taken either by Prof. Milne or by Prof. Chamberlain, and with it we must be content until scholars have carried out that series of linguistic comparisons which is "the surest key for unlocking the mysteries of racial affinity and race migrations in this portion of Asia," of which Prof. Chamberlain's work is the beginning.

The last word is very far indeed from being yet said about the Ainos. Meanwhile their numbers are growing smaller decade by decade, their industries are passing into Japanese hands; the animals which were their principal sustenance are rapidly becoming extinct; the survivors of this people almost all speak Japanese as well as their own tongue, and are losing their special characteristics. Hence they "must without delay be subjected to all the necessary scientific tests: their language must be analysed, their folk-lore registered; for soon there will be nothing left." Prof. Chamberlain does not share the regrets of those who mourn over the *Japonisation* and approaching extinction of the Ainos. They have had abundant opportunities of improvement, but they have not profited by them. The son of the greatest living Aino chief is glad to brush the boots of an American family in Sapporo. This is how their latest investigator concludes his interesting and instructive monograph:—"The Aino race is now no more than a 'curio' to the philologist and to the ethnologist. It has no future, because it has no root in the past. The impression left on the mind after a sojourn among the Ainos is that of a profound melancholy. The existence of this race has been as aimless, as fruitless, as is the perpetual dashing of the breakers on the shore of Horobetsu. It leaves behind it nothing save a few names." The whither of the race is unhappily only too certain; its whence still remains a question to perplex the ethnologist, and, if present indications are to be trusted, it will continue an unsolved problem for many years to come. Prof. Chamberlain's monograph carries us just one step back in the life-history of the race; behind that, all is still darkness and doubt.

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 Capt. G. S. Nares, R.N., F.R.S., and of the late Capt. F. T. Thomson, R.N.* Prepared under the Superintendence of the late Sir C. Wyville Thomson, F.R.S., &c., and now of John Murray, one of the Naturalists of the Expedition. Zoology—Vol. XVII. (Published by Order of Her Majesty's Government, 1886.)

VOLUME XVII. of the Zoological Reports of the voyage of the *Challenger* contains three memoirs. The first is the second and concluding Part of the Report on the Isopoda collected during the Expedition, by Mr. Frank Evers Beddard, of which the first Part was published in 1884, and dealt exclusively with the family of the Serolidæ. The collection of Isopoda made during the voyage was very rich in new species and genera, more particularly in the deep-water forms, of which no less than thirty-eight are described as new. Among the shallow-water species the greater number of novelties were dredged off Kerguelen and the adjacent islands, adding no less than fifteen new species to the previously short list known. In other parts of the world, with the exception of Australia, dredging in shallow water did not yield any considerable number of species of the group. Many of the species described as new were previously briefly diagnosed by the author, in the Proceedings of the Zoological Society of London. Passing from the description of the species of this exceedingly interesting group of Crustacea to the summary of their distribution, we find that, while the 300-fathoms line marks approximately the boundary between what may be called the deep-sea and the shallow-water species, yet there is no trace of any zone of depth that has not its Isopod fauna. From 345 fathoms down to 2740 fathoms, species were dredged continuously; there was nowhere a break of more than 100 fathoms. In passing from the lesser to the greater depths, there is evidently a decreasing number of species that are common to these depths and to shallow water; but it is impossible to draw an absolute line of division which would separate an abyssal from a shallow-water fauna. Of the seven species found at a depth of 2000 fathoms and upwards, two range into lesser depths, another (perhaps two) into shallow water, leaving only three distinctly abyssal forms.

One of the most important results of these investigations has been to show that ocean regions cannot be marked out with anything like the same definiteness as can the terrestrial areas. Among the Isopods the same genus, and even the same species, is often to be found in the most widely separated areas: thus, *Eurycope fragilis*, found in the North Pacific, near Japan, ranges as far south as lat. 60° S., close to the Antarctic ice-barrier and to the neighbourhood of the Crozets. It would also seem that the deep-sea Isopods are distributed very unevenly over the floor of the ocean. In long stretches of ocean—occurring, for instance, along the whole of the Central and Southern Atlantic, and the Central and Western Pacific—no species were found; but in drawing conclusions from such negative evidence, the imperfection of the record must be borne in mind. Among the Isopods thirty-four of the deep-sea species were found to be totally blind, and three others, unfortunately only represented by fragments, may in all probability be added to the list. In four more the eyes were evidently degenerate. On the other hand, in eighteen species, there were well-developed eyes. It must not be forgotten that certain shallow-water species are blind. Possibly, the author thinks, the explanation of these anomalies is to be sought for in the length of time that has elapsed since the migration of the different species into the abyssal regions of the ocean. This excellent Report is illustrated by twenty-five plates.

The second memoir in this volume is a Report on the Brachyura, by Mr. Edward J. Miers. We learn from the very modest preface to this really important contribution to the natural history of the brachyurous Crustacea that the groups richest in new genera and species were the Oxyrhyncha (Maioidea) and the Oxystomata (Leucosoidea), and to these belong most of the new forms collected at depths exceeding 100 fathoms. No brachyurous crab was found at a depth exceeding 2000 fathoms and but very few at depths exceeding 500 fathoms. The localities furnishing the greatest proportion of new or interesting forms were the stations at, among, or near the islands of the Malaysian archipelago, and at the Fijis. An atlas of twenty-nine plates accompanies the Report.

The third Report is by the late Mr. George Busk, F.R.S., and is on the Polyzoa, being Part 2, treating of the Cyclostomata, the Ctenostomata, and the Pedicellinea. With this memoir we propose to deal in a separate article.

THE ELEMENTS OF ECONOMICS.

The Elements of Economics. By Henry Dunning Macleod, M.A. Two Vols. (London: Longmans, 1886.)

WE should have been disposed to speak more kindly of this work upon a much troubled subject—a subject, nevertheless, affecting the happiness of the whole of human society—if we had not read in the letter of dedication that the author claimed to offer to his Right Reverend patron "a new inductive science; a new body of phenomena brought under the dominion of mathematics; a new order of variable quantities brought under the theory of variable quantities in general: the great science of analytical economics." Since recently it was an accepted theory among its students that at present there is no "science" of political economy, we were prepared to find a new revolution-working theory of the whole subject, and were surprised to find that the title sufficiently expressed the general contents of the book. The writer goes to the very elements of the science, building up from the beginning in the clearest of language, and illustrating by means of the derivation of words and of many legal phrases and customs, both ancient and modern, English and foreign, "the great science of economics." Mr. Macleod fiercely assails the opinions of Ricardo and other writers, but much of the error he attacks is only apparent. In economic language, there is no such thing, perhaps, as intrinsic value; nor does cost in all cases fix value, as every proprietor of superseded machinery, or of goods gone out of fashion, knows too well; yet it is misleading to teach that the fixing of value by cost may not be accepted as a fair working rule. In the ordinary state of any trade, 95 per cent., say, of the price will be fixed by the cost, and 5 per cent. by the state of the market; and if some such proportions as these are exceeded, increased or decreased production will soon restore them.

Mr. Macleod urges the claims of perpetual copyright, but we cannot see his distinction between property in that and in a patent. If patents are inconvenient for those whom they restrain, copyright is also inconvenient to men wanting cheap literature. An excellent argument to show how much (market) "value" arises from demand is drawn from this commodity: "Writers of the most learned works do not earn the wages of a day-labourer, whereas the writers of trashy and ephemeral novels may earn a fortune."

The practical remarks upon supply and demand and the folly of subsidising a trade so as to increase the supply where it is already too great for the demand (Vol. II. p. 213), and those upon the errors of the Socialists (p. 215), are good; and the old balance-of-trade error is fully exposed. But we do not see the mistake in the quotation from Mill (p. 76): a banker's credit would be small if he had not capital; and surely it is worse than a paradox to say that the policies of an Insurance Company are its capital; as well might the name be given to the money owing for raw material by a manufacturer!

In an attack upon writers of well-known ability, such as we have here, a critic should be careful of his own expressions. On p. 181, Vol. I., in lines 8 and 9 from the bottom, there is a reversal of the important words "debtor" and "creditor," which, though corrected in the next paragraph, adds to the puzzledom of the whole passage. Again, on p. 200, even after carefully considering the meaning of "purchaser," "consumer," the author arrives at the conclusion that "the consumer is simply the purchaser or customer," whereas the consumer is the purchaser who does not intend to sell again. If an architect builds a palace (to take Mr. Macleod's first example) to carry out some grand idea of his own which he feels sure will attract him a royal customer for it, but lack of funds compels him to sell it unfinished to a commercial company who have a similar faith in his design, they are not consumers, because they intend to sell it again. But if a monarch retired from business buys it of them for his residence, he is the consumer, because, although the palace may stand for centuries, he does not intend to sell it again. To take a much more familiar case, going on under our own eyes: a builder erects a row of villas as a speculation of his own; as long as he has them on his hands they are stock in the market, but as one purchaser is found who elects to inhabit one, and another to inhabit another, those houses are, as far as economics is concerned, "consumed," and the builder is encouraged to produce more.

Far more careful printing is required in such a book. On p. 309, Vol. II., line 1 is quite unintelligible through the misplacing of two commas. On p. 156, no doubt the "division of labour" should be the "division of employment" with combination of labour. For the sake of clearness (we suppose) qualifications have been sacrificed in many places, with, we feel sure, mischievous effect to any student inquiring into the "elements" of so intricate a science.

OUR BOOK SHELF.

Outlines of Lectures on Physiology. By T. Wesley Mills. (Montreal: Drysdale and Co., 1886.)

THIS little work of scarcely 200 pages gives at a glance very precise information as to the kind of instruction provided in the Physiological Department of the McGill University.

The teaching appears to be both scientific and practical in its character, and of a standard certainly equal to that of the teaching in many of our English schools. Prof. Mills most properly insists on the importance of comparative physiology and biology, the only keys to many of the most complicated problems in human physiology itself. It is, however, unfortunate that he is obliged to incorporate so much elementary biology in his lectures, suggesting, as it does, that this important subject is, in Canada as well as in England, often relegated to the teachers of physiology, who should be in a position to begin with students already acquainted with the fundamental facts of this science. Pathology, or the application of physiology to disease, is hardly touched upon in this book. It is a most unfortunate omission, unless both pathology and therapeutics are taught in other departments of the University far more systematically than with us. From the fact that it is so sketchy it is difficult to understand how Dr. Mills' work can be of any value to the general reader who is not at the same time interested in the progress of medical education, or to the ordinary student of physiology. Under "Saliva" (page 86), which may be taken as an example, we find the following headings without any explanatory text. "Mixed saliva found in the mouth. Secretion of *serous* and mucous glands compared. Morphological elements of saliva. Chemical constitution," &c. The work professes, however, to be only an outline, and such it is.

Chemistry for Beginners. By R. L. Taylor. (London: Sampson Low and Co., 1887.)

THIS little book is valuable as being the outcome of practical experience in the teaching of the first principles of chemistry, and, from its small size and simple statement, is likely to be much used in the sphere for which it is intended. It appears eminently suited for the use of pupils in our higher grade Board schools, where the author has gained most of his experience, and may with advantage be used as an elementary class-book, especially as it contains a graduated series of original problems. We are glad to notice the introduction of an undoubtedly beneficial method of representing chemical reactions, which, especially in more complex cases, expresses what really happens in a very clear light. An example extracted from Mr. Taylor's book is as follows:—

$$\begin{array}{r|l} K & NO_3 \\ SO_4H & H. \end{array}$$

Of course, the equation written in the ordinary form is given, as is proper, side by side with the above.

Although it is unfortunate that the illustrations are of so primitive a character, the book is very readable and likely to interest beginners, and the author may be congratulated upon the absence of all appearance of cram, which has such a paralyzing influence upon the thinking powers of those from amongst whom our future chemists are to be derived. A. E. T.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Thought without Words.

THE recent work of Prof. Max Müller contains theories on the descent of man which are entirely based on the assertion that not even the most rudimentary processes of true thought can be carried on without words. From this he argues that as man is the only truly speaking animal the constitution of his mind is separated from that of brutes by a wide gulf, which no process of evolution that advanced by small steps could possibly stride over. Now, if a single instance can be substantiated of a man thinking without words, all this anthropological theory, which includes the more ambitious part of his work, will necessarily collapse.

I maintain that such instances exist, and the first that I shall mention, and which I will describe at length, is my own. Let me say that I am accustomed to introspection, and have practised it seriously, and that what I state now is not random talk but the result of frequent observation. It happens that I take pleasure in mechanical contrivances; the simpler of these are thought out by me absolutely without the use of any mental words. Suppose something does not fit; I examine it, go to my tools, pick out the right ones, and set to work and repair the defect, often without a single word crossing my mind. I can easily go through such a process in imagination, and inhibit any mental word from presenting itself. It is well known at billiards that some persons play much more "with their heads" than others. I am but an indifferent player; still, when I do play, I think out the best stroke as well as I can, but not in words. I hold the cue with nascent and anticipatory gesture, and follow the probable course of the ball from cushion to cushion with my eye before I make the stroke, but I say nothing whatever to myself. At chess, which I also play indifferently, I usually caculate my moves, but not more than one or two stages ahead, by eye alone.

Formerly I practised fencing, in which, as in billiards, the "head" counts for much. Though I do not fence now, I can mentally place myself in a fencing position, and then I am intent and mentally mute. I do not see how I could have used mental words, because they take me as long to form as it does to speak or to hear them, and much longer than it takes to read them by eye (which I never do in imagination). There is no time in fencing for such a process. Again, I have many recollections of scrambles in wild places, one of which is still vivid, of crossing a broad torrent from stone to stone, over some of which the angry-looking water was washing. I was intellectually wearied when I got to the other side, from the constant care and intentness with which it had been necessary to exercise the judgment. During the crossing, I am sure, for similar reasons to those already given, that I was mentally mute. It may be objected that no true thought is exercised in the act of picking one's way, as a goat could do that, and much better than a man. I grant this as regards the goat, but deny the inference, because picking the way under difficult conditions does, I am convinced, greatly strain the attention and judgment. In simple algebra, I never use mental words. Latterly, for example, I had some common arithmetic series to sum, and worked them out, not by the use of the formula, but by the process through which the formula is calculated, and that without the necessity of any mental word. Let us suppose the question was, how many strokes were struck by a clock in twelve hours (not counting the half-hours), then I should have written 1, 2 . . . ; and below it, 12, 11, . . . ; then 2 . . . . 13×12, then $13 \times 6 = 78$. Addition, as De Morgan somewhere insisted, is far more swiftly done by the eye alone: the tendency to use mental words should be withstood. In simple geometry I always work with actual or mental lines; in fact, I fail to arrive at the full conviction that a problem is fairly taken in by me, unless I have contrived somehow to disembarrass it of words.

Prof. Max Müller says that no one can think of a dog without mentally using the word dog, or its equivalent in some other language, and he offers this as a crucial test of the truth of his theory. It utterly fails with me. On thinking of a dog, the name at once disappears, and I find myself mentally in that same expectant attitude in which I should be if I were told that a dog was in an obscure part of the room or just coming round the corner. I have no clear visual image of a dog, but the sense of an ill-defined spot that might shape itself into any specified form of dog, and that might jump, fawn, snarl, bark, or do anything else that a dog might do, but nothing else. I address myself in preparation for any act of the sort, just as when standing before an antagonist in fencing I am ready to meet any thrust or feint, but exclude from my anticipation every movement that falls without the province of fair fencing.

He gives another test of a more advanced mental process, namely, that of thinking of the phrase "cogito ergo sum" without words. I addressed myself to the task at a time when I was not in a mood for introspection, and was bungling over it when I insensibly lapsed into thinking, not for the first time, whether the statement was true. After a little, I surprised myself hard at thought in my usual way—that is, without a word passing through my mind. I was alternately placing myself mentally in the attitude of thinking, and then in that of being, and of watching how much was common to the two processes.

It is a serious drawback to me in writing, and still more in explaining myself, that I do not so easily think in words as otherwise. It often happens that after being hard at work, and having arrived at results that are perfectly clear and satisfactory to myself, when I try to express them in language I feel that I must begin by putting myself upon quite another intellectual plane. I have to translate my thoughts into a language that does not run very evenly with them. I therefore waste a vast deal of time in seeking for appropriate words and phrases, and am conscious, when required to speak on a sudden, of being often very obscure through mere verbal maladroitness, and not through want of clearness of perception. This is one of the small annoyances of my life. I may add that often while engaged in thinking out something I catch an accompaniment of nonsense words, just as the notes of a song might accompany thought. Also, that *after* I have made a mental step, the appropriate word frequently follows as an echo; as a rule, it does not accompany it.

Lastly, I frequently employ nonsense words as temporary symbols, as the logical x and y of ordinary thought, which is a practice that, as may well be conceived, does not conduce to clearness of exposition. So much for my own experiences, which I hold to be fatal to that claim of an invariable dependence between thoughts and words which Prof. Max Müller postulates as the ground of his anthropological theories.

As regards the habits of others, at the time when I was inquiring into the statistics of mental imagery, I obtained some answers to the following effect: "I depend so much upon mental pictures that I think if I were to lose the power of seeing them I should not be able to think at all." There is an admirable little book published last year or the year before by Binet, "Sur le Raisonnement," which is clear and solid, and deserves careful reading two or three times over. It contains pathological cases in which the very contingency of losing the power of seeing mental pictures just alluded to has taken place. The book shows the important part played by visual and motile as well as audile imaginations in the act of reasoning. This and much recent literature on the subject seems wholly unknown to Prof. Max Müller, who has fallen into the common error of writers not long since, but which I hoped had now become obsolete, of believing that the minds of everyone else are like one's own. His aptitudes and linguistic pursuits are likely to render him peculiarly dependent on words, and the other literary philosophers whom he quotes in partial confirmation of his extreme views are likely for the same cause, but in a less degree, to have been similarly dependent. Before a just knowledge can be attained concerning any faculty of the human race we must inquire into its distribution among all sorts and conditions of men, and on a large scale, and not among those persons alone who belong to a highly specialized literary class.

I have inquired myself so far as opportunities admitted, and arrived at a result that contradicts the fundamental proposition in the book before us, having ascertained, to my own satisfaction at least, that in a relatively small number of persons true thought is habitually carried on without the use of mental or spoken words. FRANCIS GALTON.

Tabasheer mentioned in Older Botanical Works.

IN recent issues of NATURE (pp. 396 and 488) Mr. Thiselton Dyer and Mr. Judd have made two interesting contributions to the knowledge of "tabasheer," and Mr. Tokutaro Ito, and others, have supplied remarkable additional notes (pp. 462, 437, &c.). But no one has told us what is to be found about so interesting a substance in the older botanical works. In numerous botanical works of the præ-Linnean period, "tabaxir," as it was called by all authors of that time, is mentioned, and some of them give us very good information about it.

The first who wrote on tabasheer seems to have been Al-Hussain Abu-Ali Ebn Sina, or Avicenna, as he is generally called by Eastern literary men, a celebrated physician and minister of the Persian Empire, who lived from 980 till 1037, and whose works, written in Arabic, obtained as early as the twelfth century a very great reputation. Avicenna introduced the Persian word tabaxir, طباشير, into the Arabian language; it signifies "condensed milk-sap," or as Ray (Raius) translates it (1688) in his "Historia Plantarum," *lac lapidescens*. Avicenna was not well instructed about the origin of tabasheer, for in lib. ii. cap. 609, he says that it is got "ex radicibus arundinum crematis," and by these words he created an erroneous opinion, which lasted several centuries. For Gerardus of Cremona, who in the twelfth century translated the work of Avicenna into Latin, was induced by this suggestion to identify the Indian tabasheer with the *σποδός* of the Greeks or the Arabian "tutia," because this remedy was also got by burning the roots of a certain plant, which was probably a Lawsonia.[1]

This error was corrected by Garcia de Orta, the physician of a viceroy of India, who wrote a book "De Plantis et Aromatibus," in Portuguese, which was translated into Latin by De l'Ecluse (Clusius) in his "Exoticarum Libri Decem," and whose information is the best I have found in writers of that time. He says:—"Vocatur autem ab indigenis 'Sacar Mambu,' quasi dicas Saccharum de Mambu, quoniam Indi arundines, sive ramos arboris illud proferentes Mambu vocant. Attamen nunc etiam *Tabaxir* vocare coeperunt, quoniam eo nomine petitur ab Arabibus, Persis, et Turcis, qui id mercimonii causa ex India in suas regiones exportant. Magno emitur hoc medicamentum pro proventus eius ratione. *Eius tamen commune pretium in Arabia est, ut pari argenti pondere ematur.* Arbor in qua gignitur interdum magna est et instar Populi procera: Inter singula internodia liquor quidam dulcis generatur, crassus veluti

[1] Afterwards, the signification of the word "spodium," or "spodos," must have totally changed, for Matthiolius and others make it a mixture of metals, probably containing zinc.

amylum congestum et simili candore, interdum multus, nonnunquam vero perpaucus. Sed non omnes arundines sive rami eum humorem continent, at ii dumtaxat quos Bisnager, Batecala et pars Provinciae Malavar profert. Hic autem liquor concretus interdum nigricans et cinereus invenitur, sed non ideo improbatur. Nam aut ob nimiam humiditatem, aut quod diutius ligno inclusus permanserit, hunc sibi colorem conciliat: non autem ob arborum incendium, veluti nonnulli putarunt. Siquidem in multis ramis, quos non contigit ignis, niger etiam invenitur."

Garcia also gives information as to the medical virtues of tabasheer, which were esteemed to be very important at that time: "Ceterum ex Medicorum tum Indorum, tum Arabum, Persarum, et Turcorum testimonio Tabaxir internis et externis convenit ardoribus, tum etiam biliosis febribus et dysenteriis: praesertim autem in biliosis fluxionibus utuntur; nostri vero trochiscos ex eo conficiunt addito semine Oxalidis."

Almost all præ-Linnean writers who mentioned tabasheer for the most part only reproduced what they found in Garcia's book. Joh. Bauhinus even identifies the name of tabasheer with the plant itself, which he calls in his "Historia Plantarum," i. 2, p. 222, "Tabaxir sive Mambu arbor, Tabaxir folio oleae." Rumphius in his "Herbarium Amboinense," of the year 1763, relates that he had never found any trace of tabasheer in the bamboos growing in the Molucca Islands, except on one occasion:—"Juniores arundines plerumque in inferioribus suis nodis semi-repletae utcunque sunt lympida aqua potabili, quae hisce in terris sensim evanescit, in aliis vero regionibus exsiccatur in substantiam albam et calceam, quae Tabaxir vocatur. Illud tantum addere debeo, mihi in Hituae ora moranti semel adductum fuisse per pueros meos substantiam albam, siccamque instar farinae amyli, quae in illa ora ab illis fuerat detecta, quantum recordor, in Bulu seru (*Beesha humilis*, Kunth, *Bambusa Fax*, Poir.) fistulis, dura autem erat, sicca, ac penitus insipida omnibusque Aethiopibus, quibus ostendebam, ignota, ipsiusque albido sensim in cinereum degenerabat colorem."

Further information on tabasheer is given by Piso, a well-instructed Dutch physician, in the year 1658, but I am inclined to think that he is wrong in identifying "Achar," a sweet dish celebrated in India as well as in Europe at that time, with the tabaxir of the Eastern peoples. But let us hear what he says:—"Novissimi autem stolones, qui maxime succulenti sunt et saprosi magni fiunt in Indiis, apud Advenas aeque ac Indigenas, quod bases sunt celebris istius compositionis 'Achar' dictae, quae in Europam invecta in deliciis habetur palutum doctis, et a me quoque non semel cum voluptate gustata est. At vero ubi hae Arundines procerae et annosae factae fuerint, liquores contenti substantia, color, sapor, et efficacia mutantur, atque paullatim protruditur foras et iuxta internodium vi Solis coagulatur ac instar pumicis albi indurescit, mox nativae suavitatis expers facta, peculiarem saporem cum parva adstrictione, eboris usti aemulum acquirit, vocaturque apud indigenas 'Sacar Mambu'—Tabaxir Garciae et Acostae—qui quo levior, albicantior ac glabrior eo praestantior: quo magis inaequalis atque cinerei coloris evadit, deterior habetur."

Though it is very probable that under certain circumstances almost all species of the genus Bambusa and its allies are able to produce tabasheer, but few are specially mentioned as capable of producing this interesting substance. All the species that I have found noted in the literature of earlier and modern times are the following: (1) *Bambusa arundinacea*, Retz, or the common bamboo; (2) *Bambusa spinosa*, Roxb., which is called by Burmann, 1737, in his "Thesaurus Zeylanicus," "Arundo indica arborea maxima cortice spinosa Tabaxir fundens"; (3) *Beesha humilis*, Kunth, which is Rumph's bamboo mentioned above; (4) *Beesha Rheadii*, Kunth, and (5) *Guadua angustifolia*, Kunth, the species from which Humboldt's specimen of tabasheer was taken. ERNST HUTH.

Frankfurt, Oder, April 17.

A Brilliant Meteor.

I SAW here this evening a splendid meteor; time, by London and North-Western Railway, 8.19. Its apparent point of origin was nearly south, and altitude 45° from the zenith; its path from east to west; finishing about west-south-west, some 30° from the horizon; duration at least four seconds. It increased in brilliancy until near extinction, when it quickly faded in a dull red glow, like that of the residuum from the fire-ball of a rocket. The head, of an apparent brilliancy three times that of a star of the first magnitude, had precisely the appearance of the incandescent spot of the oxy-hydrogen light, and the tail, very long, exhibited a red glow. Some neighbouring trees and the chimney of a house enabled me with a pocket compass to get the altitude and bearings approximately. ARTHUR NICOLS.

Watford, May 8.

THE following particulars relating to a very fine meteor may be of service in fixing its course if it was seen elsewhere:—

(1) Position of observation: the open space in front of St. Anne's, Soho.

(2) Size: three or four times > Venus.

(3) Colour: decidedly green.

(4) Path: it was first seen somewhere near γ Geminorum, and in two or three seconds disappeared slowly behind the houses in the direction of Aldebaran.

(5) Time of disappearance: 20h. 22m. 19s. May 8.

The time can be relied upon, as my watch was compared on Saturday and again this morning with G.M.T.

Saturn was just visible, and Venus, therefore, must have been very bright, yet she seemed quite dull and yellow by the side of the splendid fireball. MAURES HORNER.

28 Upper Montagu Street, W., May 9.

P.S.—γ Geminorum was of course invisible, and Aldebaran behind the houses.

ON Sunday, the 8th inst., at 8.23 p.m., a very brilliant meteor was seen here by a party of four persons, of whom I was one. When I first saw it, it was almost in the zenith, and appeared considerably larger and more luminous than Venus (which had been visible for some time), though of much the same colour. It crossed the sky in a north-westerly direction, and became invisible about 17° above the horizon. As it travelled, a brilliant trail of red light appeared behind it, which increased in length and brightness as it descended, being fully three times longer than the head, when it attained its greatest length.

The meteor was one of striking brilliance, and must have been specially so, as the sky which it crossed was still bright with the yellow glow of sunset. ISABEL FRY.

5 The Grove, Highgate, May 10.

Residual Affinity.

I WAS greatly interested in Prof. Armstrong's recent articles on "Residual Affinity," as it is a subject I brought before the Royal Society of Edinburgh fully nine years ago, as one of the main causes of solution, molecular compounds, &c. I was, however, somewhat disappointed with the conclusions he came to, and was tempted to exclaim in Scriptural language, "Ye did run well; what did hinder you that you are again entangled in the yoke of *bondage*?" Prof. Armstrong comes to the conclusion that HCl and NH_3 combine owing to the residual affinity of Cl for N. Now how can this be? If we regard it from a thermal point of view, we find that, in the combination of HCl with NH_3, 41,900 units of heat are given out, while the combinations H with Cl and N with H_3 give out 22,000 and 11,890 units respectively; that is, the residual affinity of N for Cl, as measured by heat, exceeds by about one-third the sum of the affinities of H for Cl and H_3 for N; and yet, under ordinary circumstances, Cl has very little affinity for N. Is it not more rational to conclude that the residual affinity is not confined to the negative elements, but extends to both, and that the combination of HCl and NH_3 is due mainly to the residual affinity of Cl and N for H? It is easy to understand that this residual affinity is so lowered in intensity that neither Cl nor N can retain unassisted more than one and three atoms; but when the energy of the H is reduced by combination with another body, each of them can then act upon it. That residual affinity exists in both positive and negative elements seems to me evident from the fact that the heats of solution of salts in water vary directly as the affinity of the metal for the O of the water and also directly as the affinity of the negative element for the H, as I have pointed out in my letter on "Laws of Solution," in NATURE, vol. xxxiv. p. 263. It seems strange to me that chemists will search out for occult causes of phenomena which can much more easily be explained by what is already known of the actions of one element on another rather than abandon the assumption that chemical affinity acts in definite units.

Portobello, April 28. WM. DURHAM.

[Without discussing the general question, I may point out that unfortunately we are at present unable to base any argument on the thermal behaviour of elements, as the fundamental values are entirely unknown: we do not know, for example, what amount of heat would be given out on combination of H and Cl; the value deduced for H_2,Cl_2 by Thomsen being the algebraic sum of several values, some of which are negative, some positive.—H. E. ARMSTRONG.]

The Spherical Integrator.

I FIND that my name has been alluded to in a letter by Prof. Hele Shaw, in your last number (vol. xxxv. p. 581).

I shall be glad if you will kindly permit me to state that the idea of reducing the moment of inertia of the sphere in a spherical integrator, by making it hollow, occurred to me while abroad in Algeria. An account of the modified form is given in the *Phil. Mag.*, August 1886, p. 147. I now find, from a letter from Prof. Shaw, of this month, that exactly the same method of dealing with the difficulty had occurred to him. At the end of Prof. Shaw's letter in your last issue the following words are used: "Now in the 'sine' form, of which this integrator is an example, this pin should move in the arc of a circle, and it would be interesting to know if approximately correct results have been obtained with what is in some respects a more convenient device." From this it would appear that the principle of the instrument is not correct. This morning I received a post-card from Prof. Shaw in which he writes that he had misunderstood the diagrammatic outline in the *Phil. Mag.* His words are: "You are quite right as you use it; I was thinking of a contrivance in which the sphere and frame move together." With respect to M. Ventosa's letter on the subject, in your paper of a month ago, (p. 513) in which he speaks very favourably of the method of using a hollow sphere, although M. Ventosa used a spherical integrator in a certain form of anemometer at an early date, yet I think that all who have seen and read Prof. Shaw's work will admit that he has expanded the use of the spherical integrator and its mathematical importance in a way which is both masterly and original. FREDK. SMITH.

28 Norham Gardens, Oxford, April 25.

THE HENRY DRAPER MEMORIAL.[1]

DR. HENRY DRAPER, in 1872, was the first to photograph the lines of a stellar spectrum. His investigation, pursued for many years with great skill and ingenuity, was most unfortunately interrupted in 1882 by his death. The recent advances in dry-plate photography have vastly increased our powers of dealing with this subject. Early in 1886, accordingly, Mrs. Draper made a liberal provision for carrying on this investigation at the Harvard College Observatory, as a memorial to her husband. The results attained are described below, and show that an opportunity is open for a very important and extensive investigation in this branch of astronomical physics. Mrs. Draper has accordingly decided greatly to extend the original plan of work, and to have it conducted on a scale suited to its importance. The attempt will be made to include all portions of the subject, so that the final results shall form a complete discussion of the constitution and conditions of the stars, as revealed by their spectra, so far as present scientific methods permit. It is hoped that a greater advance will thus be made than if the subject was divided among several institutions, or than if a broader range of astronomical study was attempted. It is expected that a station to be established in the southern hemisphere will permit the work to be extended so that a similar method of study may be applied to stars in all parts of the sky. The investigations already undertaken, and described below more in detail, include a catalogue of the spectra of all stars north of $-24°$ of the sixth magnitude and brighter, a more extensive catalogue of spectra of stars brighter than the eighth magnitude, and a detailed study of the spectra of the bright stars. This last will include a classification of the spectra, a determination of the wave-lengths of the lines, a comparison with terrestrial spectra, and an application of the results to the measurement of the approach and recession of the stars. A special photographic investigation will also be undertaken of the spectra of the banded stars, and of the ends of the spectra of the bright stars. The instruments employed are an 8-inch Voigtländer photographic lens re-ground by Alvan Clark and Sons, and Dr. Draper's 11-inch photographic lens, for which Mrs. Draper has provided a new mounting and observatory. The 15-inch refractor belonging to the Harvard College Observatory has also been employed in various experiments with a slit spectroscope, and is again being used as described below. Mrs. Draper has decided to send to Cambridge a 28-inch reflector and its mounting, and a 15-inch mirror, which is one of the most perfect reflectors constructed by Dr. Draper, and with which his photograph of the moon was taken. The first two instruments mentioned above have been kept at work during the first part of every clear night for several months. It is now intended that at least three telescopes shall be used during the whole night, until the work is interrupted by daylight.

The spectra have been produced by placing in front of the telescope a large prism, thus returning to the method originally employed by Fraunhofer in the first study of stellar spectra. Four 15° prisms have been constructed, the three largest having clear apertures of nearly 11 inches, and the fourth being somewhat smaller. The entire weight of these prisms exceeds a hundred pounds, and they fill a brass cubical box a foot on each side. The spectrum of a star formed by this apparatus is extremely narrow when the telescope is driven by clockwork in the usual way. A motion is accordingly given to the telescope slightly differing from that of the earth by means of a secondary clock controlling it electrically. The spectrum is thus spread into a band, having a width proportional to the time of exposure and to the rate of the controlling clock.

This band is generally not uniformly dense. It exhibits lines perpendicular to the refracting edge of the prism, such as are produced in the field of an ordinary spectroscope by particles of dust upon the slit. In the present case, these lines may be due to variations in the transparency of the air during the time of exposure, or to instrumental causes, such as irregular running of the driving clock, or slight changes in the motion of the telescope, resulting from the manner in which its polar axis is supported. These instrumental defects may be too small to be detected in ordinary micrometric or photographic observations, and still sufficient to affect the photographs just described.

A method of enlargement has been tried which gives very satisfactory results, and removes the lines above mentioned as defects in the negatives. A cylindrical lens is placed close to the enlarging lens, with its axis parallel to the length of the spectrum. In the apparatus actually employed, the length of the spectrum, and with it the dispersion, is increased five times, while the breadth is made in all cases about 4 inches. The advantage of this arrangement is, that it greatly reduces the difficulty arising from the feeble light of the star. Until very lately, the spectra in the original negatives were made very narrow, since otherwise the intensity of the starlight would have been insufficient to produce the proper decomposition of the silver particles. The enlargement being made by daylight, the vast amount of energy then available is controlled by the original negative, the action

[1] "First Annual Report of the Photographic Study of Stellar Spectra." Conducted at the Harvard College Observatory." Edward C. Pickering, Director. With Plate. (Cambridge: John Wilson and Son, University Press, 1887.)

of which may be compared to that of a telegraphic relay. The copies therefore represent many hundred times the original energy received from the stars. If care is not taken, the dust and irregularities of the film will give trouble, each foreign particle appearing as a fine spectral line.

Other methods of enlargement have been considered, and some of them tried, with the object of removing the irregularities of the original spectra without introducing new defects. For instance, the sensitive plate may be moved during the enlargement in the direction of the spectral lines; a slit parallel to the lines may be used as the source of light, and the original negative separated by a small interval from the plate used for the copy; or two cylindrical lenses may be used, with their axes perpendicular to each other. In some of these ways the lines due to dust might either be avoided or so much reduced in length as not to resemble the true lines of the spectrum.

The 15-inch refractor is now being used with a modification of the apparatus employed by Dr. Draper in his first experiments,—a slit spectroscope from which the slit has been removed. A concave lens has been substituted for the collimator and slit, and, besides other advantages, a great saving in length is secured by this change. It is proposed to apply this method to the 28-inch reflector thus utilising its great power of gathering light. . . . .

The results to be derived from the large number of photographs already obtained can only be stated after a long series of measurements and a careful reduction and discussion of them. An inspection of the plates, however, shows some points of interest. A photograph of *a* Cygni, taken November 26, 1886, shows that the H line is double, its two components having a difference in wave-length of about one ten-millionth of a millimetre. A photograph of *o* Ceti shows that the lines G and *h* are bright, as are also four of the ultra-violet lines characteristic of spectra of the first type. The H and K lines in this spectrum are dark, showing that they probably do not belong to that series of lines. The star near χ^1 Orionis, discovered by Gore in December 1885, gives a similar spectrum, which affords additional evidence that it is a variable of the same class as *o* Ceti. Spectra of Sirius show a large number of faint lines besides the well-known broad lines.

The dispersion employed in any normal map of the spectrum may be expressed by its scale, that is, by the ratio of the wave-length as represented to the actual wave-length. It will be more convenient to divide these ratios by one million, to avoid the large numbers otherwise involved. If one-millionth of a millimetre is taken as the unit of wave-length, the length of this unit on the map in millimetres will give the same measure of the dispersion as that just described. When the map is not normal, the dispersion of course varies in different parts. It increases rapidly towards the violet end when the spectrum is formed by a prism. Accordingly, in this case the dispersion given will be that of the point whose wave-length is 400. This point lies near the middle of the photographic spectrum when a prism is used, and is not far from the H line. The dispersion may accordingly be found with sufficient accuracy by measuring the interval between the H and K lines, and dividing the result in millimetres by 3·4, since the difference in their wave-lengths equals this quantity. The following examples serve to illustrate the dispersion expressed in this way: Angström, Cornu, 10; Draper, photograph of normal solar spectrum, 3·1 and 5·2; Rowland, 23, 33, and 46; Draper, stellar spectra, 0·16; Huggins, 0·1. Fig. 1, 0·06; Fig. 2, 0·10; Fig. 3, 0·63; Fig. 4, 1·3; Figs. 5 and 6, 6·5.

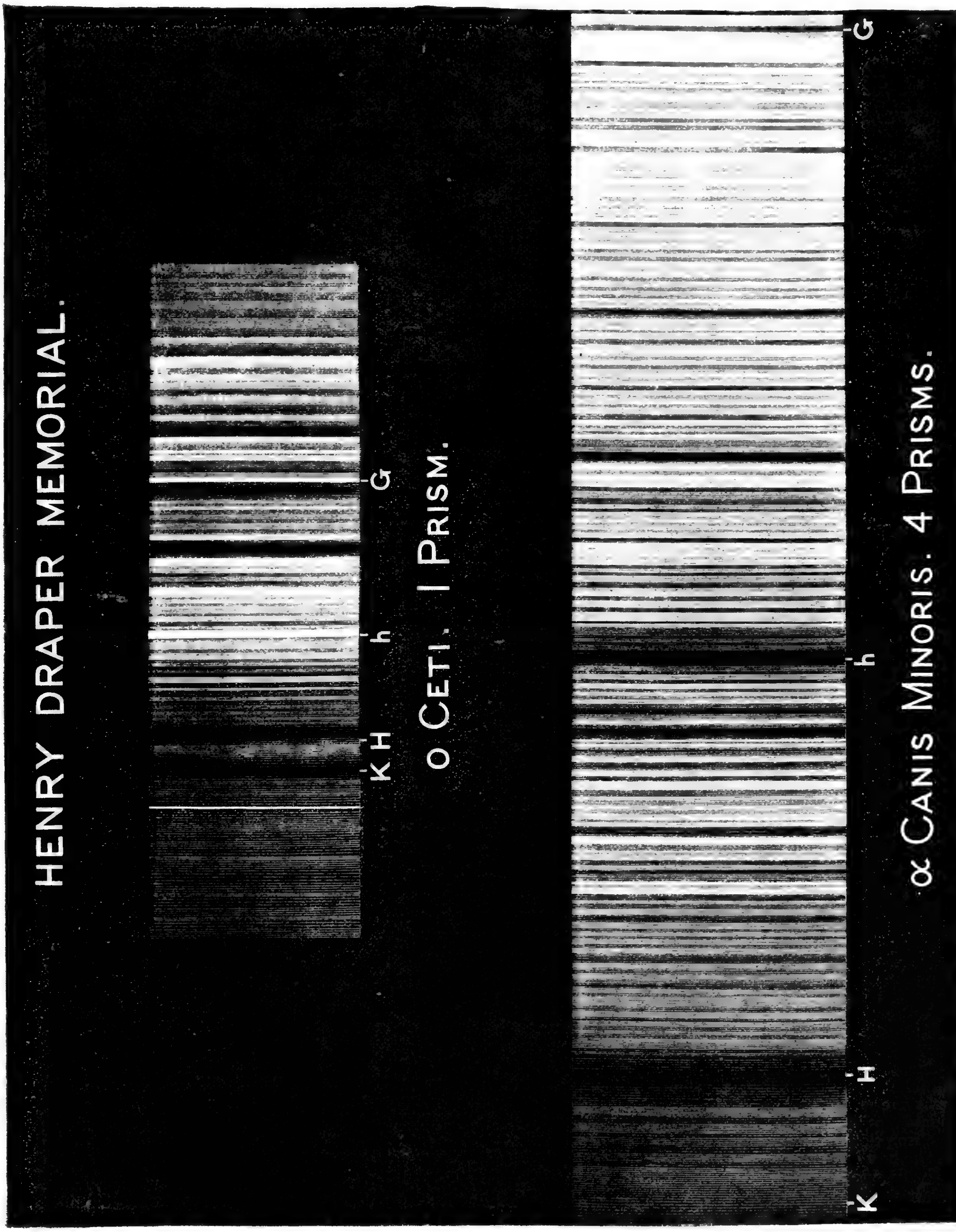
The most rapid plates are needed in this work, other considerations being generally of less importance. Accordingly the Allen and Rowell Extra Quick plates have been used until recently. It was found, however, that they were surpassed by the Seed Plates No. 21, which were accordingly substituted for them early in December. Recognising the importance of supplying this demand for the most sensitive plates possible, the Seed Company have recently succeeded in making still more sensitive plates, which we are now using. The limit does not seem to be reached even yet. Plates could easily be handled if the sensitiveness were increased tenfold. A vast increase in the results may be anticipated with each improvement of the plates in this respect. Apparatus for testing plates, which is believed to be much more accurate than that ordinarily employed, is in course of preparation. It is expected that a very precise determination will be made of the rapidity of the plates employed. Makers of very rapid plates are invited to send specimens for trial.

The photographic work has been done by Mr. W. P. Gerrish, who has also rendered important assistance in other parts of the investigation. He has shown great skill in various experiments which have been tried, and in the use of various novel and delicate instruments. Many of the experimental difficulties could not have been overcome but for the untiring skill and perseverance of Mr. George B. Clark, of the firm of Alvan Clark and Sons, by whom all the large instruments have been constructed.

The progress of the various investigations which are to form a part of this work is given below:—

(1) *Catalogue of Spectra of Bright Stars.*—This is a continuation of the work undertaken with the aid of an appropriation from the Bache Fund, and described in the Memoirs of the American Academy, vol. xi. p. 210. The 8-inch telescope is used, each photograph covering a region 10° square. The exposures for equatorial stars last for five minutes, and the rate of the clock is such that the spectra have a width of about 0·1 cm. The length of the spectra is about 1·2 cm. for the brighter, and 0·6 cm. for the fainter stars. The dispersion on the scale proposed above is 0·1. The spectra of all stars of the sixth magnitude and brighter will generally be found upon these plates, except in the case of red stars. Many fainter blue stars also appear. Three or four exposures are made upon a single plate. The entire sky north of −24° would be covered twice, according to this plan, with 180 plates and 690 exposures. It is found preferable in some cases to make only two exposures; and when the plate appears to be a poor one, the work is repeated. The number of plates is therefore increased. Last summer the plates appeared to be giving poor results. Dust on the prisms seemed to be the explanation of this difficulty. Many regions were re-observed on this account. The first cycle, covering the entire sky from zero to twenty-four hours of right ascension, has been completed. The work will be finished during the coming year by a second cycle of observations, which has already been begun. The first cycle contains 257 plates, all of which have been measured, and a large part of the reduction completed. 8313 spectra have been measured on them, nearly all of which have been identified, and the places of a greater portion of the stars brought forward to the year 1900, and entered in catalogue form. In the second cycle, 64 plates have been taken, and about as many more will be required. 51 plates have been measured and identified, including 2974 spectra. A study of the photographic brightness and distribution of the light in the spectra will also be made.

The results will be published in the form of a catalogue resembling the Photometric Catalogue given in vol. xiv. of the Annals of Harvard College Observatory. It will contain the approximate place of each star for 1900, its designation, the character of the spectrum as derived from each of the plates in which it was photographed,


HENRY DRAPER MEMORIAL.
o CETI. 1 PRISM.
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the references to these plates, and the photographic brightness of the star.

(2) *Catalogue of Spectra of Faint Stars.*—This work resembles the preceding, but is much more extensive. The same instrument is used, but each region has an exposure of an hour, the rate of the clock being such that the width of the spectrum will be as before 0·1 cm. Many stars of the ninth magnitude will thus be included, and nearly all brighter than the eighth. In one case, over three hundred spectra are shown on a single plate. This work has been carried on only in the intervals when the telescope was not needed for other purposes. 99 plates have however been obtained, and on these 4442 spectra have been measured. It is proposed to complete the equatorial zones first, gradually extending the work northward. In all, 15,729 spectra of bright and faint stars have been measured.

(3) *Detailed Study of the Spectra of the Brighter Stars.*—This work has been carried on with the 11-inch photographic telescope used by Dr. Draper in his later researches. A wooden observatory was constructed about 20 feet square. This was surmounted by a dome having a clear diameter of 18 feet on the inside. The dome had a wooden frame, sheathed and covered with canvas. It rested on eight cast-iron wheels, and was easily moved by hand, the power being directly applied. Work was begun upon it in June, and the first observations were made with the telescope in October. Two prisms were formed by splitting a thick plate of glass diagonally. These gave such good results that two others were made in the same way, and the entire battery of four prisms is ordinarily used. The safety and convenience of handling the prisms is greatly increased by placing them in square brass boxes, each of which slides into place like a drawer. Any combination of the prisms may thus be employed. As is usual in such an investigation, a great variety of difficulties have been encountered, and the most important of them have now been overcome.

(4) *Faint Stellar Spectra.*—The 28-inch reflector will be used for the study of the spectra of the faint stars, and also for the fainter portions near the ends of the spectra of the brighter stars. The form of spectroscope mentioned above, in which the collimator and slit are replaced by a concave lens, will be tried. The objects to be examined are, first, the stars known to be variable, with the expectation that some evidence may be afforded of the cause of the variation. The stars whose spectrum is known to be banded, to contain bright lines, or to be peculiar in other respects, will also be examined systematically. Experiments will also be tried with orthochromatic plates and the use of a coloured absorbing medium, in order to photograph the red portions of the spectra of the bright stars. Quartz will also be tried to extend the images towards the ultra-violet.

(5) *Absorption Spectra.*—The ordinary form of comparison spectrum cannot be employed on account of the absence of a slit. The most promising method of determining the wave-lengths of the stellar spectra is to interpose some absorbent medium. Experiments are in progress with hyponitric fumes and other substances. A tank containing one of these materials is interposed, and the spectra photographed through it. The stellar spectra will then be traversed by lines resulting from the absorption of the media thus interposed, and, after their wave-lengths are once determined, they serve as a precise standard to which the stellar lines may be referred. The absorption-lines of the terrestrial atmosphere would form the best standard for this purpose if those which are sufficiently fine can be photographed.

(6) *Wave-Lengths.*—The determination of the wave-lengths of the lines in the stellar spectra will form an important part of the work which has not yet been begun. The approximate wave-lengths can readily be found from a comparison with the solar spectrum, a sufficient number of solar lines being present in most stellar spectra. As a difference of one ten-millionth of a millimetre in wave-length exceeds half a millimetre in Figs. 5 and 6 of the accompanying plate, the readings may be made with considerable accuracy by a simple inspection. For greater precision special precautions are necessary on account of the deviation caused by the approach and recession of the stars. The deviation found by Dr. Huggins in the case of Sirius would correspond to a change in the position of the lines of Figs. 5 and 6 of about half a millimetre. If, then, satisfactory results are obtained in the preceding investigation, the motion of the stars can probably be determined with a high degree of precision. The identification of the lines with those of terrestrial substances will of course form a part of the work, but the details will be considered subsequently.

From the above statement it will be seen that photographic apparatus has been furnished on a scale unequalled elsewhere. But what is more important, Mrs. Draper has not only provided the means for keeping these instruments actively employed, several of them during the whole of every clear night, but also of reducing the results by a considerable force of computers, and of publishing them in a suitable form. A field of work of great extent and promise is open, and there seems to be an opportunity to erect to the name of Dr. Henry Draper a memorial such as heretofore no astronomer has received. One cannot but hope that such an example may be imitated in other departments of astronomy, and that hereafter other names may be commemorated, not by a needless duplication of unsupported observatories, but by the more lasting monuments of useful work accomplished.

EDWARD C. PICKERING,
Director of Harvard College Observatory.

Cambridge, Mass., U.S.A., March 1, 1887.

SCIENCE AND GUNNERY.

I.

IN the last lecture which Prof. Tyndall delivered at the Royal Institution, he expressed a doubt as to whether extensive reading and study had not a tendency to hamper original genius, whether doctrines handed down for generations as articles of faith, which it would be heresy to dispute, had not materially checked the progress of science. Had he wished to illustrate his theory, he could not have had better examples than are to be found in the administration of our naval and military systems. It has been a reproach to us, as by far the greatest maritime nation of the world, that we have no School of Shipbuilding, that, until quite recently, naval officers have had no instruction except such as they could get in the practical execution of their duties, and no method existed of testing their knowledge except such rough-and-ready examinations as their superior officers could administer. Yet under these seeming disadvantages the Navy and the merchant service have kept in the forefront of progress, and have adopted all the newest discoveries of science, or of practical skill, as fast as they have been brought to light.

On the other hand, the officers of Artillery and Engineers have long been considered as belonging to the scientific branches of the service; they have been regularly trained in schools in which theory and history have been taught, and the consequence seems to be that it is most difficult to make the departments with which they are connected move with the times. How else can it be explained that we have adhered to wrought iron as a material for guns, and to muzzle-loaders, long after nations esteemed semi-barbarous have used steel and constructed breech-loaders? or how can we explain the waste of millions in constructing fortifications of patterns long obsolete, and which show no more originality than that exhibited in using in some places iron instead of stone to resist the greater energy of modern projectiles? Not but that there have been many men both in the Artillery and Engineers who have seen the unfitness of what we have been doing, and have energetically protested against it, but they have not had force enough at the War Office to overcome the inertia due to the complacency derived from, perhaps, just pride in a profound knowledge of books.

We do not go quite the length of Dr. Tyndall's opinions, though we admit that there is much truth in them; we recognise the difficulty of teaching in advance, if we may use the expression; but there can be no doubt that precedent and routine have much to answer for, and account for the reluctance of Professors to admit that many of the old methods of fortification and artillery are as dead and useless as the matchlock or the old castle. Besides these considerations derived from experience of the services, we have the fact that most of the original inventions in the construction of guns and carriages have been the work of civil engineers and mechanics, who have been unhampered by precedent and unchecked by authority, and this circumstance must be our apology, as a non-professional paper, for devoting some space to a discussion of the present state of the science of fortification, especially with regard to our own coast defences.

It cannot be disputed, in the first place, that the projectiles delivered by modern guns are distinguished by greatly extended range, by much greater accuracy of flight, by immensely greater weight and destructive power, and by increased rapidity and precision of firing; but on the other hand it must also be admitted that in fighting at long ranges there will be greater waste of ammunition, and that, to put it plainly, excitement and fright go far to neutralise the advantages gained by our improved weapons, and that, consequently, defensive works should be planned so as to give the utmost possible security and sense of safety to the garrison. It is only necessary to study the records of recent naval actions, such as those during the War of Secession in the United States, the bombardment of Alexandria, or fights with dense hordes of savages in the Soudan and elsewhere, to be satisfied of the fact that the amount of destruction caused is small compared with the terrific fire employed. In the case of attack by artillery on shore the results are not so unsatisfactory, the steadiness of the platform, the accurately known range, the immovability of the gun and object fired at, the fact that the best and steadiest soldiers can be selected to aim, and that any nervousness in the gunner does not unsteady the gun, makes the fire of field and siege artillery approach much more nearly to what can be attained in times of peace; but even then, as in the Navy, smartness and rapidity of fire, the descendants of time-honoured drill, aggravated by excitement, are often the cause of a lamentable sacrifice of accuracy.

To make good shooting it is imperative that the men should be reasonably safe, especially against wholesale slaughter such as is caused by the bursting of a shell in a casemate, and this necessity is all the more imperative at the beginning of a war, when most of the soldiers have as yet never heard the shriek of a shell at their ears, or witnessed its terrifying effects. The shooting should be slow and deliberate in order to be effective, the result of each shot should be ascertained, for it must be remembered that the costly charges now fired are no more effective than those of the old smooth-bore artillery unless they reach their destination.

Next, the advantage of long range, accuracy, and rapidity of fire is in a great degree neutralised by the dense volumes of smoke produced by the modern large charges of powder, and although smoke may prove a valuable protection against the accuracy of an enemy's fire, it undoubtedly limits one's own offensive power except under certain conditions to which we will refer again.

In the last place, it may be conceded that an object which you cannot see you cannot aim at; that to be invisible is better than to be protected by armour, and this desirable condition of safety is easily attained in the case of coast defences against ships, because a ship, being always more or less in motion, even when at anchor, can never mark accurately any object of which it can get only an occasional peep. Thus, at the bombardment of Alexandria, one of the undoubted advantages on the side of the defenders was that some of their batteries were not to be distinguished from the irregular features of the rocky coast, and their presence could only be detected by the puffs of smoke from their guns. Even the old-established rules relating to fortifications admit the necessity of concealment; the greatest secrecy is maintained as to the internal economy of forts; access to them cannot be obtained without great difficulty, although we believe that little or nothing is to be gained by such precautions. What should be concealed is the fort itself, and its construction should be of such a nature that the fire of an enemy could not reach the essential mountings and stores it is intended to protect. Even Nature teaches us a lesson in this respect: animals liable to be the prey of others construct their nests of a form and colour and dispose of them so as to be invisible from a short distance, and even the colour of their plumage or their fur is made to assimilate to the tints which surround them; and the tactics they employ when in danger are to lie still so as not to attract attention.

The propositions which we have laid down, and which we do not imagine can be disputed, are of a nature to condemn at once the old systems of fortification, which appear to us to be specially contrived to afford the peculiar advantages which an enemy would desire; nor are alternative and more rational methods wanting, for as far back as May 7, 1869, at the Royal Institution of Great Britain, a paper, describing a new system of coast fortification calculated to meet the changes in artillery and the modern conditions of naval attack, was read by Colonel Moncrieff. In that paper the principle of concealment was laid down, the manner of carrying out the system explained, and the first workable disappearing gun-carriage, which made the realisation of the principles enunciated practicable, was described.

The time for bringing the matter before the public was also opportune, because the loan which had been contracted for strengthening the defences of the country had not all been expended, while the advance in the range and power of artillery was beginning to be fully realised. The authorities, however, were blind to the principles involved; they accepted, indeed, the disappearing gun, but they rejected the system of which it was only a detail. It would have been better had they accepted the system, and rejected the gun-carriage. The consequence of this incredible want of common-sense and discernment has been that a series of misapplications of the methods advocated by Colonel Moncrieff have been perpetrated by the War Office, as, for example, at Milford Haven, Hubberston, Newhaven, Popston, &c. In some of these forts the emplacements for the disappearing guns are actually formed on the top of casemates, crowded into the most conspicuous positions possible.

Those who have had an opportunity of witnessing the trials of guns and their carriages at the Royal Arsenal, must have been struck with the marvellous resistance which a heap of earth opposes to the proof shots fired into it. An insignificant mound stops the heaviest projectiles fired at a few yards' range from the most powerful guns loaded with proof charges, the mound remains uninjured, though daily subjected to blows which would soon wreck any structure made of the most solid materials. The Moncrieff system is specially adapted to take advantage of this stubborn resistance of earth, and that circumstance alone should have commended it to the official mind long years ago, especially as, in addition, the necessarily slight inclination of the slopes affords the farther protection derived from the shot glancing off them.

But even the partial recognition of the principle of concealment, the principle of opposing a bank of earth rather than walls of masonry or iron to the tremendous missiles of the present day, flickered and died out; and the War Office, returning to its evil ways, has, within the last few years, erected at enormous cost batteries made as conspicuous as possible, often more than one story high, and has sought to keep out the fire, which these arrangements are calculated to draw, by clothing the batteries with more iron armour, or protecting the embrasures with stronger iron shields; while to make the work of the enemy more easy, and our defence more difficult, the guns have been massed together in the orthodox style, so that but a portion of them are ever likely to come into action, while the men in the whole battery will be "demoralised"—this, we believe, is the technical expression—by a shell bursting in any one of the convenient funnel-shaped openings considerately presented for their reception.

The smoke, likewise, of so many guns is certain to prove most prejudicial to good shooting, and within the forts themselves are generally placed the barracks, which must necessarily soon be reduced to ruin, either by direct or by curved fire, and thus increase the confusion and loss of life in action. No human being, it seems to us, can with impunity stand the constant strain of such conditions on the nervous system. When off work, the garrison of a fort should be safe, their lodging should be secure, their meals should be eaten in peace and security, and the sick and wounded should not be harassed by noise and turmoil. For these reasons the barracks should be at a distance from the battery, and should be hid away out of the enemy's sight, and connected with the battery by covered or screened ways.

In elevated positions, such as are occupied by some of the forts in the Isle of Wight, the natural features of the ground should have been taken advantage of, so as to render them invisible, the guns mounted in open barbette should be painted such a colour as to render them inconspicuous, instead of the uniform black now adopted, and Nature should be allowed to obliterate as much as possible, by the growth of brushwood and grass, the changes which the Engineers may have been compelled to make in the contour of the country. The Inspecting-General and the public generally, would, no doubt, not be able to gaze with delight on the trim slopes, the regular lines, and the frowning cannon, but ample compensation for this will be found in the circumstance that, in time of need, the enemy would be equally at fault.

Again, in coast defences near to the water, the guns, instead of being concentrated, should be dispersed, each gun should have a wide lateral range, the guns should retire out of sight and of exposure, except for the few moments required to lay and fire them. The emplacements should be connected with each other and with the barracks by screened roads, and bomb-proof rooms should be provided for the use of the men on duty when not required to work the guns. The screened roads, having parapets towards the sea, and towards the land also if necessary, would serve the triple purpose of intrenchments, interior lines of communication, and emplacements for light artillery to repel landings.

It may be urged that such work would prove costly on account of the large area of land required, but that would not be the case. The strips of land for roads or military tramways, and the small plots for emplacements, would be as cheaply obtained as for a railway, and by virtue of similar powers; in most places the cost of land would not be greater than that of armoured structures, the slopes and glacis would be just as available after as before for cultivation, and need not even be purchased, while, if definite plans could at once be adopted for our extensive coasts, a most useful class of work would be available in bad times, such as now press upon us, for the unemployed, and the relief would be widely felt because works are needed all over the kingdom.

The recent experiments at Portland have proved beyond all question that it is next to impossible for a ship at even so small a range as 800 yards, to hit a gun appearing out of a pit for three minutes, when the pit is so arranged, as it is the essential feature of the Moncrieff system that it should be, that its position cannot be detected from the sea. But three minutes is at least six times as long an exposure as is necessary; indeed, the art of determining the exact position of ships approaching coast batteries has been brought to such perfection that the officer in command of each gun would receive from the observing station messages as to the exact position of the enemy, the training and elevation to be given to the gun, the proper moment for raising it into action, and even, by means of electric fuses, the guns may be fired by the observing officer without risk to a single man, and with an exposure of the guns of less duration than the time required for the flight of a projectile at long range. Contrast such arrangements as these with the open barbette battery at Inchkeith, constructed as if on purpose to offer a conspicuous target, and which recent experiments have proved to be correspondingly vulnerable; or with the quite recently constructed turret, mounting a 10-inch gun at Eastbourne, where the projectile of a machine-gun disabled the 27-ton cannon, and one shot from a 6-inch breech-loader knocked off several feet of its barrel.

A careful study of the numerous papers on coast defence read before the United Service Institution, and the discussions, in which many eminent officers of all branches of the service took part, convinces us of the correctness of the views we are maintaining, and the need which exists for laying down organised plans of defence not only for places already protected, but for our long, and in many cases easily accessible, coast-lines. The smoke, which all the speakers agreed in recognising as a great evil in concentrated batteries, would scarcely be any impediment when the guns are scattered, partly because, under most circumstances, the smoke would blow away from each emplacement without obscuring its own gun, or the others, and partly because the observing officer would be above the smoke, and could always make out the enemy.

The smoke itself offers a very feeble indication of the precise locality of the gun which produced it, partly because it is projected a good way from the emplacement at once, partly because the wind in most cases will blow it away to one side or the other. This was fully proved at Portland, when the puffs of smoke sent up as the gun disappeared proved of no assistance to the attack.

But it may be urged, by those unacquainted with the subject, that so formidable a work as raising a heavy gun into the firing position, and checking its recoil and its fall at the same time, would involve cumbersome machinery and the employment of steam or other power. The answer is, that the energy of the discharge itself has been utilised to do all that is required.

The public has been much interested of late in the beautiful mechanism by means of which Mr. Maxim has utilised the energy of recoil, not only to run out the barrel of his gun at every shot, but also to perform all the operations of loading and firing automatically, and that at a rate which almost baffles the imagination. Six hundred shots per minute can be fired without any external power being used. The energy imparted to the shot must have its counterpart in the movement of the gun and carriage in the opposite direction; and Colonel Moncrieff, twenty years ago, showed how, by suitable mechanical arrangements, guns of all sizes could be made to recoil under cover and be raised again into the firing position without the application of external force. There are two systems by which this is accomplished, by means of counterweights and by means of metallic or air springs. In the former case it is easy to see how the counterweight can be so arranged that the work represented by the falling of the gun may be exactly balanced by the work of lifting the balance weight; the energy of recoil, therefore, need only be drawn upon to overcome the friction of the descent and the subsequent friction of ascent, together with the accelerating force necessary to start the gun into smart upward movement. The total amount of work expended in friction does not probably exceed 20 per cent. of the work of raising the gun, and consequently the old muzzle-loaders, with their comparatively small charges and low muzzle velocities of projectile, yield ample power to allow the guns to be lowered completely beyond the reach of hostile shot.

This is a consideration of great importance, because year by year a large number of excellent muzzle-loading guns of all calibres will be returned into store from the Navy, and may at once be utilised for strengthening our coast defences, for they are quite powerful enough to act against unarmoured vessels, light-draught transports, and such like, as well as against the unprotected parts of ironclads; while as howitzers they would be invaluable for preventing landing from boats, and for this service would be quite as effective as the longer, more costly, and more delicately-made breech-loaders, which, however, should be associated with them to resist ironclads. It so happens, also, that the short muzzle-loader is particularly well suited to the Moncrieff carriage, because the men engaged in loading, training, and elevating, working completely under the parapet, are in absolute safety from the enemy's fire, and the only man exposed is he who lays the gun, and even that exposure, as we have already remarked, can often be dispensed with. The muzzle-loaders are also much more simple weapons to manage than the modern, more powerful guns, and would therefore be better fitted for coast batteries, which would undoubtedly have to be manned and worked by Volunteers and men not so highly trained as the Artillery of the regular army.

Some years ago, the War Office definitely adopted the Moncrieff counterweight carriage, and mounted, successfully, guns as large as the 9-inch of 12 tons weight; but after a time evil counsels prevailed, inveterate prejudice triumphed, and the nation has been saddled with a vast expenditure on forts, which are already obsolete, for by no sort of ingenuity can they be made to carry artillery fitted to cope with that which will be opposed to them. Not that the system was ever rightly applied: Colonel Moncrieff, though attached to the War Department for the express purpose of developing his views, does not appear to have been consulted as to the arrangement of his batteries, or, if consulted, his views were ignored, and the consequence is that, in the case of the comparatively few guns which have been mounted, most of the emplacements are made as conspicuous as possible, and in that way the inestimable advantages of concealment have been thrown away.

The counterweight system, however, becomes very cumbersome when guns exceed some 20 tons in weight. Recourse can then be had to compressed air as a means of storing the energy of recoil. But the work done in compressing air reveals itself in the form of heat, which raises its temperature, and is slowly dissipated as it cools. Again, the air, in expanding to raise the gun, is cooled by the amount of heat converted into work, and its pressure is thereby reduced, so that the losses on these two accounts, added to the somewhat increased friction of the machinery, set a limit to the height to which the stored energy of recoil can raise the gun: the increased charges used in modern artillery, however, compensate for these losses, and it is possible by hydro-pneumatic arrangement to give efficient cover to the heaviest guns. The natural fear arises lest the introduction of water and compressed air may not add elements of danger in the facility with which dirt and debris, not sufficient to injure an ordinary mounting, may affect the more complicated arrangement. There is no doubt that a breech-loading gun requires more care in its use than a muzzle-loader, and a hydro-pneumatic mounting is not so simple as a carriage with an ordinary friction or hydraulic compressor, but experience with the 6-inch hydro-pneumatic siege carriage has shown that the system is capable of enduring very rough usage, and is by no means easily deranged.

The Australian colonies, acting under the advice of the late General Scratchley and General Steward, seem to be more intelligent and far-seeing than the mother country, and have acquired a considerable number of breech-loading guns, mounted on the system recommended, and carried out completely in all its details. It is difficult to see how official opposition can long brave the assaults made on it by common-sense, and the glaring defects of the old methods.

(*To be continued.*)

THE TEMPERATURE OF THE CLYDE SEA-AREA.

I.

IN the spring of 1886 a regular system of temperature observations was commenced in the water of the Clyde sea-area, by the staff of the Scottish Marine Station, under the personal superintendence of Mr. John Murray of the *Challenger* Commission. The work has since proceeded steadily, and will probably be continued to the close of the present year. Previous to 1886, few temperature observations had been recorded dealing with the deep water on the west coast of Scotland; these were almost entirely the work of Mr. J. Y. Buchanan on occasional summer cruises.

The scope of the present investigation is limited chiefly by the capabilities of the Marine Station's steam-yacht *Medusa*. She is a vessel of 30 tons, yacht measurement, steaming 6 knots in ordinary circumstances; but not adapted for working amongst the tremendous tidal currents of the North Channel except in the calmest weather. On the other hand, her small size, and the convenient arrangement of a steam-winch for working the sounding-line enables observations to be made with great rapidity in quiet water. Inside of Cantyre, soundings have been obtained in almost every kind of weather, and the present article will deal with this part of the west coast only.

The Clyde sea-area[1] comprises all the connected water-system, 1300 square miles in extent, lying to the north of a line drawn from the Mull of Cantyre to Corsewall Point in Wigtownshire. This line corresponds nearly to the 50-fathom contour; outside it the depth increases rapidly to over 80 fathoms; towards the inner or northern side it diminishes at first, and then remains at about 27 fathoms over an area of 270 square miles. This bank is termed the Clyde Barrier Plateau; it crosses from Cantyre to Ayrshire, past the south end of Arran, and around Ailsa Craig. The shallowest water covers a ridge at a depth of about 20 fathoms from the surface. The water deepens on the inside of the Plateau to form the Arran Basin, which in form resembles the letter λ, surrounding Arran on the west, east, and north, and running up into Lower Loch Fyne. The depth in this basin exceeds 50 fathoms over 100 square miles; the deepest water, 107 fathoms, occurs off Skate Island, near Tarbert. A much smaller depression runs in a straight line from the Cumbraes to Dog Rock at the mouth of Loch Goil. It is known as the Dunoon Basin, and has an average depth of 40 fathoms and a maximum of 56. Of the numerous lochs, reference will be made to two only, Upper Loch Fyne and Loch Goil. The former measures 25 miles from Otter Ferry to the head; it consists of a basin 30 fathoms deep, bounded by channels having an

[1] For detailed description and map see *Scottish Geographical Magazine* for January 1887.

average depth of less than 15 fathoms at Otter Ferry and Minard Narrows, and of a much longer and deeper basin beyond; the maximum depth of the latter (80 fathoms) is found off Strachur. Loch Goil, only 7 miles long and 47 fathoms deep in the centre, is cut off from the Dunoon Basin by a barrier rising to within 10 fathoms of the surface, and is thus exactly similar in its situation to Upper Loch Fyne. The average depth over the whole Clyde sea-area is 31 fathoms, and it contains approximately 150,000 million tons of sea-water. The estuary of the River Clyde is both narrow and extremely shallow, and the river does not appear to affect the Firth to such an extent as the Forth does the firth bearing its name.

The submarine features of the Clyde sea-area are varied and complicated; and this character is shared by the surface of the intervening land, producing a diversity of mountain, glen, and plain, and corresponding effects of sunshine, cloud, and mist, that lend to the temperature cruises a picturesque charm such as rarely invests physical research.

The cruises take place at intervals of about 50 days, and each occupies a little more than a week. Observations are repeated at about sixty stations, distributed over the whole area. The temperature is ascertained at the surface, at 5 and 10 fathoms, and at distances of 10 fathoms down to the bottom. Whenever a considerable difference is noted in the readings of two adjacent thermometers, observations are repeated at close intervals between them, so that when the curve of vertical distribution of temperature is drawn, points are most numerous where they are most wanted, at the regions of change of curvature. All temperature observations are made with Messrs. Negretti and Zambra's patent standard deep-sea thermometers. These are mounted in the Scottish frame, and are reversed by the fall of a brass messenger. Three thermometers are used on the line at once. The readings may be relied upon to one-tenth of a degree Fahrenheit, except when the sea is rough; then the very lively motion of the *Medusa* introduces a little uncertainty, on account of the difficulty of reading. A slight correction for change of volume of the detached column of mercury is necessary when the temperature of the water differs more than 5° F. from that of the air; the air-temperature being observed by the wet-bulb sling-thermometer.

A slip water-bottle is used on the line along with the thermometers, and samples of water are secured from various depths.

The entire set of observations made on the Clyde sea-area, up to November 1886, have been published in the last number of the Scottish Meteorological Society's Journal; and for the purpose of giving a general idea of the main results as yet ascertained, it will suffice to describe the varying seasonal conditions in three typical regions, and then to indicate the general distribution of temperature in the whole area throughout the year.

In the *North Channel*, near the Mull of Cantyre, observations could only be made on five cruises, and of these only two could be extended far enough to reach deep water, that of April 16, when the weather was remarkably fine, and that of September 22, when Mr. Mathieson, of Liverpool, was kind enough to give the use of his large steam-yacht *Oimara* for the purpose. The result of all the observations is shown graphically in Fig. 1. The distribution was always uniform from surface to bottom (except for a variation of not more than 1° in the superficial layer); and, as the accompanying figures show, there was a steady rise of temperature from April to September, while by December there had been a marked fall. It is noticeable that in all cases except December the surface water was a little warmer than that beneath; in December it was a little colder. Temperature:—

April 16	June 19	August 12	September 22	December 25
42°·0	47°·4	52°·5	54°·5	48°·5

The annual range, so far as observations go, appears to be about 12°·5 F. The uniformity of temperature throughout the mass of water continues over the Plateau, but gives place to a slightly different distribution in the deep Arran Basin.

Off *Skate Island* eight observations have been made between March 1886 and February 1887, and the curves presenting their results are given in Fig. 2. The actual figures observed for surface and bottom are:—

	March 27		April 19		June 21		Aug. 10
Surface	41°·4	...	43°·8	...	48°·3	...	53°·6
Bottom	41·5	...	41·3	...	44·0	...	45·6
	Sept. 26		Nov. 16		Dec. 29		Feb. 7
Surface	54°·7	...	49°·3	...	46°·6	...	43°·7
Bottom	47·4	...	51·1	...	47·4	...	44·3

The range of temperature on the surface thus appears to be 13°·3, and on the bottom 9°·8. The maximum surface

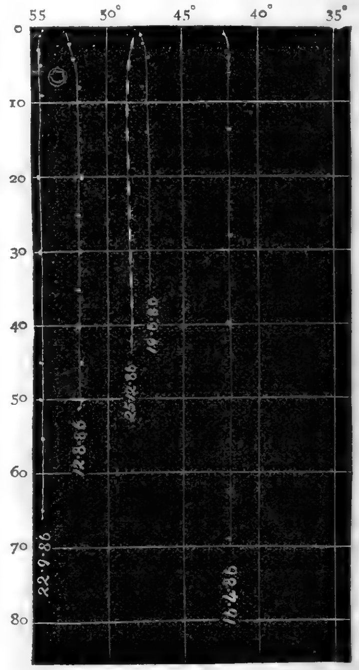


Fig. 1.—Channel.

temperature was observed in September, the maximum bottom temperature in November. The continuous curves (Fig. 2) show the course of heating; the broken lines that of cooling. They illustrate the development of conditions hinted at in the curves for the Channel. Starting with a practically uniform temperature of 41°·4 in March, the water had heated considerably on the surface, and cooled very slightly at the bottom, by April. From that time it warmed throughout, the surface most rapidly, and a mass of water next the bottom was warmed uniformly. The depth of this mass steadily decreased, until in September there was an unbroken gradient of temperature, falling from surface to bottom. By November the surface had chilled considerably; but at 24 fathoms the temperature was the same as in September, and below that depth higher; there being little change from 30 fathoms to the bottom. In succeeding months the fall of temperature has proceeded nearly uniformly, the curve approaching the form of a straight line, gradually becoming more nearly perpendicular. It will be noticed that the curves are not in all cases perfectly regular, but the deviations are so slight that they might almost be attributed to errors of observation, or to the use of slightly erroneous corrections for the thermometers. This is not the true explanation, as the next group of curves illustrates.

Strachur is near the deepest part of Upper Loch Fyne; the water which the depression contains is cut off from communication with the outside by the double doors of Otter and Minard with a shallow hollow between. Eight sets of observations have been made, as follows:—

	April 20	June 21	Aug. 11	Aug. 25
Surface	42°·6	49°·2	54°·1	53°·5
Bottom	41·9	44·1	44·2	44·2

	Sept. 27	Nov. 17	Dec. 29	Feb. 4
Surface	52°·4	46°·4	41°·0	43°·0
Bottom	44·1	44·2	44·7	45·9

The surface range was 13°·1, with a maximum in August;

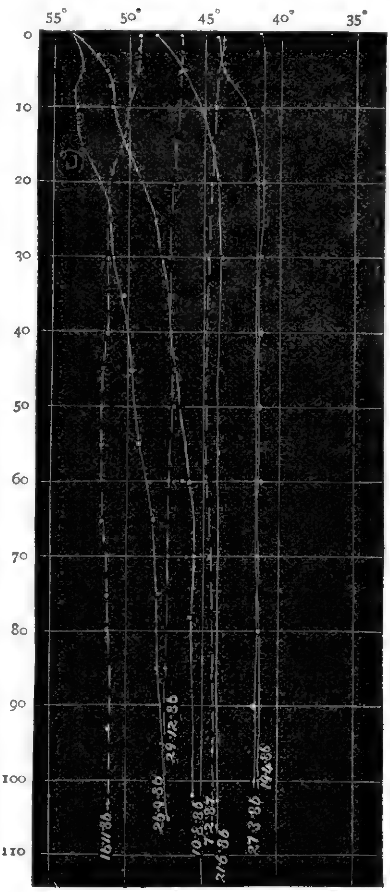


FIG. 2.—Skate Island.

the range of temperature on the bottom has as yet been only 4°; but it is impossible, until further observations have been made, to speak definitely about this. The most remarkable thing apparent from the above figures is that from June to December there should only have been a change of half a degree Fahrenheit in bottom temperature; but an examination of the curves in Fig. 3, will bring out some other curious relations. In April a uniform temperature of 41°·9 was found under 10 fathoms, and this was quite analogous to all the other April observations. In June the surface was found greatly warmed, but at 15 fathoms the temperature was only half a degree higher than it was two months before (42°·5): beneath that point there had been considerable rise of temperature (to 44°·1), so that the phenomenon was presented of a layer of cold water with warmer water above and beneath. It may be mentioned in passing that but for Negretti and Zambra's outflow thermometers this singular distribution could not have been traced out, perhaps not even detected; as, using the deep-sea thermometers on Sixe's principle, the natural induction would have been that below 15 fathoms the temperature was uniform at 42°·5. In August this minimum had almost disappeared, though a trace of it remained at 35 fathoms, the point where the August curve merges with that for June. By September surface cooling had begun, but below 2 fathoms and down to 50 there was

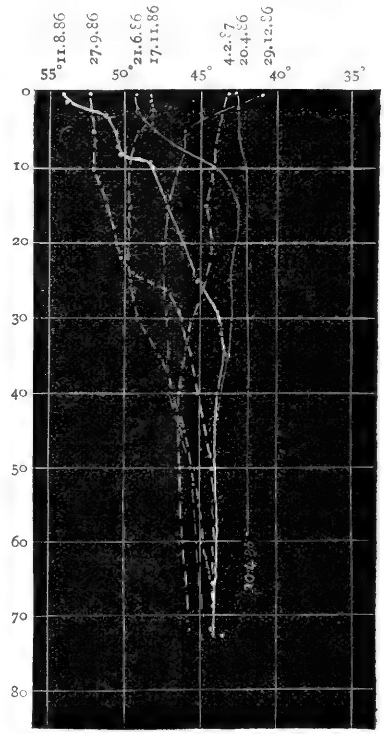


FIG. 3.—Strachur.

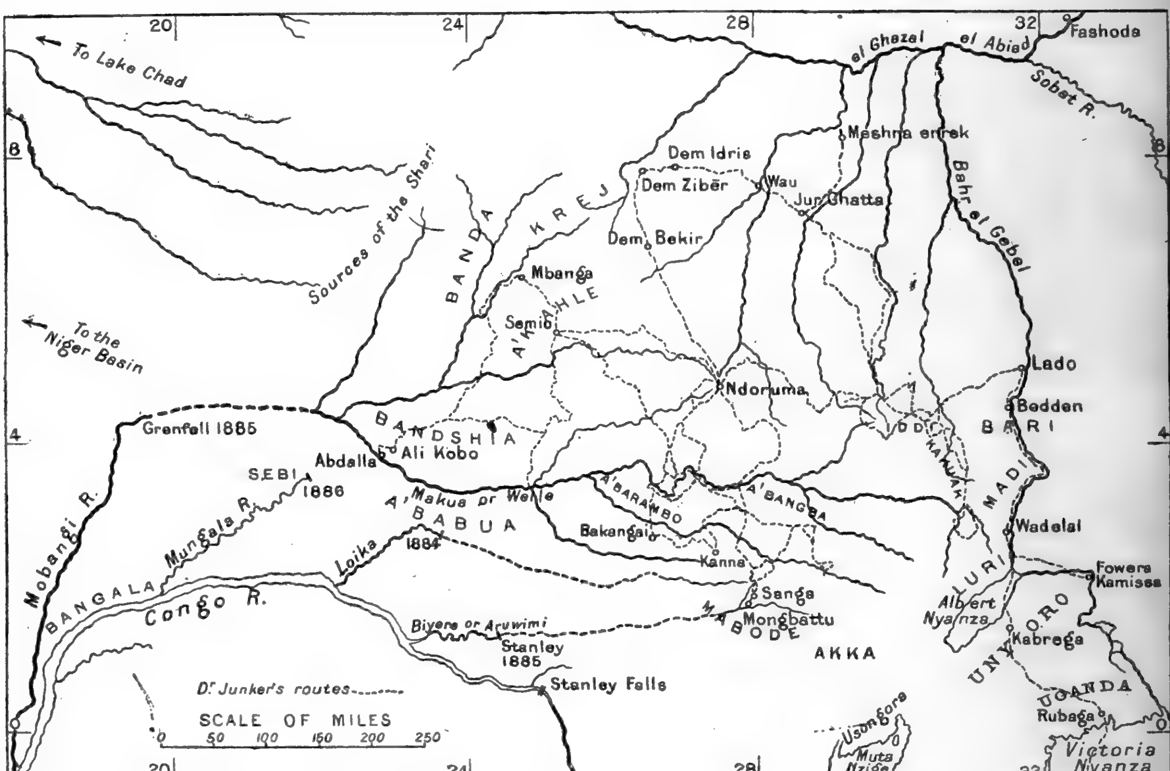
a rise of temperature. At the latter depth the temperature became constant to the bottom at 44°·2 as before. November and December showed the gradual cooling of the surface, and the still more gradual motion downwards of the point of maximum temperature. In December the bottom water had begun to warm, and in February the much attenuated maximum had reached to 45 fathoms, and the remains of summer heat had fairly influenced the bottom temperature. Many more very interesting relations will become apparent from the study of the interlacing curves of Fig. 3, which, with some modifications, are applicable also to Loch Goil, a rock basin "similar and similarly situated" to Loch Fyne.

HUGH ROBERT MILL.

(*To be continued.*)

DR. JUNKER.

NOT since Greely told his story to the Royal Geographical Society has there been so crowded and enthusiastic an audience at Burlington Gardens as assembled on Monday night to welcome Dr. Junker, who, during the last ten years, has done so much good work for geography and science in the important region between the Upper Nile, the Congo, and Lake Chad. Dr. Junker said little of himself and his own work on Monday night; he spoke mainly of the Mahdi rebellion and of his friend Emin Pasha. By the aid of the fine large maps which were shown (one of them drawn by Schweinfurth), and the few details which Dr. Junker did give, the audience obtained a fair idea of the extent and value of his work. Dr. Junker, who was born in 1840, and had an excellent scientific training at St. Petersburg, Göttingen, Berlin, and Prag, first went to Egypt in 1875, and between that and 1879 travelled extensively in the region of the Western Nile tributaries, as far as the Tondi and Wau affluents of the Bahr-el-Ghazal. On Monday night, however, he confined himself to the journeys of the last six years, which have been spent in exploring the Niam-Niam and Monbuttu countries, settling the problem of the course of the Wellé-Makua, and endeavouring to ascertain the watershed that divides the basin of the Congo from that of the Nile and Lake Chad. Dr. Junker's journeys have gone far to solve this problem, and to settle that the Wellé-Makua discharges its waters into the Congo through the Mobangi, which has been explored by Mr. Grenfell. As will be seen from the map, the region traversed by Dr. Junker is a complicated network of rivers, which it will take many journeys to unravel and chart with accuracy. To reach the field of his last six years' exploration, Dr. Junker went up the Bahr-el Ghazal to Djur Ghatta, and hence across the various southern affluents of the Ghazal to Dem-Zebehr, and then southwards to the town of Ndoruma, the powerful prince of the Niam-Niams. Junker's considerate and generous treatment of the natives gained for him a welcome wherever he went. Ndoruma may be said to have been his head-quarters, where he built his houses and planted his gardens, though he himself could not rest there for many weeks;



he was always eager to be on the march into new fields. While he was away exploring, his assistant Bohndorff occupied himself in preparing the natural history specimens collected; and it is sad to think that all this labour has been lost, as, partly owing to a fire and partly owing to the depredations of the Mahdi's people, none of these collections have reached Europe. After being established at Ndoruma, Dr. Junker made a journey of four months to the southwards, to the Monbuttu country, crossing the Wellé at two points. The details as to his discoveries on this and on subsequent journeys he reserves for the book which he hopes to write when he finds leisure. His next journey was in the same direction, to the country of the A-Mahdi, on the Upper Wellé, where he was detained several months. In November 1881, Junker was able to carry out his project of visiting the country of the Bakangai. From this time he was almost constantly on the march, and until June 1882 he was exploring the countries on the south of the Wellé. He made the acquaintance of many peoples whose language, manners, and customs differ essentially from those whom he had previously known. He was well received by the Niam-Niam princes, Bakangai and Kanna, to the south of the large river Bomokandi. Women are very differently treated among the Niam-Niams or A-Sandeh from what they are among the Monbuttus; among the former they are simply slaves, whereas among the latter they are in many respects treated on a footing of equality with the men. The Monbuttu women paint and tattoo their bodies in a most elaborate fashion, which Dr. Junker described in detail. When he left Prince Kanna and the southern A-Sandeh, Junker returned to the Monbuttu country, and spent some days at the station of Tangasi with the Italian traveller Casati. He then traversed the A-Bangba and Momfu countries to the south of the Wellé, crossed again the Bomokandi, and discovered the Nepoko, which he is inclined to identify with the

Aruwimi of Stanley. Here, while detained for months by a Monbuttu chief, A-Sanga, Dr. Junker suffered much from want of proper food and other causes. On the south of the Bomokandi he met with the pygmy people known as Akka or Tikki-Tikki, whom he found clever hunters, leading a nomad life. He was glad to get back to Tangasi to recruit. Crossing the Wellé in a north-west direction, he reached his new station at Semio's in September 1882. Setting out in December, he pushed southwards and westwards, touching the Wellé again at two different points, one of them being his farthest west point on this river.

The remainder of Dr. Junker's paper was occupied with the troubles caused by the Mahdi insurrection to his friends Emin Pasha and Lupton Bey. These troubles prevented Junker himself from proceeding to Europe northwards. He spent much time at Lado, where, under great difficulties, he constructed a large and beautiful map of his explorations. After being with Emin Pasha at Dufilé and Wadelai, and being detained for some time in Unyoro and Uganda, he at last persuaded King Mwanga to let him go. Crossing the Victoria Nyanza, he proceeded by one of the caravan routes to Zanzibar, and thence to Cairo, and so to Europe, where he arrived only a few weeks ago. Besides Dr. Junker's own paper, the only records of his ten years' work are a few letters which appeared at intervals in *Petermann's Mitteilungen*, so that his forthcoming work will abound with novelty. Its scientific value will be unusually great.

NOTES.

THE Royal Society's first *soirée* of the session was held last night. More trouble than usual had been taken to bring interesting things together, and the result was most satisfactory. We shall refer to some of the most striking objects next week.

WE print this week the firstfruits of a new organization for the furthering of astronomical research, which Mrs. Draper has established at the Harvard Observatory in memory of her husband. We do not think that a more noble memorial has ever been suggested to perpetuate the memory of any man, and certainly, if the fair promise of the opening work is kept up, Draper's name will go down to long distant ages. In addition to the first memoir, which we reprint, we have received from Prof. Pickering several enlarged copies of the stellar photographs already obtained. The scale of these photographs and their perfection will be gathered from the illustration which we give, and it does not seem too much to hope that within no very great number of years we shall possess photographs of the different orders of stars, with photographic spark comparisons, in which it may be quite easy to trace the lines due to the absorption of any particular element, and have, in fact, for stars of the various classes an exact equivalent of Ångström's *spectre normal* of the sun with the metallic coincidences. The friends of the late Henry Draper, and they are many in this country and on this side of the Atlantic, will thank his widow for the noble memorial she is erecting to his memory.

THE foundation-stone of the Imperial Institute will be laid by the Queen on Monday, July 4.

THIS afternoon the Croonian Lecture will be delivered before the Royal Society by Prof. H. G. Seeley, F.R.S. The subject is "*Pariesaurus bombidens* (Owen), and the Significance of its Affinities to Amphibians, Reptiles, and Mammals." On Thursday, May 26, the Bakerian Lecture will be delivered before the Royal Society by Prof. J. J. Thomson, F.R.S.

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

THE visitation of the Royal Observatory at Greenwich takes place this year on June 4.

MR. WOODS, the President of the Royal Institution of Civil Engineers, and Miss Woods, have issued cards of invitation to a *conversazione* to be held at the South Kensington Museum on the 25th inst.

THE general meeting of the Institution of Mechanical Engineers will be held on Monday evening, May 16, and Tuesday afternoon, May 17, at 25 Great George Street, Westminster. The President, Mr. Edward H. Carbutt, will deliver his inaugural address on Monday evening. The following papers will be read and discussed, as far as time permits:—"On the Construction of Canadian Locomotives," by Mr. Francis R. F. Brown, Mechanical Superintendent of the Canadian Pacific Railway; "Experiments on the Distribution of Heat in a Stationary Steam-Engine," by Major Thomas English, R.E., of the War Office; and "On Irrigating Machinery on the Pacific Coast," by Mr. John Richards, of San Francisco.

ON Saturday evening next a lecture on "Savages" will be delivered by Sir John Lubbock in the New Schools, Oxford.

THE Deutsche Seewarte at Hamburg has published a chart showing the positions of the icebergs in the North Atlantic, compiled from reports received up to the middle of April. The chart is issued without charge to captains applying for it. As early as the first half of March several icebergs were met with south of 42° N. lat., and one even south of 41°. In drifting southwards, the icebergs, as always, are found between the meridians of 46° and 52° W.

THE fifth Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, gives an account of bowstring hemp. This is not at present an article in commercial use, but Mr. J. G. Baker, the writer of the paper, thinks attention may well be directed to the capabilities of numerous species of *Sansevieria* for producing fibre of great value. Plants of *Sansevieria*, of which there are ten or twelve species, are very abundant on both the east and west coasts of tropical Africa, which, indeed, may be looked upon as the head-quarters of the genus. One well-known species (*S. zeylanica*) is indigenous to Ceylon; and this and others are found along the Bay of Bengal, extending thence to Java and to the coasts of China. The leaves of these plants are more or less succulent, and abound in a very valuable fibre, remarkable alike for fineness, elasticity, and for strengh. Mr. Baker gives a description of those species which are now under cultivation at Kew.

THE other day Dr. Robert W. Felkin, of Edinburgh, received three letters from Emin Pasha. The latest of them is dated Wadelai, October 26, 1886, and no more recent news from the writer has reached this country. Before starting for the coast from Uganda, Dr. Junker had collected a caravan and obtained permission from King M'Wanga to send it to Wadelai. "Besides bringing me a good quantity of cloth," writes Emin Pasha, "there were many presents from yourself, as well as newspapers from 1884 to 1886, a few books, *Graphics*, and, what pleased me most and will prove most valuable, a good many numbers of NATURE, so that at last I am permitted once more to see what is taking place in the scientific world." Along with this letter Dr. Felkin received a scientific paper which will be published in the *Scottish Geographical Magazine*. It is an account of a tour to the Albert Nyanza.

IT has been decided to remove the Royal Observatory of Brussels to Uccle, about 3½ miles to the south-west of its present position. The new buildings were commenced in September 1883, and are now so far advanced that the transfer of the instruments, &c., is arranged to take place next year. Observations have already been taken at the new Observatory for about a year for the purpose of deducing corrections to be applied to the temperature-observations made in the town since 1833, to reduce them to the temperatures taken in the country.

A GERMAN mathematician has, from certain measurements effected, calculated that the quantity of snow which fell in Central Germany from December 19 to 23, between 50° and 52°·5 N. latitude and between 7° and 18° E. longitude, weighed no less than ten million tons.

THE Lord Mayor, sometime a member of the school, has arranged to be present at the opening, on May 24, of the new science and art buildings of Sir Andrew Judde's School, Tunbridge.

DR. F. DAY, F.R.S., author of "The Fishes of Great Britain and Ireland," will shortly publish with Messrs. Williams and Norgate his monograph on the Salmonidæ. It will be illustrated by coloured plates, and, in the first instance, be published for subscribers. It will be ready in July.

MESSRS. MACMILLAN will publish immediately a volume of "Essays and Addresses," by the Rev. J. M. Wilson, Head Master of Clifton College. The writer discusses the relation between ethical and theological questions and the ideas of modern science.

THE Council of the London Mathematical Society have sanctioned the issue of a complete index of all the papers printed in the Proceedings of the Society since its foundation. Seventeen volumes have been published. All persons who take an interest in mathematical researches and who wish to know what has been done by the Society in their respective branches are invited to apply to the Secretaries (22 Albemarle Street, W.) for a copy of the index.

The Clothworkers' Company of London have shown lately that they thoroughly understand the necessity for an improved system of technical education. At Dewsbury the Jubilee is to be celebrated by the establishment of a technical school, and the Clothworkers' Company have agreed to raise the local fund for the building and equipment of the institution from £10,000 to £11,000. In addition to this they have promised an annual subscription of £50 towards the maintenance of the school. The same Company, having contributed £3,500 to the fund for the erection of the Bradford Technical College, as well as £500 per annum towards its maintenance, have now promised to contribute £500 to a fund which is being raised to pay off the debt still remaining on the building. The additional buildings of the Textile Industries and Dyeing Departments of the Yorkshire College, now completed and equipped, were erected by the Clothworkers' Company at an expense of £30,000.

WE regret to learn that the amount of support given to the proposed memorial to the late Thomas Edward, the Banff naturalist, has been so small that the project is in abeyance; and the Committee are contemplating the return of the subscriptions received. It will be much to be regretted if some means of commemorating Edward cannot be found, similar to the John Duncan Prizes in the Vale of Alford. It will be remembered that a considerable proportion of the sum subscribed for Duncan in his old age was placed by him in the hands of trustees just before his death to found prizes for the encouragement of the study of botany in his own locality. Edward accomplished much more for science than Duncan, and it will be lamentable if no memorial of him can be established. Any persons who may wish to prevent the threatened abandonment of the memorial should communicate at once with Mr. John Allan, Town Clerk of Banff.

IN the *Monatsheft* of the Berlin Chemical Society (viii. 73) Dr. K. Olszewski has a paper on the "Absorption-Spectrum of Liquid Oxygen and of Liquid Air." On examining the absorption-spectrum of liquid oxygen with the help of a small direct-vision spectroscope—employing solar light—two strong dark lines were noticed in the orange and yellow portions of the spectrum, and these did not completely disappear after the volatilization of the oxygen. They were in fact found to be present in the ordinary solar spectrum, being faint at midday, but very distinct towards sunset. On employing greater dispersion, the oxygen absorption-lines expanded to bands like the telluric bands of the solar spectrum, and they were noticed, not only when solar light was employed, but also when the electric arc or the lime-light was made use of. In these experiments the oxygen layer was 7 mm. thick, and on increasing this to 12 mm. two more bands made their appearance; namely, a very faint one in the green, and another somewhat stronger in the blue. The positions of the four oxygen bands were determined with a Vierordt's spectroscope, the wave-length numbers being—

Band in orange	634—622
,, ,, yellow	581—573
,, ,, green	535
,, ,, blue	481—478

or, taking the middle of the lines, 628, 577, 535, and 480. Line 628 is distinguished by its breadth, and 577 by its intensity; the more feeble bands, 535 and 480, appear to be absent from the solar spectrum. With the view to determine the spectrum of the other main constituent of the atmosphere, pure nitrogen was not employed, but merely air carefully freed from moisture and carbonic acid. The spectrum of the liquefied air was examined under the same conditions as in the case of the oxygen, but no new bands made their appearance. The spectrum consisted merely of the bands 628 and 577 mentioned above, and these were but faint; they became stronger as the air became richer in oxygen through the volatilization of nitrogen, but were still far less intense than in the spectrum of pure oxygen. The determination of the absorption-bands of liquid oxygen is of importance in connexion with the discussion of the origin of the telluric lines of the solar spectrum. Janssen and Secchi have shown that most of these are due to aqueous vapour, and, according to Ångström, the bands which on account of their stability cannot well be due to aqueous vapour are A, B, α, and δ, the two latter coinciding with the two strongest oxygen bands. According to Egoroff, who recently examined the spectrum of compressed gaseous oxygen, the telluric bands A, B, and probably α, are due to oxygen. Janssen obtained similar results, but found also some other bands in the spectrum of compressed oxygen. Olszewski cannot confirm either the presence or absence of the groups A and B from the absorption-spectrum of liquid oxygen, as he has been unable to make exact observations in this part of the spectrum.

ANOTHER paper by Dr. K. Olszewski in the *Monatsheft* (viii. 69) is on the "Determination of the Boiling-Point of Ozone." It has been shown by Hautefeuille and Chappuis that when ozonized oxygen is exposed to a pressure of 125 atmospheres and to the temperature of boiling ethylene (−102°·5), the ozone is obtained in the form of a dark-blue liquid which retains the liquid form for a short time at the above temperature, after the removal of the pressure. It seemed, therefore, that the boiling-point of ozone could not be much below that of ethylene, and attempts were therefore made by Olszewski to liquefy ozone at the atmospheric pressure merely by the application of cold. At a temperature of −150° no liquid was obtained, the large proportion of oxygen present probably hindering the condensation of the small percentage of ozone. When a lower temperature (−181°·4) was employed—that of boiling oxygen—the ozone readily condensed to a dark-blue liquid. At this tem perature it is transparent in very thin layers, but is almost opaque in layers 2 mm. thick. In order to determine its boiling-point, the tube containing it was introduced into a vessel containing liquid ethylene cooled to about − 140°. The ozone still retained the liquid form, and only began to vaporize when the temperature of the ethylene had risen to near its boiling-point. The temperature of the ethylene was determined by means of a carbon bisulphide thermometer, which at the moment of incipient ebullition of the ozone indicated a temperature of − 109°, this corresponding to − 106° of the hydrogen thermometer. The boiling-point of pure ozone is therefore approximately − 106°. Experiments with liquid ozone require great caution on account of the readiness with which explosions occur. If, for instance, liquid ozone comes into contact with ethylene gas, an extremely violent explosion occurs in spite of the low temperature. It is therefore necessary to exclude the inflammable gas from contact with the ozone, and then explosion need not be feared.

NOT less interesting than syntheses of vegetable or animal principles are the attempts which are made from time to time to build up minerals of the same crystalline form and chemical composition as those occurring upon the surface of our planet. One of the most widely distributed minerals—the historic magnetite—found so universally throughout the whole of the more basic rocks, and the square or triangular sections of which are familiar to every micro-petrologist, has long been a favourite subject for attempts, partially successful, at artificial reproduction. But probably the best method of effecting this has of late been devised by M. Alex. Gorgeu (*Comptes rendus*, No. 17, 1887), who has obtained fine crystals, sufficiently large to enable him to prove their complete identity with those of native magnetite. His method was to drop iron wire or filings into a bath of fused sulphite and sulphide of sodium, when a double sulphide of iron and sodium was formed, together with an oxide of iron richer in protoxide than magnetite; in a short time this oxidized to magnetite, and the sulphide and sulphite were converted to sulphate of sodium. The crystals of magnetite obtained, when washed free from the sodium sulphate, were a millimetre in section, of octahedral form modified by faces of the rhombic dodecahedron, and attracted by the magnet; they possessed the metallic lustre and the same specific gravity and hardness as crystals of naturally occurring magnetite.

WE have received the second edition of Miss Clerke's "Popular History of Astronomy during the Nineteenth Century," published by Messrs A. and C. Black. We regard it as a most encouraging sign of the times that in a period of not over eighteen months, the first edition of such a book as this should have been exhausted. It shows that the number of persons interested in astronomical science who care to read sound treatises requiring a considerable amount of attention is on the increase, and we know no book which is likely to foster the love of the subject among such people better than Miss Clerke's. The mere process of bringing up to date has involved the insertion of a considerable amount of new matter. Celestial photography naturally comes in for an added share of attention, directed chiefly to the discoveries of nebulæ in the Pleiades by the MM. Henry and Mr. Roberts; to the work in stellar spectral photography in progress at Harvard College; and to the preliminary essays in photographic charting made at Paris, Liverpool, and the Cape of Good Hope. Other new or extended passages relate to the bright-line spectra of γ Cassiopeiæ and β Lyræ, stellar photometry, the effects of tidal friction on the satellite system of Mars, and the daylight photography of the sun's corona. The theory of sunspots unfolded by Mr. Lockyer in his "Chemistry of the Sun" finds a place in the chapter on "Solar Observations and Theories," and that on "Solar Spectroscopy" includes an account of the observations of the spectra of sunspots at South Kensington, 1879-85, with their results for solar chemistry. We notice some modification in the author's views regarding the dissociation of terrestrial elements in the sun, the presence of the bright-line spectrum of oxygen in the solar spectrum, and Young's "reversing layer." She moreover (apparently on good grounds) withdraws the statement that comets, moving sensibly in the same track in the parts of their orbits near the sun, must have nearly identical periodic times. Paragraphs in the new edition are assigned respectively to the last comer (Comet 1887 I.) of the remarkable group connected with the comet of 1843, and to the singularities of Comet Pons-Brooks; while the observations on the meteors of November 27, 1885, on the new star in Andromeda, and at Grenada during the total eclipse of August 29, 1886, are fully particularized. We are glad to perceive that Miss Clerke has taken advantage of many of the hints of her critics, supplying, for instance, the few omissions in her work pointed out by Sir Robert Ball in NATURE (vol. xxxiii. p. 314). A completely new feature is a chronological table of the principal astronomical events between the years 1774 and 1887; and a frontispiece and vignette, reproducing Mr. Common's and the MM. Henry's photographs of the Orion Nebula, Jupiter, and Saturn, add to the attractions of the second edition.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus*) from India, presented by Mr. G. Lester; a Brazilian Tree-Porcupine (*Sphinghurus prehensilis*) from Brazil, presented by Dr. William Studart; a Vervet Monkey (*Cercopithecus lalandii*) from South Africa, presented; a Domestic Sheep (*Ovis aries*, four-horned var.) from Arabia, presented by Mr. C. E. Kane; a Tooth-billed Pigeon (*Didunculus strigirostris*) from the Samoan Islands, presented by Mr. Wilfred Powell; a Great-crested Grebe (*Podiceps cristatus*) from Norfolk, presented by Mr. T. E. Gunn; a Goldfinch (*Carduelis elegans*), a Greenfinch (*Ligurinus chloris*), a Red Bunting (*Emberiza schœnichus*), British, presented by Master H. J. Walton; an Eyed Lizard (*Lacerta ocellata*) from Cannes, presented by Mr. J. E. Warburg; a Smooth Snake (*Coronella lævis*) from Hampshire, presented by Mr. H. B. Pain; a Green Turtle (*Chelone viridis*) from Ascension, presented by Dr. Keenan; a Squirrel Monkey (*Chrysothrix sciurea*) from Guiana, a Servaline Cat (*Felis servalina*) from West Africa, a Black-necked Swan (*Cygnus nigricollis*) from Antarctic America, two Natterer's Snakes (*Thamnodynastes nattereri*) from Brazil, purchased; four Prairie Marmots (*Cynomys ludovicianus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE MELBOURNE OBSERVATORY.—Mr. Ellery has recently issued his Annual Report referring to the year ending June 30, 1886. From it we learn that the new transit-circle has been in constant use during the year, and is in excellent order. There appears, however, to be a very gradual lowering of the west pier of the instrument since its erection in August 1884. There also appears a decided diurnal change in the level, the east pivot being higher in the morning and lower in the evening—probably due to the heating effects of the sun on the earth's crust, or on the building. The objects observed with the transit-circle during the year comprised fundamental clock stars, standard circumpolar stars, faint stars selected from the Melbourne zones, comet stars, refraction stars, and a list of stars proposed for insertion in the *Connaissance des Temps*. The great telescope was almost exclusively devoted to the revision of the southern nebulæ. During the year 214 of Sir J. Herschel's nebulæ were finally revised, 7 were searched for but not found, whilst 30 new nebulæ were discovered. There now remain only 95 nebulæ, which were observed by former observers, requiring final revision before publication. The photoheliograph was not in working order for several months during the year, owing to difficulties arising from the change in the size of the sun pictures taken, from 4 to 8 inches diameter. The number of photographs of the sun obtained during the year was therefore only 92.

THE TRANSIT OF VENUS IN 1882.—Mr. Stone's Report exhibiting the results deduced from the British observations of the transit of Venus in December 1882 has been published. The resulting values for the sun's mean equatorial horizontal parallax from the different phases of the transit, are as follow:—

External contact at ingress	$\pi = 8''\cdot760 \pm 0''\cdot122$
Internal ,, ,, ,,	$\pi = 8\cdot823 \pm 0\cdot023$
,, ,, ,, egress	$\pi = 8\cdot827 \pm 0\cdot050$ (α)
,, ,, ,, ,,	$\pi = 8\cdot882 \pm 0\cdot043$ (β)

(α) or (β) are the values resulting from this phase according to the phenomenon selected to represent true contact. The mean of these gives for

Internal contact at egress	$\pi = 8''\cdot855 \pm 0''\cdot036$
External ,, ,, ,,	$\pi = 8\cdot953 \pm 0\cdot048$

The combination of the values deduced from the internal contacts at ingress and egress gives $\pi = 8''\cdot839 \pm 0''\cdot021$ or $\pi = 8''\cdot825 \pm 0''\cdot028$ according as (α) or (β) is used. In the mean from internal contacts $\pi = 8''\cdot832 \pm 0''\cdot024$.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 MAY 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 May 15.

Sun rises, 4h. 10m.; souths, 11h. 56m. 8·1s.; sets, 19h. 42m.; decl. on meridian, 18° 51′ N.: Sidereal Time at Sunset, 11h. 15m.

Moon (one day after Last Quarter) rises, 1h. 35m.; souths, 6h. 34m.; sets, 11h. 40m.; decl. on meridian, 12° 20′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury ...	3 49	11 3	18 17	13 33 N.
Venus ...	6 6	14 38	23 10	25 29 N.
Mars ...	3 59	11 36	19 13	17 33 N.
Jupiter ...	16 57	22 13	3 29*	9 27 S.
Saturn ...	7 40	15 48	23 56	22 13 N.

* Indicates that the setting is that of the following morning.

Occultation of Star by the Moon (visible at Greenwich).

May.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
19 ...	29 Ceti ...	6½	2 52	3 50	63 260

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
T Cassiopeiæ ...	0 17·1	55 10 N.	May 20,		*m*
U Cephei ...	0 52·3	81 16 N.	,, 19,	2 38	*m*
R Sculptoris ...	1 21·8	33 8 S.	,, 17,		*M*
S Cancri ...	8 37·5	19 26 N.	,, 17,	20 28	*m*
U Ophiuchi ...	17 10·8	1 20 N.	,, 15,	0 16	*m*
β Lyræ ...	18 45·9	33 14 N.	,, 19,	2 0	m_2
R Lyræ ...	18 51·9	43 48 N.	,, 16,		*m*
R Cygni ...	19 33·8	49 57 N.	,, 21,		*M*
S Vulpeculæ ...	19 43·8	27 0 N.	,, 18,		*M*
η Aquilæ ...	19 46·7	0 43 N.	,, 19,	23 0	*m*
S Sagittæ ...	19 50·9	16 20 N.	,, 17,	1 0	*M*
T Delphini ...	20 40·1	15 59 N.	,, 20,		*M*
δ Cephei ...	22 25·0	57 50 N.	,, 17,	0 0	*M*

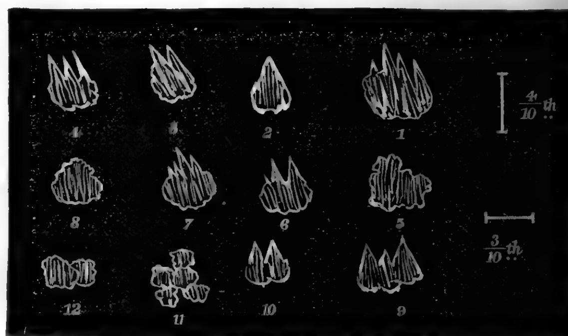
M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near α Coronæ ...	231	27 N.	Rather slow and faint.
,, η Aquilæ ...	294	0	Very swift.
From Delphinus	315	15 N.	Very swift.

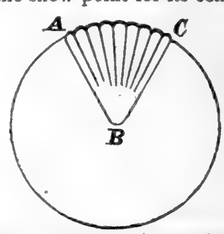
REMARKABLE HAILSTONES.

MR. E. J. LOWE writes to us from Shirenewton Hall, Chepstow, that remarkable hailstones fell there on April 5 from 1 55 p.m. till 2 p.m. They were far apart, and fell with but little force, and were entirely opaque, and had a vertical cleavage. Some were conical, with an irregular base; some were spiked at the apex, and of these no two were alike; others were very irregular in form. A great number were composed of two or three united; in one case as many as five were fast together. The longest were four-tenths of an inch long, and three-tenths of an inch broad. They melted very slowly, lasting as much as two minutes. The temperature was 39°·5, wet bulb 35°·4, and temperature on grass 36°·7. The hailstones were quite different from anything that Mr. Lowe had ever seen. The accompanying figure records a few of them.



Another account of remarkable hailstones has been sent to us by Mr. Reginald G. Durrant, of Marlborough College:—

"On April 24, about 12.30," Mr. Durrant writes, "while walking between Melrose and Kelso, a friend and myself were overtaken by a sudden and very violent hailstorm, accompanied by thunder. The violent burst lasted about two minutes, in which time the ground was completely covered with large hailstones rather more than half an inch long. I say 'long' advisedly, for all the specimens I examined were conical, and were all of them formed in the same way. The points had all the appearance of snow, being softer than the main bulk of the 'stones.' These snow portions occupied about one-third of the whole length, being white and non-transparent. The main portions of the hailstones were hard and ice-like, stranded lengthwise with from forty to fifty fibres of ice—each fibre curved separately at the top—and together forming a curved surface, as of a sphere having the snow point for its centre. Thus—



Angle A B C of section between 50° and 60°.

"On melting, the pointed part became translucent, while the other part became more opaque than at first, strands often remaining for a time, partially separated and curving outwards, as though they had been freed from compression in their lower extremities. Thus—

"The above appearances might admit of the hypothesis that these hailstones were fragments of radiated crystalline spheres, ut one would expect in that case to find pyramidal rather than onical shapes, or at least to find some shaped so as to complement the cones. I failed to notice any indications of such hapes in the specimens (about thirty) which I examined. I hould be inclined to believe that the soft, snow-like portions ad been formed during the passage of the harder stranded tones through a moist and possibly clouded stratum of air.

"I was unable to see how they reached the ground, whether oint or blunt end downwards. If in the latter way, one could ccount for the soft part, as being formed from previously unrozen particles, cooled by contact with the nucleus, and, so to peak, sliding back to a position sheltered from the air, as it wept by the sides of the cone.

"But if the narrower end were foremost (and that would be the nore natural position), then, unless the little mass—like an iceerg—could freeze particles in front of it before reaching them, it vould seem that the snow point must have resulted from the ccretion of small particles already frozen, and the pointed shape vould be what we should expect. The only rotatory motion ossible would be that in a plane perpendicular to the direction f the path through the air, and might account for the conical hape, the edges of any pyramid being rounded off."

M. A. Wentzil, of Izdebno, near Warsaw, writes to us of a ailstorm which occurred there on the 4th inst. "At 3 o'clock n the afternoon," he says, "hail began to fall, at first of small ize, but in a few minutes the hailstones increased to the size of valnuts. Nine such which I picked up at hazard weighed toether 13 lut (0·165 kilo). They were almost spherical with a nean diameter of $1\frac{3}{8}$ English inches. In the centre of each was kernel of clear ice about the size of a pea, and from this ernel radiated conical masses of white ice, so that the surface of he hailstone was like that of a mulberry, the interspaces being illed with clear ice. The damage in the gardens and to glass anes was, as may be imagined from the size of the stones, onsiderable

On March 3 we printed a letter from Mr. C. S. Middlemiss, lescribing a fall of top-shaped hailstones near Ramnagar, in the North-West Provinces of India (NATURE, vol. xxxv. p. 413). Writing to us on March 7, Mr. T. Spencer Smithson said p. 438) that a fall of hailstones, almost exactly similar to those lescribed by Mr. Middlemiss, had taken place in the neigbourood of Rochdale on August 6, 1885. Mr. Smithson, however, ointed out that besides the horizontal stratification in these hailtones there was a perpendicular one, giving each hailstone the ppearance of being composed of alternate cylinders of clear nd white ice; and he asked Mr. Middlemiss to state whether he hailstones seen at Ramnagar had this peculiarity. Mr. Middlemiss now writes to us, in reply to Mr. Smithson's question, hat the broad end of the hailstones showed no trace of any livisional planes whatever, being perfectly amorphous as originlly stated. "The banded portion, so far as my memory serves ne," he says, "may have possessed a faint longitudinal *striation*, ust sufficient to run the bands together and to induce me to hade the diagrams vertically rather than horizontally, but I canot be certain of it. It was not a marked feature, I feel sure."

SCIENTIFIC SERIALS.

Rivista Scientifico-Industriale, February.—The total solar clipse of August 19, 1887, by Prof. Cacciatore. Prof. Tacchini aving at the last eclipse established the presence round the sun f delicate white protuberances different from the ordinary roseoloured protuberances daily visible under the spectroscope, it s announced that the Minister of Public Instruction will send Prof. Tacchini and Prof. Riccò to observe the August eclipse in Siberia for the express purpose of studying these new maniestations.—On the origin of the variations of intensity in the dry ile, and on the means of preventing them, by Prof. Luigi Palmieri. The author's experiments lead to the conclusion that he dry pile is not only the most durable, but also the most onstant, and that the variations of intensity are due to disersions. These dispersions are independent of the moisture and emperature of the surrounding atmosphere, at least within ertain limits, while the pile enveloped in a volume of air will reserve its force almost unaltered for years, and not only ot diminished, but even slightly increased, by the atmospheric noisture.

March.—A new method of measuring the specific weights of luids, by Dr. Alessandro Sandrucci. A new method is described, for which a single apparatus alone is needed, and for which the author proposes the name of areovolumeter, combining as it does the functions of the areometer and volumeter. Although somewhat less accurate than Marangoni's recently invented double volumeter, this process reduces the disturbing influence of superficial tension to a minimum, while completely dispensing with the empirical scales on the volumeters, the determination of which involves considerable difficulty.

Bulletin de l'Académie Royale de Belgique, February.—Determination of the direction and velocity of the movement of the solar system in space, by M. P. Ubaghs. For the direction, the same method is adopted as that already known through the labours of M. Folie. For the velocity, use is made of three groups of stars of the second, third, and fourth magnitudes, the first group belonging probably to the solar nebula itself. The resulting velocity is only 16,500,000 kilometres for the year as compared with the 850,000,000 obtained by Homann working on the spectroscopic observations of Greenwich.—On the influence of diurnal nutation on the questions connected with the observations of γ Draconis made at the Observatory of Greenwich, by L. Niesten. By employing M. Folie's formula of diurnal nutation the author has determined a source of error long suspected in the calculations of Main and Downing. By introducing the necessary correction he arrives for the first time at a positive parallax for γ Draconis. He thus also, for the first time, determines beyond all doubt the real existence of diurnal nutation.—On the two tetrabromureted hydrocamphenes, by W. De la Royère. It is shown that by the action of the chlorobromide of phosphorus on camphor there are produced two tetrabromureted hydrocamphenes differing in their physical properties, specific weights, points of fusion, and molecular rotatory power. By subjecting them to the action of the nitrate of silver, heat, and chlorine, the author transforms the two isomeres into one and the same tribromureted camphene; while metallic silver reduces them to an identical bibromureted camphene, chlorine producing a bichlorureted and tetrabromureted hydrocamphene also identical for both.

Rendiconti del Reale Istituto Lombardo, February.—State of education in Italy, by Prof. A. Amati. The results of the recent official returns are given in tabulated form for the 284 circuits of the kingdom, showing in separate columns the percentage of "analfabeti" (illiterate) in each communal district and its chief town. The general result appears to be more unsatisfactory than had been anticipated, the disparity especially between the towns and rural districts being still excessive, even in Piedmont, Liguria, and some of the other best regulated departments.—Measurement of the muscular force in man, by Prof. G. Zoja. A brief account is given of the various instruments devised for determining scientifically the degree of muscular force in individuals, according to sex, age, and other conditions, from Regnier's dynamometer to the present time. The author also proposes a scheme of classification based on the degree of muscular energy possessed by the individual, and ranging from a given mean (Mesostheni) upwards and downwards through the Megastheni and Microstheni to the two extremes of Heraclestheni and Astheni.

March.—Observations on the luminous solar rays, by Giovanni Cantoni. The attention of meteorologists is called to the lucimeter recently constructed at Milan, which is stated to give more satisfactory results than the English heliograph with glass sphere or Craveri's chemical photometer. It determines with great accuracy the relative measure of the luminous rays at all hours of the day in relation to the altitude of the sun above the horizon of the place of observation. It also gives the integral of the successive and varying luminous influences of the sun during the course of a whole day. With regard to the instrument described by Clark in NATURE (vol. xxxii. p. 233) for measuring the radiant energy of the sun, its principle is stated to be based not so much on Wollaston, as on the discovery made many years ago by Bellani, and for some time applied by the author to agricultural meteorology.—Meteorological observations made at the Brera Observatory, Milan, for the month of February.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, April 21.—"Some Applications of Dynamical Principles to Physical Phenomena. Part II." By J. J. Thomson, M.A., F.R.S., Fellow of Trinity College and

Cavendish Professor of Experimental Physics in the University of Cambridge.

This is a continuation of a paper with the same title published in the Phil. Trans., 1885, Part II. In the first paper dynamical principles were applied to the subjects of electricity and magnetism, elasticity and heat, in order to establish relations between phenomena in these branches of physics. In this paper corresponding principles are applied to chemical and quasi-chemical processes such as evaporation, liquefaction, dissociation, chemical combination, and the like.

Many of the results obtained in this paper have been or can be obtained by means of the Second Law of Thermodynamics, but one of the objects of the paper is to show that there are other ways of attacking such questions, and that in many cases such problems can be solved as readily by the direct use of dynamical principles as by the Second Law of Thermodynamics.

A great deal has been written on the connexion between the Second Law of Thermodynamics and the principle of Least Action; some of these investigations are criticised in the first part of the paper, after this it is shown that, for a collection of molecules in a steady state, the equation (which for ordinary dynamical systems is identical with the well-known Hamiltonian principle)—

$$\delta(\overline{T} - \overline{V}) = 0,$$

is satisfied; where $\overline{T}$ and $\overline{V}$ are respectively the mean values of the kinetic and potential energies taken over unit time, and where the variation denoted by δ is of the following kind.

The co-ordinates fixing the configuration of any physical system, consisting according to the molecular theory of the constitution of bodies of an immense number of molecules, may be divided into two classes:—

(*a*) Co-ordinates, which we may call molar, which fix the configuration of the system as a whole; and

(*b*) Molecular co-ordinates which fix the configuration of individual molecules.

We have the power of changing the molar co-ordinates at our pleasure, but we have no control over the molecular co-ordinates.

In the equation—

$$\delta(\overline{T} - \overline{V}) = 0,$$

only the molar co-ordinates are supposed to vary, all velocities remaining unchanged. Hence in applying this equation we need only consider those terms in $\overline{T}$ and $\overline{V}$ which involve the molar co-ordinates. Expressions for these terms for gases, liquids, and solids are given in the paper; the rest of the paper after these have been obtained consists of applications of the above equation.

The density of a vapour in equilibrium with its own liquid is obtained as a function of the temperature, and the effect upon the density of such things as the curvature or electrification of the surface of the liquid is determined.

The phenomenon of dissociation is next investigated, and an expression for the density of a dissociated gas obtained which agrees substantially in form with that given by Prof. Willard Gibbs in his well-known paper on the "Equilibrium of Heterogeneous Substances."

The effect of pressure upon the melting-point of solids and the phenomena of liquefaction are then investigated, and the results obtained for the effect of pressure upon the solubility of salts are shown to agree with the results of Sorby's experiments on this subject. The effect of capillarity upon solubility is investigated, and it is shown that if the surface-tension increases as the salt dissolves then capillarity tends to diminish the solubility, and *vice versâ*.

The question of chemical combination is then considered, particularly the results of which is called by the chemists "mass-action," and of which a particular case is the division of a base between two acids.

The general problem investigated is that in which we have four substances, A, B, C, D, present, such that A by its action on B produces C and D, while C by its action on D produces A and B. The relation between the quantities of A, B, C, D present when there is equilibrium is obtained and found to involve the temperature; when the temperature is constant it agrees in some cases with that given by Guldberg and Waage, though in others it differs in some important respects. Thus, if $\xi, \eta, \zeta, \epsilon$ be the number of molecules of A, B, C, D respectively, when there is equilibrium, θ the absolute temperature, H the amount of heat given out when the chemical process which results in the increase of ξ by unity takes place, and k a quantity which is the same for all substances, then it is proved that—

$$\frac{\xi^p \eta^q}{\zeta^r \epsilon^s} = CE^{k\theta},$$

where C is a constant; p, q, r, s are quantities such that if (A) represents the molecule of A, with a similar notation for the other molecules, then the chemical reaction can be represented by the equation—

$$p\{A\} + q\{B\} = r\{C\} + s\{D\}.$$

Thus if A, B, C, D be respectively sulphuric acid, sodium nitrate, nitric acid, and sodium sulphate, in which case the reaction is represented by—

$$H_2SO_4 + 2NaNO_3 = 2HNO_3 + Na_2SO_4,$$

then, if the molecules of sodium nitrate and nitric acid be represented by $NaNO_3$ and HNO_3—

$$p = 1, \quad q = 2, \quad r = 3, \quad \text{and } s = 1.$$

If, however, the molecules of sodium nitrate and nitric acid are represented respectively by $Na_2N_2O_6$ and $H_2N_2O_6$, then since the chemical reaction may be written—

$$H_2SO_4 + Na_2N_2O_6 = H_2N_2O_6 + Na_2SO_4,$$

$$p = 1, \quad q = 1, \quad r = 1, \quad \text{and } s = 1.$$

According to Guldberg and Waage the relation between $\xi, \eta, \zeta, \epsilon$ is—

$$\xi\eta = k\zeta\epsilon;$$

this, when the temperature is constant, agrees with the above expression if $p = q = r = s$.

We see that the state of equilibrium will vary rapidly with the temperature if H be large, that is, if the chemical process is attended by the evolution of a large quantity of heat.

The effect of alterations in the external circumstances such as those which may be produced by capillarity, pressure, or electrification are investigated, and it is shown that anything giving rise to potential energy which increases as the chemical combination goes on tends to stop the combination.

The last part of the paper is taken up with the consideration of irreversible effects such as those accompanying the passage of electric currents through metallic conductors or electrolytes. These are looked upon as the average of a large number of discontinuous phenomena which succeed each other with great rapidity. The ordinary electrical equations with the usual resistance terms in, represent on this view the average state of the system, but give no direct information about its state at any particular instant. It is shown that if we take this view we can apply dynamical principles to these irreversible effects, and the results of this application to the case of electrical resistance are given in the paper.

"On Parts of the Skeleton of *Meiolania platyceps*." By Sir Richard Owen, K.C.B., F.R.S.

The subjects of the present paper are additional fossil remains of *Meiolania platyceps* from Lord Howe's Island, transmitted to the British Museum since the author's previous paper on that extinct reptile. Additional cranial characters are defined and illustrated by drawings of more or less perfect specimens of the skull, of vertebræ of the neck, the trunk, and tail, of limb-bones, and portions of the dermal skeleton.

The author sums up the affinities, deducible from the above parts of the skeleton, to the orders *Chelonia* and *Sauria*, with grounds for the conclusion, mainly based on the absence of evidence of a carapace and plastron, that the genera *Megalania* and *Meiolania* are more nearly akin to the Saurian division of the class REPTILIA; in which he proposes to refer those extinct genera to a sub-order called *Ceratosauria*.

"Conduction of Heat in Liquids." By C. Chree, B.A., King's College, Cambridge. Communicated by Prof. J. J. Thomson, F.R.S.

Linnean Society, April 21.—Mr. W. Carruthers, F.R.S., President, in the chair.—Mr. E. M. Holmes exhibited specimens of various species of *Shorea* from Borneo and Sumatra, which plants yield vegetable fats used for technical purposes. Several species of *Dichopsis* affording gutta-percha from the bark and fat from the seeds were also shown. Mr. Holmes pointed out the importance of the cultivation of the more valuable of these trees, among others, *D. oblongifolia* and *Ceratophorus*

Leerii, since they are being rapidly destroyed by the natives. Their cultivation has already been commenced by the Dutch, but not a day too soon, as the trees take at least twenty years ere they are productive and valuable.—Mr. Patrick Geddes read a paper on the nature and causes of variation in plants and animals. The fact of organic evolution is no longer denied, but its physiological factors have not yet been adequately analyzed. Even those who regard natural selection as at once the most important and the only ascertained factor of the process admit that, such an explanation being from the external standpoint—that of the adaptation of the organism to survive the shocks of the environment—stands in need of a complementary explanation which shall lay bare the internal mechanism of the process, *i.e.* not merely account for the survival, but explain the origin, of variations. The relative importance of the external and internal explanations will, moreover, vary greatly in proportion as variations are found to be "spontaneous," *i.e.* in any direction indifferently, or "determinate," *i.e.* in some given direction continuously. Avoiding any mere postulation of an "inherent progressive tendency," common to both pre- and post-Darwinian writers, the definite analysis of the problem starts with that conception of protoplasm which is the ultimate result of morphological and physiological analysis, viz. to interpret all phenomena of form and function of cells, tissues, organs, and individuals alike in terms of its constructive and destructive ("anabolic and katabolic") changes. While the external or environmental explanation of evolution starts with the empirical study of the effect of human selection upon the variations of animals and plants under domestication, the internal or organismal one as naturally commences with the fundamental rhythm of variation in the lowest organism in nature. It also investigates the nature of the simple reproductive variation upon which the origin of species as well as individuals must depend, before attempting that of individual variations. The interpretation of all the phenomena of male and female sex as the outcome of katabolic and anabolic preponderance is shown largely to supersede the current one of sexual selection, and in some cases at least that of natural selection, *e.g.* the specially important one of the origin of such polymorphic communities as those of ants and bees. In such cases natural selection acts not as the cause of organic evolution, but as the check or limitation of it, and acquires importance rather as determining the extinction than the origin of species. The process of correlation, especially that between individualization and reproduction, is mooted by the author, and its application to the origin and modification of flowers, &c., outlined. A discussion is given of the embryological and pathological factors of internal evolution, with an application of the whole argument to the construction of the genealogical tree of plants and animals.—A report on the Gephyreans of the Mergui Archipelago, by Prof. Emil Selenka, of Erlangen, was read; this communication dealing chiefly with a technical description of species.

Zoological Society, April 28.—Fifty-eighth Anniversary Meeting.—Prof. Flower, LL.D., F.R.S., President, in the chair.—Many members of the Council and other Fellows of the Society were present. After some preliminary business, the report of the Council on the proceedings of the Society during the year 1886 was read by Mr. P. L. Sclater, F.R.S., Secretary to the Society. It stated that the number of Fellows on January 1, 1887, was 3146, showing a decrease of 47 as compared with the corresponding period in 1886. The total receipts for 1886 had amounted to £25,787 0*s.* 4*d.*, showing a decrease of £22 9*s.* 9*d.* as compared with the previous year. This slight decrease was mainly due to the falling off of the number of Fellows, and consequently of the receipts for subscriptions. The balance brought from 1885 was £972 8*s.* 1*d.*, making a total of £26,759 8*s.* 3*d.* available for the expenditure of 1886. The ordinary expenditure for 1886 had been £24,438 17*s.* 9*d.* Besides that, an extraordinary expenditure of £129 15*s.* had been incurred, which brought up the total expenditure for the year to £24,568 12*s.* 9*d.* The usual scientific meetings had been held during the session of 1886, and a large number of valuable communications had been received upon every branch of zoology. These had been published in the annual volume of Proceedings for 1886, which contained 716 pages, illustrated by 60 plates. Besides this, five parts of the twelfth volume of the Society's Quarto Transactions had been issued, thus making up all the arrears in this branch of the publications. A new edition of the Library Catalogue had also been prepared and issued. The Society's library now contained about 15,000 separate volumes. The "Zoological Record," which consisted of an annual volume containing a summary of the work done in the various branches of zoology in each year, would in future be published by the Society under the superintendence of a committee of the Council appointed for the purpose, and edited by Mr. F. E. Beddard, Prosector to the Society. The visitors to the Society's Gardens during the year 1886 had been altogether 639,674. The corresponding number in 1885 was 659,896. A slight alteration in the arrangements for the Davis Lectures on zoological subjects had been made for the present year. Mr. F. E. Beddard, Prosector to the Society, had been appointed Davis Lecturer, and had commenced a course of ten lectures on the Classification of Vertebrate Animals. The lectures were a continuation of a series given last year in connexion with the London Society for the Extension of University Teaching. The number of animals in the Society's collection on December 31 last was 2609, of which 777 were mammals, 1429 birds, and 403 reptiles. Amongst the additions made during the past year, 15 were specially commented upon as of remarkable interest, and in most cases as representing species new to the Society's collection. About 30 species of mammals, 20 of birds, and 3 of reptiles had been bred in the Society's Gardens during the summer of 1886. The report concluded with a long list of the donors and their various donations to the Menagerie during the present year.—A vote of thanks to the Council for their report was then moved by the Hon. J. S. Gathorne-Hardy, M.P., seconded by Mr. H. Berkeley James, and carried unanimously. The report having been adopted, the meeting proceeded to elect the new members of the Council and the Officers for the ensuing year. The usual ballot having been taken, it was announced that Sir Joseph Fayrer, K.C.S.I., F.R.S., Mr. John P. Gassiot, Col. James A. Grant, C.B., C.S.I., F.R.S., Prof. A. Newton, F.R.S., and Mr. Joseph Travers Smith, had been elected into the Council in place of the retiring members; and that Prof. W. H. Flower, 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 Sir Joseph Fayrer, K.C.S.I., and seconded by Mr. Herbert Druce, and carried unanimously.

Chemical Society, April 21.—Mr. William Crookes, F.R.S., President, in the chair.—The following papers were read:—The atomic weight of gold, by Prof. T. E. Thorpe, F.R.S., and Mr. A. P. Laurie.—The atomic weight of silicon, by Prof. T. E. Thorpe, F.R.S., and Mr. J. W. Young.—Note on substitution in the benzene nucleus, by Dr. H. Foster Morley.—Reply to the foregoing note, by Prof. Henry E. Armstrong.

Royal Microscopical Society, April 13.—Rev. Dr. Dallinger, President, in the chair.—Mr. T. C. White exhibited a series of photomicrographs which he had recently taken, showing the result of the method of cutting off some of the superfluous light by means of a sliding diaphragm so as to be able to admit just enough to bring out the detail and nothing more. The specimens shown were printed on Eastman's bromide paper instead of silver paper which he found brought out the character of the detail very much better.—Mr. F. R. Cheshire called attention to some specimens of bees, known as "fertile workers." It was generally well known that in the bee-hive all the eggs were usually laid by the queen, and in her absence no ovipositing occurs until they have taken some of the eggs remaining in the hive, and by a special feeding of the larvæ have been able to produce fresh queens. If, however, it should happen that in a hive which has lost its queen there are not eggs available for this purpose it was found that some of the workers under some special circumstances which could not be very clearly explained, became capable of laying eggs, but that such eggs produced drones only. These bees were known as fertile workers, and though there could be no doubt as to their frequent existence, they were very difficult to catch, owing to their being the same in appearance as the ordinary workers. He now exhibited two of these fertile workers having the ovaries drawn out of the bodies and attached to the stings and abdominal plates so as to show that they really were workers. There was a remarkable peculiarity to be observed in connexion with the ovarian tubes of these insects—every ordinary worker possessed an undeveloped ovary which it was very difficult both to detect and dissect, but when under the influence of some stimulus the worker became fertile, a number of points began to appear in the tubes which afterwards became developed, and it would seem that the eggs were developed in alternation, an examination of the tubes showing them to contain developed eggs alternating with others in an undeveloped condition and of which some very curious instances were seen in the specimens before the meeting.—Mr. Crisp called attention to some photomicrographs of animalcules sent by Mr. J. B. Robinson; and to photographs of snow-crystals sent by Mr. Waters, from Davos Platz; also to a specimen of one of the earliest forms of the compound microscope by Campani, of Rome, made some time prior to 1665.—A new form of adjustable nose-piece, by Dr. Zeiss, was exhibited, in which the objective was made to slide in a groove in an inclined plane which insured its not scraping along the surface of the cover-glass when being changed.—A paper by Mr. P. H. Gosse, on twelve new species of Rotifera, was read.

LIVERPOOL.

Biological Society, March 12.—Prof. Herdman, Vice-President, in the chair.—A paper was read by Mr. I. C. Thompson on some new and little known Copepoda of Liverpool Bay. The paper included the description of several new points in the anatomy of several species new to British seas.—Dr. Collins communicated some observations on anatomical abnormalities.—Mr. Harvey Gibson (Secretary) read the first of a series of notes on floral morphology, dealing with the angle of insertion of the petals on the thalamus in the *Polypetalæ* and the form of the flower as a whole in the *Gamopetalæ*, in their relation to the protection of the essential organs.

April 23.—Prof. Herdman, Vice-President, in the chair.—The Secretary (Mr. Harvey Gibson) read a preliminary paper on a research into the nature and function of the so-called "hepatic cells" of *Lumbricus terrestris*, by himself and Mr. A. J. Chalmers. The results so far tend to show that the so-called "cells" are rather digestive glands and not "vasifactive tissue" as suggested by some biologists.—Mr. G. F. Moore read a note on a new tank for the maceration of osteological specimens.—Dr. Herdman read a preliminary paper by Miss F. Palethorpe and Miss C. Wilson on a collection of Ascidians from Australian seas, sent by the Sydney Museum authorities to the Fisheries Exhibition, and containing a number of new species.—Dr. Bruce exhibited a collection of surface animals from Maltese seas, and Mr. R. McMillan exhibited a specimen of a pile from the works of the Canadian Pacific Railway, destroyed by the borings of *Teredo*.—Mr. G. H. Morton exhibited the spicules of sponges that he recently found in several places in the chert-beds of the Cefn-y-Fedw sandstone of Denbighshire and Flintshire, on the horizon of the millstone grit. Mr. Morton's observations have been confirmed by Dr. Hinde. The spicules probably belong to a genus of *Hyalonema*, and have not been recorded previously from North Wales.

BERLIN.

Physiological Society, April 15.—Prof. Du Bois Reymond, President, in the chair.—Dr. Prause spoke on the degeneration of nerves resulting from sectional injuries. According to Waller, when a nerve is cut through, the peripheral parts degenerate, whereas the central remain intact. The result of a thorough investigation of the nerves in cases of amputation, which the speaker carried on some years ago in conjunction with Dr. Friedländer, has however shown that the central parts of the divided nerves had degenerated even right up to the spinal cord. Quite recently, Dr. Prause has repeatedly examined the nerves in cases where, owing to gangrene of the foot, the leg had been amputated close below the knee. Here the degeneration of the nerves extended up to, and probably beyond, the surface of amputation, having in such cases started from the gangrenous parts, and progressed centripetally. Side by side, however, with the larger number of degenerated fibres a few normal fibres were also found. From experiments on animals in which nerves of very different kinds, both sensory and mixed, were cut through, it appeared that in the peripheral parts by far the larger number of the fibres degenerate, while at the same time a not inconsiderable number remain unaltered; similarly degenerated and normal fibres were found in the central part of the nerve, only in this case the relative number of each kind is in an inverse proportion to that in which they are found in the peripheral part. It follows from the above that, starting from the point of section of a nerve, one set of fibres degenerates towards the periphery, the other towards the centre. It seemed right to assume that those fibres which degenerate towards the periphery have the[ir] trophic centre in the spinal cord or brain as the case may b[e], while those which degenerate centripetally are dependent f[or] their nutrition on some centre at the periphery, such as presum[ably] the tactile corpuscles of Meissner. Were this not s[o], Waller's law would again hold good, since only those parts of [a] nerve degenerate which are cut off from their trophic centre[;] only sensory nerves degenerate centripetally.—Dr. Grunmac[h] communicated the results of some experiments on the relatio[n] between the curve of distension of elastic tubes and the rate [of] the pulse-wave in the same. These experiments were carrie[d] out with various gutta-percha tubes and with the aorta of horses[;] the internal pressure being varied from 0 to 200 mm. of mercury[;] the alteration of volume of the tubes and the rate of trans[-]mission of the pulse-wave were both measured. The result[s] showed that the rate of the pulse-wave is most markedl[y] dependent upon the distension-curve or coefficient of elasticit[y] of the tube; this coefficient is, however, very variable wit[h] different tubes. The behaviour of a horse's aorta approximate[d] to that of an india-rubber tube wrapped round with linen. Th[e] thickness of wall of the tubes and the size of their lumen wa[s] very slightly, if at all, altered by the varying pressure, and thei[r] influence upon the relationship of pressure and rate of pulse-wave was quite subordinate.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

La Cytodiérèse chez les Animaux: J. B. Carnoy (Peeters, Louvain).—Report on the Mining Industries of New Zealand (Wellington).—Gold Fields of Victoria; Reports of the Mining Registrars for the Quarter ended December 31, 1886 (Ferres, Melbourne).—Elements of Dynamics, part 1, book iv.: W. K. Clifford (Macmillan).—Lessons in Elementary Practical Physics, vol. ii.: B. Stewart and W. W. H. Gee (Macmillan).—Pioneering in New Guinea: James Chalmers (R.T.S.)—Eastern Geography: Prof. A. H. Keane (Stanford).—Systematic Lists of the Flora, Fauna, Palæontology, and Archæology of the North of Ireland, vol. i. (Belfast Naturalists' Field Club).—Proceedings of the Linnean Society of New South Wales, 2nd series, vol. i., part iv. (Trübner).—*Challenger* Reports—Zoology, vol. xix. (Eyre and Spottiswoode).—Beobachtungen der Russischen Polarstation auf Nowaja Semlja, ii. Theil.; Meteorologische Beobachtungen: K. Andrejeff.—Beobachtungen der Russischen Polarstation an der Lenamündung, ii. Theil.; Meteorologische Beobachtungen, i. Lief.; Beobachtungen v. Jahre 1882–83: A. Eigner.—A Classification of Animals: E. T. Newton (Philip).—Botanische Jahrbucher für Systematik, Pflanzengeschichte und Pflanzengeographie, Achter Band, iv. Heft (Leipzig).—Journal of the Society of Telegraph-Engineers and Electricians, vol. xvi. No. 66 (Spon).—Journal of the Royal Agricultural Society of England, April (Murray).—Beiblätter zu den Annalen der Physik und Chemie, No. 4, 1887 (Leipzig).

CONTENTS.

THURSDAY, MAY 19, 1887.

LOCAL NAMES OF BRITISH BIRDS.

Provincial Names and Folk-Lore of British Birds. By the Rev. Charles Swainson, M.A. Published for the English Dialect Society. (London, 1885 [*sic*]).

The Folk-Lore and Provincial Names of British Birds. By the Rev. Charles Swainson, M.A. Published for the Folk-Lore Society. (London, 1886.)

INEPTITUDE for the performance of a literary task has long been held by some publishers to be no bar to a man's undertaking it; but we believe that hitherto this opinion has not been shared by publishing Societies. These bodies may not always have been fortunate in the selection of editors or authors; but in a general way it may be asserted that a grave mistake is seldom made. Such a mistake, however, it is our unhappy lot now to record, and it is the more marked in that it is common to two of them—the English Dialect Society and the Folk-Lore Society. Most of the publications of the former are everywhere recognized as possessing high value—some naturally are better than others; but each of them has reflected credit upon the Committee of that Society, formed as it is of some of the best English scholars, and its work has undeniably been of great use. With the publications of the latter the present writer must avow himself inadequately acquainted, though he is willing to accord to them a reputation not inferior to that which those of the sister Society enjoy. By what perverse fate, then, these two Societies have combined to intrust a subject so exceedingly interesting, and of which it was possible to make so much, as the "Provincial Names and Folk-Lore of British Birds," to a gentleman whose knowledge of it is obviously inadequate, is beyond the reviewer's power to explain. Perhaps it may be only one of those well-known results of divided responsibility which are almost invariably exhibited in statesmanship, generalship, and editorship. A more unsatisfactory work than that of which the double title stands above has seldom appeared on the counter of a careless publisher, and of this fact the Committee of the English Dialect Society seems, when too late, to have become aware; since its thirteenth Report, read at the annual meeting on February 14 last, contains what cannot be looked upon as otherwise than an apology for the course into which it was led. Here we read—

"Mr. Swainson's 'Provincial Names of British Birds' has been published in conjunction with the Folk-Lore Society, at whose instance it was undertaken. . . . The work is interesting, and the list of local names is the best yet published; but it is only right to point out that, in the catalogue given by Mr. Swainson of the books which he has consulted for the purposes of his compilation—about one hundred in all—not a single publication of the English Dialect Society is mentioned. This means, of course, that the words used in almost fifty counties or districts have been entirely overlooked and neglected. Several recent monographs on the ornithology of English counties, most of which contain the local names of the birds, are also omitted from Mr. Swainson's list. . . . It is obvious, therefore, that the Dialect Society, whilst acknowledging their indebtedness to Mr. Swainson for the work he has done, can only regard it as a partial and temporary treatment of the subject; and they will be pleased if they could induce Mr. Swainson or some other member to attempt the compilation of an exhaustive and final list of local bird-names."

This free acknowledgement goes far to exonerate the Committee from their offence, into committing which they would seem to have been dragged by the Folk-Lore Society. Whether the Council of the latter body has expressed itself in any corresponding terms, the present writer is not aware; but that some explanation is due to its members, if they are above caring for anything more than a parcel of old wives' fables indifferently told, is very clear.

That the compiler of a successful list of provincial names of birds should be somewhat of a philologist and somewhat of an ornithologist would seem to be obvious. There is little evidence to show that Mr. Charles Swainson is either one or the other, and a good deal to make us suspect that he is neither. Moreover, we cannot free ourselves from an uncomfortable thought that he has not personally consulted some of the works he quotes, for he certainly "makes hay" with their authors' names and the titles of the books, while he is not above citing a passage at second-hand from a popular author who may or may not have correctly reproduced the original passage—a passage that may or may not occur in any very rare or recondite volume. Furthermore, besides the omissions noticed in the Report just cited, there is a considerable amount of material available which has been wholly passed by. The earlier volumes of the *Zoologist* contain several lists of the local names of birds that seem to be unknown to him, and from those lists, and others there collected, it was many years ago fondly hoped that a gentleman—the Rev. J. C. Atkinson—who, by his later labours, has proved his efficiency, would have compiled a work having the same scope as that now before us.

It does not appear to have occurred to Mr. Swainson that a name has not only a locality, but a history, and that, though information concerning the locality in which it is used is very desirable, information concerning its history is more important still. In regard to the former, what he tells us is generally little enough; and in regard to the latter, what he tells us is generally nothing at all. More than this, the source of such information on either subject as he does vouchsafe is very rarely indicated—still more rarely than is done by M. Rolland, whose "Faune Populaire de France" (a very good book in its way, but one capable of great improvement) is confessedly the model which the work before us tries to copy.

To take the first species in Mr. Swainson's list (p. 1), which species, by the way, he calls by the corruptly abbreviated name "Missel Thrush." He writes of "the fondness of this bird for the berries of the mistletoe, holly, and holm." If he had looked at the careful "Dictionary of Plant-Names" of Messrs. Britten and Holland (published by this same English Dialect Society), or almost any British Flora, he might have seen that holly and holm are synonyms, instead of being, as he would have them, the names of different trees. He also tells us that among the names which this bird has received, from the harsh note it utters when alarmed, is that of "Screech"; but he gives not a hint to connect that word with its undoubted parent form, the Anglo-Saxon *Scric*, which is rendered *turdus* in the older vocabularies, and is, there can be little question, the early form also of "Shrike," though that was by one of Turner's friends applied to a wholly different kind of bird, which since 1544 has borne it, in books at any rate. Indeed this last species (*Lanius excubitor*) has a very doubtful claim to any English vernacular name at all, though its common congener may rejoice in that of Butcher-bird. But even under "Shrikes" we have no reference to the Anglo-Saxon *Scric*, and we may remark that here (p. 47) we find a passage, some four or five lines in length, inclosed in inverted commas, as though a quotation, and followed by "(Yarrell.)," as if the naturalist of that name were its author. Truth compels us to say that search in all the editions of Yarrell's classical work has failed to show that the so-printed quotation is anything but a paraphrase, and a very inadequate one, of the passage Yarrell wrote.

Coming to Mr. Swainson's second species, the Song-Thrush (p. 3), we find no attempt to trace the nice distinction which runs through more than one Teutonic tongue between Thrush and Throstle—the latter being the diminutive of the former, as Prof. Skeat's "Dictionary" shows, and our author is content to quote Macgillivray at second-hand from Mr. Harting, whereby an accidental error (slight, but enough to give the passage a wrong meaning) is repeated. Space fails us to criticize what else Mr. Swainson says of these two species alone, and of course it would be impossible to go through the whole of his volume in this way, even if we wished to do so. Suffice it to say that there is scarcely a page to which exception of one kind or another could not be taken, and now let us turn to the very end of the book. Here (p. 217) in what he says of the last but three of the species in his list we find the astounding statement that from the French word *Guillemot* (corresponding with the same in English) comes the Welsh *Guillem*! This is enough to make any patriotic inhabitant of the principality go off his head, for though doubtless the words have a common origin, the derivation, if such there be, must be the other way, and the modern French[1] *Guillemot* be the offspring of the Cymric and probably Breton *Gwillim*, or *Gwylym* as Pennant wrote it. On the next page, Mr. Swainson gives us a piece of information as curious and, we fear, unwarranted, telling us that the names Greenland Dove and the like (applied to what in books is called the Black—but here by accident misprinted "Lack"—Guillemot) are bestowed on account of "the great attachment shown to each other by the male and female, thus resembling the dove." Ornithologists knew that in one of its plumages the Tysty is very dove-like, but we think they did not know, and, if they believe it, will doubtless be thankful to Mr. Swainson for the news, that it is remarkable for conjugal affection! We should doubt whether the Rotch, or Little Auk, was ever called by Icelanders "*ålka*," for by that name, when properly spelt, they mean the Razorbill; and, arriving at the last bird in the list, we are concerned to find that the Puffin is "so called either from its puffed-out appearance, or from its swelling beak." Setting aside the fact that no one who has ever examined the compressed "coulterneb" of the Puffin could reasonably apply thereto the epithet "swelling," we may remark that the name seems to have been first applied to the young birds in their downy clothing, which, when salted and dried, were held in some estimation as an article of food, and were described by Gesner in 1555 ("Hist. Avium," pp. 110, 768), from an account furnished to him by Caius, as wanting true feathers, and being covered only with a sort of woolly black plumage—natural "powder-puffs," in fact. It is true that Caius himself, fifteen years later ("Rarior. Animal. Libellus," fol. 21) declared that the name is derived "a naturali voce pupin"; but that assertion will not be confirmed by those who know the bird in life. Mr. Swainson would no doubt have mentioned these particulars had he known them, and he might easily have found them out by searching for the earliest record of the species; but, as before remarked, investigation is a quality in which he appears to be singularly deficient.

There may be readers who will condone such blemishes as those of which we have given some half dozen instances out of—we should be sorry to say how many that we could notice. Several far more flagrant than those we have particularized have attracted attention elsewhere,[1] and are therefore purposely passed over by us; but before we leave the subject we should like to say a few words on the distinction which exists between *real* and what we may perhaps call *book* names—a distinction in no way heeded by our author. To use the phrase of Sir Hugh Evans, these last are "affectations." They may or may not be needful, they may or may not be apposite, and they may or may not be adopted into our language; but they are artificial grafts, and not its natural outgrowth. Consequently, from the linguistic and philological point of view, the difference between the two classes of names should be always most carefully drawn, and the more carefully since, in some cases, the child of adoption puts on an appearance amazingly like that of the child of generation. To show this difference an investigation of the history of names is needed; but that is not attempted by Mr. Swainson. Few persons would suspect that the name "Dipper," which of late years has in common use almost wholly ousted the Water-Ousel, Water-Crow, or Water-Pyot of former days, was not of very ancient origin, and referred to that bird's habit of diving below the surface of a stream in quest of its prey, as indeed is stated by Mr. Swainson (p. 30). Yet, directly we inquire into the history of the name, we shall be unable to trace it beyond 1804, when it was apparently introduced by the writer of the work known as "Bewick," and also find that it was applied because the bird "may be seen perched on the top of a stone in the midst of the torrent, in a continual dipping motion, or short courtesy often repeated." Here the need of explanation is all the greater, because this particular sense of the word "dip" or "dipping"—though familiar enough to our forefathers, has become almost obsolete, and, indeed, is passed over by Prof. Skeat. "Dipper," therefore, is nothing but a book-name. Then, again, under "Hedge-Sparrow"—a name which must last so long as Shakespeare is read—we have, amid half a dozen genuine local synonyms, "Hedge-warbler, Hedge-accentor, Hedge-chanter" brought in, as if they were ever employed except by a few crotchety writers, who tried to confine the use of the word Sparrow in

[1] In older French, *Guillemot* was applied to a Plover, as by Belon in 1555.

[1] *Athenæum*, March 19, 1887, pp. 386-387.

a way that not many English words will brook, and certainly not a word of such wide meaning and acceptance as this. In the same way the book-names of the Stone-Curlew—Thickknee, Norfolk Plover, and the rest, to the number of half a dozen—are gravely printed as if they were "provincial"; and here we may remark that Mr. Swainson applies (p. 200) the Arabic name of this species "*Karrawan*" (as he prints it) to its namesake the Long-billed Curlew or Whaup, which must be wholly unknown to most of the descendants of Ishmael.

We very much regret that we have to express ourselves in such terms of this book. We doubt not that the author has done the best that in him lies, and we are especially sorry to find from his preface that the delay in its appearance (for it had been long looked for) is due to his ill health. It is on this last account that we are indisposed to drive home many charges of carelessness which might easily be established, and we part from him trusting that in another edition he may have the opportunity of justifying his selection by these two learned Societies for the duties that we strongly suspect he must already regret having undertaken; but to do this he should acquire some knowledge of the ways and looks of birds, and learn the rudiments of etymology.

RECENT WORKS ON THE THEORY OF DETERMINANTS.

Primeiros Principios da Theoria dos Determinantes. Por J.-A. Albuquerque. (Porto, 1884.)

Die Determinanten in genetischer Behandlung. Von Adolf Sickenberger. (München, 1885.)

Vorlesungen über Invariantentheorie. Bd. I. *Determinanten.* Von Paul Gordan. (Leipzig, 1885.)

Elemente der Theorie der Determinanten. Von Paul Mansion. 2te vermehrte Auflage. (Leipzig, 1886.)

An Elementary Treatise on the Theory of Determinants. By Paul H. Hanus. (Boston, 1886.)

Beiträge zur Theorie der Determinanten. Von Wilhelm Schrader. (Halle, 1887.)

THREE of these works are introductory text-books of from fifty to eighty pages, and may consequently be dismissed in a few lines. The first is a skilfully arranged and well-written manual, furnished with suitable exercises, and ought to be found exceedingly serviceable in the secondary schools of Portugal. That by Prof. Mansion, of Ghent, has already been referred to in NATURE; the fact that it is now in the fourth French edition and second German edition is sufficient proof of its value. The third, by Gymnasial-Professor Sickenberger, is the largest and yet the most elementary, having been intended (not very wisely, we are disposed to think) for pupils very imperfectly prepared in algebra. Twelve pages, including a collection of thirty exercises, are devoted to determinants of the second order, thirty-eight pages to those of the third order, and the remaining thirty pages to determinants in general, the whole being prepared with endless pains and much preceptorial skill. Introductory works of this kind have for a number of years been appearing in Germany at the rate of 1.3.... per annum: in England we have not had one since 1875. Our insular way of doing things, however, is so different from that of the Germans that it may be fairly questioned whether we are any the worse for the deficiency. It would certainly be very erroneous to conclude that the advance of the theory of determinants in the two countries during the period referred to has shown the same marked contrast.

Gordan's "Vorlesungen" is a book on the lines of Salmon's "Modern Higher Algebra." The theory of determinants is consequently not taken up in its entirety, the design having been to give the main propositions regarding general determinants and to include the discussion of only those special forms which are connected with the chief subject of the work—to give, in fact, such a knowledge of determinants as would enable the student to prosecute investigation in the theory of invariants. It is nevertheless a very full exposition—much fuller than Salmon's, and much more methodical. The section on Permutation and Substitution should be carefully noted: although in essence it dates from the time of Cauchy, it will be none the less fresh to many readers.

"Beiträge" is rather a misnomer for the remaining German work on our list. The book is nothing more nor less than an ordinary, or very ordinary, *text-book*, containing three chapters, the first on determinants in general, the second on the adjugate determinant, and the third on determinants of special form. The author attaches considerable importance to a new notation which he introduces and uses throughout, and to various new theorems which he enunciates and proves; indeed, the title-page bears the intimation, "Neue Sätze und eine neue Bezeichnung." The said new notation is obtained by placing the lengthy but excellent umbral notation—

$$\left| \begin{matrix} 1, 2, 3, \ldots, n \\ 1, 2, 3, \ldots, n \end{matrix} \right|$$

atop of another notation, which is itself none too compact, viz. $D(a_{i,k})$, the outcome being

$$D(a_{i,k})^{1, 2, \ldots, n}_{1, 2, \ldots, n}$$

It is a little hard to see that this piling of Pelion on Ossa results in "grössere Einfachheit." As for the new theorems, we are satisfied that the author will change his mind in regard to them as his range of reading widens. All the results on pp. 98-111, for example, are perfectly well known in England and America: indeed, the whole of them which concern alternants of the third order are included in a single theorem of Prof. Woolsey Johnson's. None the less credit, however, is due to the author for the work he has done; and we trust that, having examined the literature of his subject, he will continue his investigations and attain a more enduring success.

The new American text-book stands out in marked contrast with the preceding. First of all, it is a book of good outward appearance; paper, printing, and binding being unexceptionable. In the second place, the author makes no pretensions to originality: in his preface he enumerates a few manuals, English and Continental, to which he is indebted, and frankly states that he has used them all freely. Some of them, we should say, he has used more freely than others, but, on the whole, with good judgment, and in such a way as to show that he possesses an independent grasp of the subject. The only notable instance of lack of insight is in the section on continuants, where two pages (pp. 179, 180) are unintentionally devoted to proving the theorem—

$$\frac{mA}{mB} = \frac{A}{B},$$

this very theorem itself being employed in the proof. Mr. Hanus will doubtless yet come to see that the book in which this originally appeared requires to be perused in a spirit of scepticism rather than of faith. We may note also that the identity (5) on p. 37 is exactly the same as (3) on the preceding page, that the footnote on p. 196 is misleading, and that the investigations referred to on p. 199 might with advantage have been further drawn upon. These latter, however, are small points which can be attended to in the second edition. The book, on the whole, is trustworthy, and well adapted for College use. On this account, and as being the first American text-book on the subject, it deserves a cordial welcome both in America and in Britain. THOMAS MUIR.

OUR BOOK SHELF.

The A B C of Modern Photography. 22nd Edition. (London: The London Stereoscopic and Photographic Company, 1887.)

ALTHOUGH this is called a new edition, it is really a new book, having been reconstructed and much new matter added. Those who are about to begin photography cannot do better than study and carry out the instructions which are here clearly stated. The book is divided into two parts.

In Part I. the beginner is taken through the whole process—exposing, developing, printing, &c.—and this is followed by tables of weights and measures.

Part II. contains good accounts of all the advanced parts of the art, such as re-touching, portraiture, &c., together with chapters on photo-micrography, instantaneous photography. One of the latest developments of photography is shown in the "detective book camera," which has the appearance of an octavo book of a thickness corresponding to about 200 pages. The new method of taking negatives on paper is fully described. Lastly, under the headings of "New Apparatus and Processes," Rayment's patent tripod top is mentioned, which allows the camera to be pointed in any direction, and also the patent photographic Gladstone bag, which is fitted up so as to contain a complete photographic outfit. We must not omit to say that the book is fully illustrated, the frontispiece being a photo-mezzotype taken by a pupil of the Stereoscopic Company.

Newcastle-upon-Tyne Public Libraries. Supplementary Catalogue of Books added to the Lending Department. (London: G. Norman and Son, 1887.)

IN this supplement the compiler has given nearly as much space to 10,000 volumes as was occupied by twice that number in the catalogue published in 1880 (see NATURE, vol. xxiii. p. 262). Most of the works have been published since 1880, but some earlier books have also been added. The rapid accumulation of knowledge makes it extremely difficult to provide adequate references to the subjects of pamphlets and of articles in treatises and serial publications. The compiler has, however, recognized the importance of this part of his work, and the results of the labour he has devoted to it will be of real service to students who may have occasion to consult the supplementary catalogue.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Thought without Words.

I DO not see that Prof. Max Müller's theory of the inseparability of thought from language, whether true or erroneous, has any important bearing on the origin of man, whether by evolution or otherwise. It is a question at all events to be studied by itself, and to be tested by such experiments as we can make by introspection, or by such facts as can be ascertained by outward observation.

My own opinion is strongly in favour of the conclusion urged by Mr. F. Galton. It seems to me quite certain that we can and do constantly think of things without thinking of any sound, or word, as designating them. Language seems to me to be necessary to the progress of thought, but not at all necessary to the mere act of thinking. It is a product of thought; an expression of it; a vehicle for the communication of it; a channel for the conveyance of it; and an embodiment which is essential to its growth and continuity. But it seems to me to be altogether erroneous to represent it as any inseparable part of cogitation. Monkeys and dogs are without true thought not because they are speechless; but they are speechless because they have no abstract ideas, and no true reasoning powers. In parrots the power of mere articulation exists sometimes in wonderful perfection. But parrots are no cleverer than many other birds which have no such power.

Man's vocal organs are correlated with his brain. Both are equally mysterious because they are co-operative, and yet separable, parts of one "plan." ARGYLL.

Argyll Lodge, Kensington, May 12.

HAVING much of the same experience as Mr. Galton, I nevertheless prefer dealing with a larger group of facts. I have often referred to the mutes of the seraglio at Constantinople, who cannot be charged with thinking in words. They have their own sign conversation among themselves, and which has no necessary reference to words. Even the names of individuals are suppressed among themselves, though they sometimes use lip reading to an outsider to make him understand a name. Anyone having a knowledge of sign language is aware that it is independent of words. The tenses of verbs, &c., are supplied by gestures.

The mutes are not deficient in intelligence. They take a great interest in politics, and have the earliest news. It is true this is obtained by hearing, though they are supposed to be deaf-mutes, but among themselves everything is transmitted by signs.

HYDE CLARKE.

32 St. George's Square, S.W., May 12.

I THINK that all who are engaged in mechanical work and planning will fully indorse what Mr. Francis Galton says as to thought being unaccompanied by words in the mental processes gone through. Having been all my life since school-days engaged in the practice of architecture and civil engineering, I can assure Prof. Max Müller that designing and invention are done entirely by mental pictures. It is, I find, the same with original geological thought—words are only an incumbrance. For the conveyance and accumulation of knowledge some sort of symbols are required, but it appears to me that spoken language or written words are not absolutely necessary, as other means of representing ideas could be contrived. In fact, words are in many cases so cumbersome that other methods *have* been devised for imparting knowledge. In mechanics the graphic method, for instance. T. MELLARD READE.

ON reading Mr. Galton's letter, I cannot help asking how Prof. Max Müller would account for early processes of thought in a deaf-mute: does he deny them? S. F. M. Q.

Scorpion Virus.

I AM much obliged to Sir J. Fayrer for pointing out a mistake in my paper on this subject in the Proceedings of the Royal Society of January 6, 1887. In referring to his experiments I remarked, "They show conclusively that the cobra poison will not affect a cobra, and will not even affect the viperine *ptyas*." "*Ptyas*" was written by mistake for "*Daboia*."

I take the opportunity of recording an observation with regard to the slight power scorpions possess of withstanding the heat of the sun's rays. If a scorpion is placed in an open pie-dish in the sun (the experiments were tried in Madras on an averagely hot day), it will run violently round and round, lash its sting about, and then gradually become torpid; this happens in from seven to ten minutes. If then removed into the shade, it will gradually recover; but if left for longer in the sun, it dies. As the scorpion is an inhabitant of hot countries, this sensitiveness to the sun's rays is very remarkable. A. G. BOURNE.

Madras, April 13.

Weight and Mass.

I FIND it convenient to distinguish in writing between mass and weight by using the symbols *gr.* or *kgr.* or *lb.* to denote masses, and reserving capital letters *Gr.*, *Kgr.*, &c., where weights, or forces in gravitation measure, are understood; say, 50 *kgr.* of stone, or wood, or iron—1000 *Kgr.* denoting a stress in some structure or the like. Some agreement in these notations would be desirable. W.

Lemberg, May 14.

Dynamical Units.

IN reference to this subject may I remark that the proposed term "cel" is etymologically incorrect for the meaning intended to be conveyed? It might stand as a contraction for "celerity," *i.e.* velocity, but not for the rate of *increase* of velocity. The essential distinction between velocity and acceleration is wholly expressed in the prefix "ac." If we must cut all our words down to one syllable, the "ac" would really have in it more correct meaning than the "cel."

Early in 1886, Prof. D. H. Marshall, of King's University, Kingston, published a book on dynamics, in which he uses the word "tach" to mean unit velocity of one centimetre per second. He has no special name for the unit of acceleration, but the unit of momentum he calls a "gramtach," and the unit rate of doing work a "dyntach." The unit pressure-intensity of one degree per square centimetre he calls a "prem."

I would like to suggest that names for the units might be formed systematically by the addition to the ordinary name for the quantity of the invariable affix "on," which is the root part of the word "one." Thus as unit names we would employ "velociton" or "velon"; "acceleron" or "accelon"; "momenton"; "presson"; "tenson," &c., &c. For the sake of uniformity we might change "radian" into "radion."

Birmingham, May 4. ROBERT H. SMITH.

Monkeys opening Oysters.

SO many people have expressed their surprise at hearing that I constantly saw monkeys breaking open oysters with a stone on the islands off South Burmah, that it may be of interest to give a short description of their method of using such a tool.

The low-water rocks of the islands of the Mergui Archipelago are covered with oysters, large and small. A monkey, probably *Macacus cynomolgus*, which infests these islands, prowls about the shore when the tide is low, opening the rock-oysters with a stone by striking the base of the upper valve until it dislocates and breaks up. He then extracts the oyster with his finger and thumb, occasionally putting his mouth straight to the broken shell.

On disturbing them, I generally found that they had selected a stone more apparently for convenience in handling than for its value as a hammer, and it was smaller in proportion to what a human being would have selected for a proportionate amount of work. In short, it was usually a stone they could get their fingers round. As the rocks crop up through the low-water mud, the stone had to be brought from high-water mark, this distance varying from 10 to 80 yards. This monkey has chosen the easiest way to open the rock-oyster, viz. to dislocate the valves by a blow on the base of the upper one, and to break the shell over the attaching muscle. The gibbon also frequents these islands, but I never saw one of them on the beach.

ALFRED CARPENTER.

Marine Survey Office, Bombay, April 14.

Zirconia.

OUR attention has been drawn to a letter in NATURE, vol. xxxv. p. 583, written by Mr. Lewis Wright. He makes the statement that we supplied him with a sample of zirconia as "pure," which, upon examination, he found to contain silica, as well as some soda, rendering the sample quite useless for the purpose for which it was required.

We trust you will allow us to correct this statement. We sold the zirconia as "impure," and when Mr. Wright asked us to purify it further for him, declined to do so. We told him that it was an article obtained as a residue produced during the preparation of another body, and was sold, in consequence, at a price far lower than the usual price at which the article can be produced in a pure state. HOPKIN AND WILLIAMS.

16 Cross Street, Hatton Garden, London, E.C.

Sunspots.

DR. VEEDER is perfectly correct in his letter appearing in NATURE, vol. xxxv. p. 584, in his description of the tiny group of spotlets which were seen on November 15, 16, and 17. The complete record of spots for the month of November 1886 appears to have been as follows, the areas of the spots being expressed, as in the Greenwich results, in millionths of the sun's visible hemisphere.

Date.	Number of spots.	Total area.
November 12	1	7
„ 13	1	9
„ 14	2	8
„ 15	1	1
„ 16	2	5
„ 17	2	12
„ 26	1	6

The mean daily area of these seven days, the only days in the month showing spots, was only 7 millionths, and for the month as a whole, 1·6 millionths. The exceptional character of the month will be better seen when it is remembered that the Greenwich results give 24 millionths as the mean daily area for 1878, the year of minimum; whilst at maximum, as in 1883, the mean daily area was 1155.

With reference to the "six days" which Dr. Veeder quotes from the note on "Solar Activity in 1886," appearing in the Astronomical Column of NATURE, vol. xxxv. p. 445, the assertion was based on a record which was defective for three or four days. The group he describes as making its first appearance on December 8 was not seen here until December 10, and had only become important by December 12. Since the appearance of Dr. Veeder's letter, I have been privileged to inspect the series of photographs taken in India and in the Mauritius, under the auspices of the Solar Physics Committee. These show that the group had not come into view at the east limb until after the photographs on December 8 had been taken, so that, for Europe and Asia, December 9, which was cloudy here, was practically the first day of the spot.

THE WRITER OF THE NOTE.

"The Game of Logic."

IN the course of a review of Lewis Carroll's "Game of Logic" (p. 3), Mr. A. Sidgwick says incidentally that "In Mr. Venn's scheme propositions either tell us that a compartment is empty or tell us nothing about it." This is not quite correct; he should have confined his statement to *universal* propositions. It is quite true that on the schemes of Boole and Jevons nothing is recognized but 0 and 1; nothing but the excision of a combination and the letting it stand; and they both make the attempt to express particular propositions with such resources. But I have taken particular pains to show that such a scheme of dichotomy will not suffice to represent affirmatives and negatives, universals and particulars; and that for this purpose, when we are dealing with logic on the compartmental theory, if we intend to grapple with every kind of proposition we require a threefold division. We must be able to show that a compartment is empty, that it is occupied, or that we do not know what is its state.

Speaking only of diagrammatic illustration, since it is to this that Mr. Sidgwick is referring, I may say that I have indicated in an article in *Mind* (1883, p. 599) how such a threefold scheme of alternatives could be displayed. Reference to this will be found also in Mr. Keynes's "Formal Logic."

I cannot ask for space to discuss the subject fully here. But I would remark that any scheme that confines itself to *two* alternatives seems to me to be necessarily open to one or other of two serious drawbacks. Either (1) we have to assume that the assertion of a proposition carries with it the existence of its subject. This begins plausibly enough; but when fully worked out it forces us to abandon various universally recognized rules, such as some of those for conversion, contraposition, &c. It also, in the case of complex propositions, departs even further from convention than the opposite doctrine does. And it wholly fails to express hypothetical propositions. I have pointed out these difficulties in my "Symbolic Logic," and the first of them will be found very fully treated by Mr. Keynes. Or (2) we may reject the assumption just mentioned, as Boole and Jevons practically do. We are then wholly unable (in spite of the attempts made by each of these writers) to express *particular* propositions. JOHN VENN.

Cambridge.

THE PARIS ASTRONOMICAL CONGRESS.

IN our last article we brought down our reference to the *procès-verbaux* as follows:—General Congress meeting, April 19; Photographic Committee meetings, April 20 and 21; Astronomical Committee meetings on the 20th, 21st, and 22nd. We have since then received from Admiral Mouchez the following records:—Third and fourth meetings of the Congress on the 23rd and 25th; meeting of the Permanent Committee on the 26th; and meeting of the Permanent Bureau on the 27th. The two final meetings of the Congress were held chiefly to receive the reports of the Astronomical and Photographic Sub-Committees which have been appointed, and to whose proceedings we have referred in detail, and also to appoint a Permanent Committee, and if necessary a Bureau.

It will be convenient, then, that we should commence by referring to the third and fourth general meetings held on the 23rd and 25th. This first meeting considered the various resolutions which had been arrived at by the Sub-Committees, and the discussions upon them do not appear to call for any more remarks upon our part. With regard to the construction of the object-glass for the light near G, the eminent optician, Steinheil, communicated a note which will appear among the records of the Congress, but of which no details are given in the *procès-verbal*. With regard to the supplementary negatives which are to be obtained for purposes of a catalogue of the stars of reference, the Astronomer-Royal was evidently under the impression that to endeavour to obtain stars of the eleventh magnitude might be going a little too far, and he therefore proposed that a resolution should give authority to the Permanent Commmittee to determine down to what magnitude, not beyond the eleventh, these photographs should include. The Astronomer-Royal also was evidently under the impression that the Congress had been called together to obtain a photographic autobiography or map of the heavens in this present century chiefly, and that it should not lay so much stress as the astronomers present were inclined to do upon a mere catalogue.

Mr. Gill, Her Majesty's Astronomer at the Cape, seems to be of a different opinion, as he remarked that it is necessary to make a catalogue if the thing is possible. The number of stars to appear in the catalogue if the eleventh magnitude is adopted will be about 1,500,000 according to M. Paul Henry, and according to Dr. Schoenfeld, if the 11·5 magnitude were adopted no less than 3,500,000 stars. Subsequently the matter was put to the vote, and the resolution as it came down from the Astronomical Section was approved, Mr. Christie's amendment being lost.

The Congress next passed on to consider the distribution of work among the different Observatories. MM. Beuf and Cruls, representing the Observatories of La Plata and Brazil, were the first to reply that they were ready, it being understood that the price of a telescope similar to that employed by the Brothers Henry would be something like 40,000 francs. Admiral Mouchez stated that it had been decided that the Observatories of Algiers, Bordeaux, Paris, and Toulouse would represent the part which the French Government would be prepared to take, and he also stated that in all probability the Observatory of Santiago in Chili could undertake some portion of the work. M. Weiss stated that the participation of the Vienna Observatory might be regarded as certain. MM. Tacchini and Oom, representing the Observatories of Rome and Lisbon, had little doubt that their Governments would furnish the requisite sums. M. Dunér stated that the Senate of the University of Helsingfors would contribute a photographic refractor, and he did not anticipate any difficulty with his Government. M. Struve had no instructions; M. Auwers was in the same case; Messrs. Christie and Gill followed suit. Mr. Russell declared that in his opinion the necessary funds would be provided to enable the co-operation of the Observatories of Melbourne and Sydney to be assured. Mr. Peters said that he had no doubt that there would be ten Observatories in America anxious to help in the work, but he did not know if they would accept all the terms and conditions of the resolutions of the Congress. M. Pujazon, representing Cadiz, thought that he could promise assistance towards the map, but he could promise nothing relating to the catalogue.

M. Foli, representing Brussels, then proposed the following resolution:—

"If an astronomer takes, by means of a telescope different to that suggested by the Committee, photographs which fulfil the conditions laid down for the map, he shall be able, with the concurrence of the Permanent Committee, to join in the execution of it."

This was subsequently withdrawn.

The final meeting of the Congress was held on the 25th, and the point first discussed was that relating to the Permanent Committee. M. Knobel proposed, on behalf of some of the French astronomers, the following resolution:—

"That there should be two categories of members in the Permanent Committee—first, the Directors of Observatories where the work is carried on; and secondly, others not necessarily taking part in the construction of the map. This Permanent Committee should name its own Bureau, consisting of a President, two Vice-Presidents, and two Secretaries."

Next followed a discussion as to the number of members of which the Committee should consist. A considerable difference of opinion was made manifest by the remarks of many members of the Committee, and Admiral Mouchez, with the apparent intention of coming to an agreement, proposed the following series of resolutions:—

(1) "Before separating, the International Congress shall delegate its powers to a Commission of eleven members, forming an Executive Commission, charged to study all the questions which have been referred to, and to hasten the preparations for the execution of the map as much as possible. The Directors of Observatories

who shall obtain from their Governments instruments constructed according to the decisions adopted by the Congress shall take part in the work and be members of the Commission."

(2) "The Executive Commission shall meet once a year in one of the cities named beforehand where one of the Observatories is situated. Those members who cannot take part in this reunion shall forward their remarks upon the principal questions to be discussed, of which the President of the Commission shall give notice one or two months beforehand."

(3) "Between these annual meetings, the President should keep himself in communication with the members, and receive or give advice on all matters touching the preparation or execution of the work and the progress already effected."

(4) "The division of the heavens between the different Observatories, and all questions not settled by the Congress, must be fully studied in advance, so that they may be decided in a definite way, at least in two years' time, at a meeting in 1889, by which period many instruments will be ready to begin work."

On the occasion of the Universal Exhibition, Paris will probably be the city most convenient for this second meeting.

(5) "While the map is in progress, each Director who takes part in the work shall send to the Permanent Committee one or two months before each meeting, stating the work done, together with any remarks which he may consider necessary."

(6) "The *procès-verbaux* of these annual meetings, and the resolution of all work done, shall be published regularly, and sent to each Observatory."

The President, M. Struve, asked if M. Knobel would withdraw his resolution in favour of that of Admiral Mouchez. Finally, after some discussion, part of M. Knobel's resolution was carried, all except that portion relating to the constitution of the Bureau.

It was next determined that the number of members of the Permanent Committee, beyond those Directors of Observatories who have declared their readiness to join at once, should be eleven. The members elected were Christie, Dunér, Gill, Paul Henry, Janssen, Lœwy, Pickering, Struve, Tacchini, Vogel, Weiss.

The following resolution was next carried, proposed by M. Auwers:—

"The Congress resolves that it is desirable to appoint a Sub-Committee occupying itself with the application of photography to astronomy, other than the construction of the map, showing the importance of all these applications and the relations which it is important to establish between these different kinds of work. This Committee should place itself in relation with the Permanent Committee. The Congress desires that MM. Common and Janssen be charged to carry out this resolution."

The thanks of the Congress to the French Government, Admiral Mouchez, and the President, M. Struve, brought the Congress to a close.

The first meeting of the Permanent Committee was held on April 26, M. Struve in the chair; M. Trépied was requested to act as Secretary. Much time was spent in discussing whether a Bureau or Sub-Committee should be appointed, although the relation of this Bureau or Sub-Committee to the Permanent Bureau appointed at the last meeting of the Congress does not come out very clearly. Admiral Mouchez stated that the Bureau to be elected at the present meeting was rather an Executive Committee than a deliberative one. This was generally agreed to. The composition of the Bureau was then fixed at one President, five Members, and three Secretaries, and the members elected were as follow:—President: Admiral Mouchez; Members: Messrs. Struve, Christie, Tacchini, Dunér, and Janssen; Secretaries: Gill, Vogel, and Lœwy. M. Vogel then suggested that his colleagues on the Permanent Committee should present to the Bureau before July 1 next any propositions they might have, concerning experiments to be undertaken, and other preparatory work. He suggested also that the Bureau should be allowed to confide directly to men of science, possessing special knowledge, some of the researches which the International Congress has left to the Permanent Committee the responsibity of undertaking and directing.

After discussion of all the documents thus received, a definite plan of preparatory work might be elaborated and distributed among the different Observatories. Admiral Mouchez then stated that the Academy of Sciences would bear all the expenses of printing connected with the work, and it was also agreed that all documents should be sent in French. The Astronomer-Royal was requested to undertake experiments with curved plates. M. Struve promised in a month's time to forward suggestions relating to the methods of proceeding to be adopted by the Bureau. It was further decided that all memoirs and communications should be addressed to the President, and distributed by him and the Secretaries residing in Paris.

The last question discussed was the probable number of Observatories. Mr. Gill remarked that the small number of Observatories in the southern hemisphere were almost all situated in the same latitude, and he suggested that if France would establish a new Observatory in the Island of Réunion or New Caledonia, probably the Government of New Zealand might establish another in that colony. The general opinion was that the Island of Réunion would be better than New Caledonia as a station, and that the Observatory in New Zealand should be built in latitude about 48° S. The following resolution was unanimously adopted:—

"The Permanent Committee of the International Astronomical Conference met together for the construction of a photographic chart of the heavens, and finding that the number of Observatories in the southern hemisphere was insufficient for the good and prompt execution of the work, expressed a desire that two new Observatories might be erected, at least as a temporary measure, one in New Zealand, the other in the Island of Réunion."

The President was charged with the duty of transmitting this resolution to the English and French Governments.

Finally, we come to the Bureau of the Permanent Committee, which held its first sitting on April 27, M. Struve being President.

Mr. Gill announced that Sir James Anderson, Director of the Eastern Telegraph Company, has provisionally authorized the exchange of free telegrams between the Cape and Paris. Thanks were unanimously voted to Sir James Anderson for this offer. All members of the Committee were requested to reply within a month to any question addressed to them, and six was made a quorum. Mr. Gill volunteered to draw up complete instructions regarding all photographic operations. The assistants appointed to conduct the photographic work in the Observatories now taking up photography for the first time are to be trained in Observatories where photographic work is already carried on.

The institution of a series of test objects was then agreed to, and Messrs. Gill, Vogel, and Henry, were requested to draw up a list. On the proposal of Admiral Mouchez, the following distribution of the experimental work was agreed to:—

(1) Systems of cross-wires.—M. Vogel.

(2) Photographic magnitudes.—Messrs. Struve and Pickering.

(3) Optical determinations of images by means of photographs supplied by the Brothers Henry.—M. Struve.

(4) The study of three or four stars nearly in a straight line embracing the total angular distance of about 1°, and photographed necessarily at the centre and at the corner of a plate.—Paris, Algiers, Pulkowa, and Leyden.

(5) Study of the deformations of films.—Algiers, Meudon, and Potsdam.

(6) Study of curved plates from the triple point of view of construction, means of covering with a film, and measures.—Mr. Christie.

(7) Study of absolute orientation—that is to say, the mounting of the plates in the photographic telescope.—The Cape and Paris.

(8) Study of the measuring-instruments to be applied for the future utilization of negatives.—This was postponed.

(9) The study of formulæ for the preparation of plates in accordance with the general rules laid down by the Conference—Messrs. Abney and Eder.

(10) Opinions of colours of stars on their photographic magnitudes.—M. Dunér.

THE TEMPERATURE OF THE CLYDE SEA-AREA.[1]

II.

FROM the curves for each station, temperature sections were constructed for every cruise, showing the position of the isotherms with relation to a profile of the bottom along certain lines. It is not easy to give an intelligible description of the distribution of temperature without reference to those diagrams; but an attempt may be made. The most important section runs from the Channel, across the Plateau, up the Kilbrennan Sound branch of the Arran Basin, through Inchmarnoch Water, to the head of Loch Fyne. It is sufficient to recollect that the Plateau is covered by about 25 fathoms of water, that the depth increases on the inside up to 107 fathoms off Skate Island, then diminishes rapidly to 15 fathoms at Otter and Minard, and increases again to nearly 80 in Upper Loch Fyne. The section is a little more than 90 miles long.

In April the whole section was filled with water between 41°·3 and 44°. The water of the Channel, the Plateau, and the surface layers (to 10 or 20 fathoms) was above 42°. The average bottom temperature was 41°·3, except in the Channel (42°), and in Upper Loch Fyne (41°·9). The June section shows marked surface heating to a depth of about 5 fathoms. Water at 47°·5 filled the Channel, covered the Plateau, and extended in a layer of about 5 fathoms thick over the inner reaches. The great mass of water was between 44°·5 and 44°. In Upper Loch Fyne the remarkable distribution of temperature, referred to when discussing the curves for Strachur, was found to extend from Minard to the head of the Loch, in the form of a lenticular mass of water of temperature under 44°, with warmer water above and below. The minimum temperature, 42°, was found off Inveraray at a depth of 30 fathoms, and the gradient of temperature was much steeper in the upper layers of the cold mass than in the lower. No satisfactory explanation of the mode of formation of this intermediate minimum of temperature has yet been arrived at, and any suggestions as to its origin would be received with interest. In August the section shows that the cold mass remained in the same position but with a rather higher temperature, and of much smaller dimensions. As in previous months, the warmest water was that nearest the Atlantic, which had a temperature of over 53°. The great Arran Basin presented a considerable range; from 54° on the surface to 50° at 20 fathoms, 48° at 30, 46° at 60, and 45°·3 on the bottom. The September cruise showed a very similar state of matters, accompanied by a general rise of temperature and an increase in thickness of the warmer layers. As in each previous month, the Channel was warmest (54°·5 throughout), and the warm surface layer became thinner and thinner until at Otter the surface temperature was under 53°. The section clearly shows, what careful experiments have proved, that the abrupt rise of the sea-bottom, from off Skate Island at 107 fathoms to Otter at 15, is characterised by a rise of colder water from beneath to the surface. The gradient at this place is 550 feet in ten sea-miles, or 1 in 100; and perhaps vertical circulation is set up as much by the sudden narrowing of the Channel, as by its shoaling. A similar effect was observed at Row Point in the Gareloch, and at the narrowest part of the Kyles of Bute. In September the bottom temperature of the Arran Basin was 47°·5, that of Upper Loch Fyne 44°·2; the intermediate minimum had disappeared from the latter. November showed the influence of surface cooling in a marked degree. The Channel and Plateau had cooled down to 50°, and for the Arran Basin the average surface temperature was 49°·5, that at the bottom 51°·5. This shows a great equalisation of temperature, and a reversal of the summer conditions, the warmer water being now below, the cooler on the surface. In Upper Loch Fyne the temperature was 44° at surface and bottom, but a maximum of a little over 50° was found at 15 fathoms. Further cooling and greater equalisation of temperature characterised December; the Channel was warmest, at 48°·5; the whole Arran Basin varied from 46°·8 on the surface to 47°·5 on the bottom; and Loch Fyne maintained its independent position by a quite new arrangement of temperature-layers. On the days of our work there (December 29 to 31) the whole upper part of the Loch was covered with a sheet of frozen fresh water, the ice being nearly half an inch thick in places. Three inches beneath the ice the temperature was 36°, and a few feet under, it was 44°. The maximum temperature of 47°·5 was met at 20 fathoms; and the warm layer of water was giving out its heat to the superficial strata, being cooled by this winter's cold, and to the lower mass which still retained the cold of last winter, although the bottom temperature had risen about half a degree since November. In February it was impossible to observe in the Channel on account of bad weather, but the water on the Plateau was slightly colder (43°·4) than that in the Arran Basin (43°·7 to 44°). There was little range of temperature, the surface being in all cases, however, slightly colder. Throughout the Arran Basin the temperature of the mass of water was the same as in June: this may be held as pointing to the end of April as the period of minimum. Loch Fyne showed a steady rise of temperature as the depth increased down to 45 fathoms, where the thermometer registered 46°·5; from that point to the bottom there was a fall to 45°·8.

Dividing the Clyde sea-area into three parts, each comprising regions of like physical configuration, the direction of the annual march of temperature may be summed up thus.

Starting from the simple case of a minimum uniform distribution, the Channel heats uniformly up to September, and then cools uniformly; the strong tidal currents, or some other cause, keeping the water thoroughly mixed, and equalising all heat transactions.

The deep open basins, to which the tide has free access, heat up most rapidly on the surface, and more uniformly lower down; the mass which heats uniformly decreases until at the period of maximum there is an unbroken fall of temperature from surface to bottom, and a considerable range. Then, at the autumnal equinox, the surface water begins to cool, while summer heat is still travelling downwards: this leads to the typical winter state—exactly complementary to the summer condition—of a uniform gradient of temperature rising from surface to bottom, but with a slight range. As winter goes

[1] Continued from p. 39.

on, the rate of cooling becomes more nearly equalised, and on approaching the spring minimum the whole mass of water is at one temperature, and cooling steadily throughout.

The deep inclosed basins differ from the deep open basins only in degree; but in the same direction as the deep open basins differ from the Channel. On this matter I do not care to speak with so much certainty; as, the conditions in the inclosed basins being much more complicated, there is more probability there than elsewhere of local and temporary disturbances being mistaken for the normal progress of events. It appears, however, that summer heating takes place more slowly throughout the mass, although the surface maximum is earlier; and that in the deep, comparatively still water there may be at one time the conjoint effects of more than one summer and winter.

One step further in the direction of conditioning the phenomena of temperature in water is to entirely cut off even superficial tidal communication with the ocean; to form, in fact, a deep inland lake. Observations made by Mr. Buchanan, Mr. Morrison, and myself, on Loch Lomond and Loch Katrine, show that there the annual march of temperature is very much what might be expected from the Clyde observations; but there is the great difference of the water being fresh, and having a maximum density-point varying with the depth, which prevents a rigid comparison being made.

From the temperature sections, which have been described, the average temperature of the whole mass of water for each trip was deduced, by measuring the areas occupied by each range of 2°, multiplying these by their respective mean temperatures, adding the results together, and dividing by the number representing the whole area of the section. In order to ascertain the temperature of the surface water, that of the superficial 2 fathoms was calculated in the same way. By the kindness of Mr. Buchan, of the Scottish Meteorological Society, I was supplied with the mean monthly air temperature (average of twenty-four years) of the Clyde sea-area, and the deviations from the average for each month from January 1886 to February 1887. The figures are given in the accompanying table, and are expressed graphically by curves in Fig. 4.

MEAN MONTHLY TEMPERATURES 1886–87.

	January	February	March	April	May	June	July	August	September	October	November	December	January	February
	°	°	°	°	°	°	°	°	°	°	°	°	°	°
Mean air temperature ...	39·5	40·0	41·5	46·0	50·5	56·0	58·0	58·0	54·5	48·5	42·5	40·0	39·5	40·0
Deviation for 1886–87 ...	−3·5	−4·0	−2·5	−1·5	−2·5	−2·5	−1·0	−1·0	−0·5	+2·5	+2·5	−4·0	−2·5	—
Temperature of 2 surface fathoms ...	—	—	—	43·8	—	49·6	—	53·4	53·0	—	49·5	45·6	—	42·5
Average temperature of water ...	—	—	—	41·7	—	44·8	—	48·7	52·0	—	50·6	46·7	—	44·0

The temperature of water is not the monthly mean, as in the case of air, but that at the time when observations were made.

This shows that the year in which our observations have been made is rather an unfortunate one; because the low temperature of spring and summer, and the high temperature of autumn tended to retard the heating and the cooling of the water so as to produce a curve much flatter than the normal one may be expected to be. The maximum of air temperature occurred between July and August, that of the surface water between August and September, and of the whole mass of water apparently in October. The air and the whole mass of water from surface to bottom had the same mean temperature about the beginning of October; after that date the water remained warmer than the air, and the whole mass of water than the 2 superficial fathoms. It is specially noticeable that, while during heating the surface water is far above the main mass in temperature, it is only a very little below it during cooling.

Knowing the mass of water in the sea-area under consideration, it is easy to convert the temperature data into terms of heat; and, using for convenience the unit of one ton of sea-water raised 1° Fahrenheit in temperature, the following table expresses the actual changes taking place:—

QUANTITY OF HEAT.

Trip.	Commenced	Ended	Interval from last	Change of mean temp.	Average change per day	Total change of heat	Average change of heat per day
			days	°	°	millions	millions
April ...	13th	21st	—	—	—	—	—
June ...	16th	22nd	63	+3·1	+0·05	+465,000	+ 7,000
August ...	4th	12th	50	+3·9	+0·08	+585,000	+11,600
September ...	22nd	29th	49	+3·3	+0·07	+495,000	+10,000
November ...	11th	19th	50	−1·4	−0·03	−210,000	− 4,000
December. ...	23rd	31st	42	−3·9	−0·09	−585,000	−14,000
February ...	3rd	12th	42	−2·7	−0·07	−405,000	− 9,600

To summarise the above and give an account of heat transactions, it is sufficient to say that from April to September there was a gain of 1,545,000 million ton-degrees, corresponding to a rise in average temperature of 10°·3; while from September to February there was a loss of 1,200,000 million ton-degrees, corresponding to a fall in average temperature of 8°·0, thus leaving 343,000 million ton-degrees of heat to be expended by April next, supposing the water to return to the state in which it was

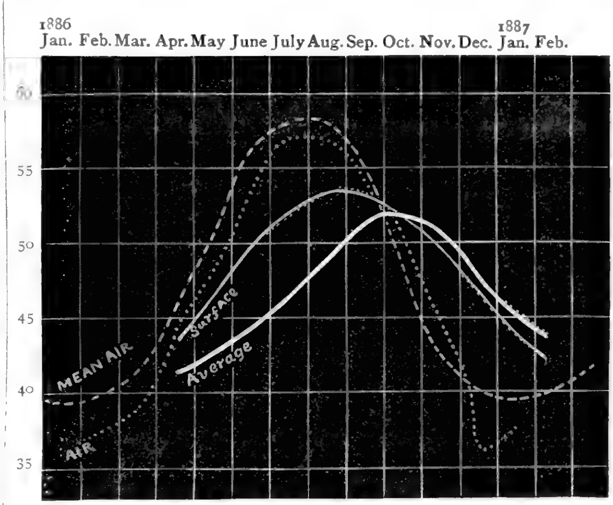


FIG. 4.—Clyde Sea-Area. Annual march of temperature.

in April last. These numbers for heat are of course based to a considerable extent on assumptions, and must only be taken as part of the preliminary discussion of the exact observations already recorded.

Observations made from March 25 to April 3, since the greater part of this paper was in type, show a general temperature of about 43°·5 over the whole area, and confirm all the provisional conclusions stated above. The figures observed in the three typical positions are as follow:—

Place ...	Channel.	Off Skate Island.	Strachur.
Date ...	March 30.	March 28.	March 29.
Temp. surface ...	44°·7	43°·8	44°·9
,, bottom ...	44·2	43·9	45·5

From the forms of the curves the spring minimum appears to be past, and over all the temperature is about 2°·5 higher than at the same period last year. The water in Upper Loch Fyne is now cooling at the bottom and heating again on the surface, the range for the year at great depths having been only 4°. The actual change of temperature in the sea-area between the beginning of February and the end of March is very slight, but it is significant in showing by its direction that the period of minimum lay between the two.

One interesting application of the observations may be made to climatology. A great proportion of the heat gained is derived not from solar radiation, or the contact of heated air with the surface, but from the warm Atlantic water entering by the tides. Since the water on the Plateau appears to remain warmer than that inside all the year round, no heat is lost to the Atlantic in winter; but all must be radiated off from the surface or employed in evaporating water or heating air by contact, and in this way more heat is returned to the air of the Clyde sea-area in winter than was received from it in summer. Another observation may be mentioned which serves to show how important a bearing temperature observations may have on biology. On February 4, four tow-nets were used off Strachur at different depths: one at 70 fathoms, one at 50, one at 30 fathoms deep, and the fourth at the surface. There was nothing in the surface-net, and the surface temperature was 43°. The contents of the other three nets were examined by Mr. David Robertson, of Millport, who reports:—"In all three nets Copepoda were moderately abundant. The nets at 70 and 30 fathoms contained one and the same species; but the contents of the net at the middle depth were different, confined to an abundant species of copepod loaded with ova (*Euchæta norvegica*). With them there were two or three adult schizopods (*Nyctiphanes norvegica*)." At 70 fathoms the temperature was 45°·9, at 30 fathoms 45°·6, and at the position of the middle net 46°·3. Mr. Robertson concludes: "As the middle water of the loch at this time is shown to be warmer than either the layer above or below, we may reasonably assume that the species in ova sought the warmer layer." Similar observations repeated at many different places during the March trip showed the same result, the minute Crustacea being most abundant where the temperature was highest.

The work is being carried on meanwhile, purely as a piece of physical and meteorological research, and a considerable time must necessarily elapse before all the latent meaning of the great mass of figures now being accumulated can be brought to light. There is no doubt that when the problem of the interchanges of heat in comparatively deep water has been made out, important practical applications to other sciences, and to some arts and industries, will be discovered.

HUGH ROBERT MILL.

SCIENCE AND GUNNERY.[1]

II.

LAST week we pointed out the great advantages which accrue from retiring guns behind inconspicuous parapets, and mentioned that the energy of the discharge had been utilised to raise the guns again into the firing position without the aid of extraneous power.

[1] Continued from p. 37.

The theory of the discharge of cannon involves many interesting considerations, not only with respect to the strength and structure of the guns but also with reference to the force required to control the recoil. A gun may be considered as a heat engine of the simplest construction, performing its work in one stroke. The fuel used is gunpowder, and the energy developed is, as in other engines of this class, in proportion to the weight of fuel used and to the heat it is capable of developing. The main difference between explosives and most other fuels is that explosives are complete in themselves; that is to say, they burn independently of the presence of extraneous bodies, and that consequently the chemical union which causes the explosion takes place simultaneously throughout the mass and in an exceedingly short time.

Fuel in large masses burns slowly because the air, which forms its complement, can come into contact with only limited surfaces, but if reduced to fine powder the combustion may be made to assume almost the intensity of an explosion, as for example in the dust-fuel used in Crampton's furnace, and the dusty atmosphere of coal-mines and flour-mills.

The materials in gunpowder, intimately mixed throughout, are in a state of unstable equilibrium with respect to each other; a very moderate increase to the thermal movement of the molecules causes them to clash together with sufficient energy to insure combination, and if such increase of motion be communicated to one portion of the explosive by the application of percussion or of a hot body, it is carried through the mass by the luminiferous ether with all the rapidity with which radiant energy travels, and the increase of motion, sufficient to cause combination, is communicated to every molecule nearly simultaneously, the consequence being a change of form and volume produced with the suddenness which marks an explosion. We believe that Mr. Anderson was the first, in his lectures on heat at the Society of Arts, to point out that it is unfair to compare the calorific value of fuels in their incomplete form; that is to say, that such fuels as require air for combustion should have the necessary weight of air added to them, and when that was done the singular fact appeared that the quantity of heat evolved by most combustibles per unit of weight was very nearly the same; thus in nine cases cited, which included coal, coke, wood, petroleum, illuminating gas, and gunpowder, the extreme variations from the mean calorific value did not exceed 9 per cent. In the same lectures it was shown that in guns, as in most heat-engines, a very large proportion of the thermal energy of the fuel was dissipated in a useless manner; in the case of cannon more than half was wasted in heating up the gun, and about one-third only in producing recoil, which was the reaction to the energy communicated to the shot, to that imparted to the powder gases, and to the work of displacing the atmosphere. Of these three effects only the energy imparted to the shot was known with precision, for by means of sufficiently simple apparatus it was possible to determine with great accuracy the velocity with which the projectile left the gun, and the energy therefore was easily determined by multiplying half its mass by the square of that velocity.

The determination of the work done in expelling the powder gases was more difficult to estimate. In the first place, only about 43 per cent. of the products of the combustion of gunpowder are in the state of gas, the remaining 57 per cent. are in the form of very finely-divided solids; next, the combustion goes on nearly all the time that the shot is travelling out of the gun, the pebbles of powder igniting in succession, a fact which is proved by the circumstance that in short guns a good deal of powder is blown out without being consumed at all, and it is doubtful even whether in the modern long guns combustion is always complete. While the shot is travelling along the chase, the centre of gravity of the powder charge is moving also at an uncertain rate, but the moment the shot leaves the gun the whole of the products of combustion appear to spring out with a velocity equal to, if not greater than, that of the shot. The evidence of this supposition is found in the fact that in the case of disappearing guns fired with their muzzles close to a masonry parapet, and in which the recoil below it is completed in a small fraction of a second, no blackening of the masonry is noticeable. A portion of the gases follow the shot and keep up with it for a considerable distance, as is shown by the circumstance that smoke issues plentifully from the earth banks into which proof shots at short range are fired, proving that the smoke of the discharge must have followed the shot into the tunnel momentarily made and as quickly obliterated by passage of the projectile through the earth. It is evident that the velocity with which the gases issue must depend upon the pressure in the gun at the moment of the shot leaving the muzzle, and this pressure again depends upon the volume of the bore, the weight of powder consumed, and the final temperature, the latter depending partly upon the expansion and the consequent heat converted into work.

The final temperature of the gases can only be conjectured: it probably does not exceed a bright red heat, or between 1200° C. and 1400° C. absolute; and knowing that one pound of powder at 0° C. and standard barometer develops about 4·48 cubic feet of gas, it is possible to estimate what the final pressure in the gun should be. Given, however, a barrel full of gas at a definite pressure, we are not in a condition to say what energy its expulsion would generate; and the assumption that the mean velocity will be that due to a body falling from a height equal to that of a column of gas of uniform maximum density which would correspond to the observed pressure would probably be as accurate as any other. On that assumption the velocity of the gas would be 4544 times the square root of the product of the final pressure in the bore in tons per square inch into its volume in cubic feet divided by the weight of the powder in pounds, and, this velocity determined, the energy is, of course, at once arrived at.

The displacement of the atmosphere also forms a very considerable item. The expansion on leaving the gun being instantaneous, the pressures and temperatures fall approximately as in adiabatic expansion; hence it is easy to calculate what probable temperature and consequent volume the gases will assume as they stream out of the gun, and this temperature is comparatively low; otherwise powder smoke would be insupportable to those feeling its influence close to a gun. The work done in displacing the air is found by multiplying the volume pushed aside by the atmospheric pressure. A small portion of this work is performed as the shot travels along the chase, but the greater part is done after it leaves the muzzle. The energy of the reaction to the sudden liberation of gases under high pressure is but too familiar to us in the case of boiler explosions, in which it commonly happens that great masses of material are hurled with destructive force and often to great distances.

The pressure-curve inside the gun is still very ill-defined; the forms commonly given are certainly a long way from the truth, because the areas included, which form indicator diagrams representing the work done, will not account for the energy developed. The pressure probably falls in proportion to the distance travelled by the shot, and the time in which the discharge takes place may be calculated on that assumption, or even with sufficient accuracy on the supposition that the velocity of the shot is uniformly accelerated as from the action of a constant force equal to the mean pressure producing the known velocity of the shot in a known distance. The easiest way to take account of all the forces causing recoil is to ascertain the velocity of the combined powder and projectile which will possess the total energy of discharge calculated, and then to equate the momentum of the gun and moving parts of the carriage to that of the shot and powder. In the case of a carriage receding along a slide, this operation is a very easy one, but when a Moncrieff mounting has to be dealt with, the case becomes very complicated, the gun moves along a curved path, the sides and counterweights have a rolling motion, and it becomes necessary to calculate the path of the centre of gyration, and determine the virtual weight concentrated in the gun, and a similar process has to be followed in the case of the massive levers which carry the guns in hydro-pneumatic mountings.

Recoil consists of two parts: first, the period, a very brief one, in which the velocity of recoil is got up; and secondly, the period in which the energy so acquired by the parts in motion is more slowly absorbed or dissipated. The first part of recoil must necessarily occupy the same time as the discharge, that is to say, a small fraction of a second, because acceleration can only go on so long as the accelerating force is acting, but that force is the pressure of the powder gases on the base of the bore, and the pressure only lasts while the discharge is taking place. The motion of the whole system of gun and carriage does not, however, coincide with the motion of the shot. In all but very long guns the shot has left the barrel before the motion of the muzzle commences; during the time of discharge, perhaps the 1/50 part of a second, the gun is being stretched by the inertia of its forward end and of the carriage resisting the tendency to put them into motion, but the reaction to this stretching carries on the acceleration of recoil a little after the shot has left the gun. The pressure on the parts during this period is very severe, the work done being exactly the same as that performed by the shot, the powder, and the displacement of the atmosphere. The full speed of recoil is attained, not only in a very short time, but in a very restricted space, rarely more than 3 inches, and the difficulty in constructing carriages may be said to lie in providing for the violent strains which produce a velocity of some 20 feet a second in the great mass of the gun and carriage in the exceeding short time and space named. The momentum of the moving parts of the system being equal to that of the ejected charge, the velocity is readily calculated, and generally ranges between 16 and 30 feet per second, and their energy is then easily ascertained.

In the counterweight Moncrieff carriages, which have been made for short muzzle-loaders up to 9-inch calibre and twelve tons weight, the whole mass set in motion is so great compared with the energy of the discharge, that the gun sinks below the parapet with a comparatively slow and stately movement; but, with the long breech-loaders and heavy charges, with the comparatively light moving parts which characterise hydro-pneumatic mountings, the motion is very violent, and requires great strength in the parts to resist the strains. In addition, the gun describes a circular path, and by the time the maximum velocity of recoil is attained, sufficient centrifugal force is engendered to produce a sudden upward pull, which has to be met by arrangements for holding the carriage down to the masonry of the emplacements. The longer the arms which carry the gun, the less this tendency is, because the pull of centrifugal force is inversely as the length of the radius of the curve described by the trunnions. The front of the carriage has generally to be held down for another reason. The gun, when fired, is high above the base of the mounting; the mechanism, self-contained in the carriage, for absorbing recoil, offers a certain amount of resistance to the backward movement of the gun, hence a couple is established which tends to turn the carriage over on its rear wheels, and this tendency varies with the height of the gun and the length of the base.

In the hydro-pneumatic system the fall of the gun actuates a ram or piston working in a cylinder full of water, and communicating by an automatic valve, opening outwards from the cylinder, with an air-vessel about two and a half times the capacity of the ram, and filled with air compressed to a degree sufficient not only to support the weight of the gun, but also to raise it quickly into the firing position. When the gun is up, the air-vessel is nearly empty; when down, a volume of water equal to that of the ram displaces the air and increases its pressure, and the ratio of the fall of the gun to the stroke of the ram, and the relative velocities of the two, are so adusted that the increase of air-pressure corresponds to the increasing leverage which the gun acquires as it descends.

It is possible to provide sufficient air-pressure not only to arrest the fall of the gun, but also to absorb the energy of recoil; but unless the gun is allowed to fall a very great distance this is not necessary, and any excess energy can be more conveniently absorbed by regulating the opening of the recoil-valve so as to throttle the water in its passage from the cylinder into the air-vessel. At first sight it might be assumed that, saving friction of the mechanism, the air-pressure which would suffice to check the fall of the gun would be sufficient to raise it again; but a little consideration will show that this is not the case. To allow the gun to fall in the short space of time during which recoil takes place, the pressure of the air must be less than that necessary to support the gun, because its pressure rises nearly according to the ordinates of an adiabatic curve, the temperature rising in exact proportion to the work done. During the time the gun is being loaded, the heat developed in the air is dissipated, so that when the gun requires to be raised the store of heat is gone, and the air, expanding, falls in temperature by the amount of heat converted into the work of raising the gun; the pressure consequently falls.

To meet these two sources of loss, amounting to the heat corresponding to the work of the gun falling twice the height to which it rises and falls, the energy of the discharge has to be drawn upon; it compresses the air far above its isothermal line, although that line is so fixed as to yield sufficient heat for conversion into the work of raising the gun. In addition, the energy of discharge has to provide the means of overcoming the friction of the machinery which resists the falling of the gun, and again resists its rising, so that, taking all the sources of loss enumerated together, the energy of recoil of even our most powerful guns is not adequate to do more than allow them to fall some 8 or 9 feet, an amount, however, sufficient for the most ample protection.

It will be readily seen that the construction of a disappearing carriage offers a number of problems of great scientific as well as practical interest. We have only dwelt upon some of the most prominent points. There remain the strains on the elevating gear, which is arranged so as to bring the gun into the same loading position, irrespective of the angle at which it is fired, and has, therefore, to communicate a sudden rotatory motion to the gun; the resistance of the levers and elevating-bars to the cross strains caused by their own inertia when brought into sudden motion sideways; the resistance offered to the water in its passage at variable velocities from the cylinder to the air-vessel, the accelerating force to be provided to raise the gun in a given time, and many minor problems which tax to the full the application of mathematics to the design of machinery.

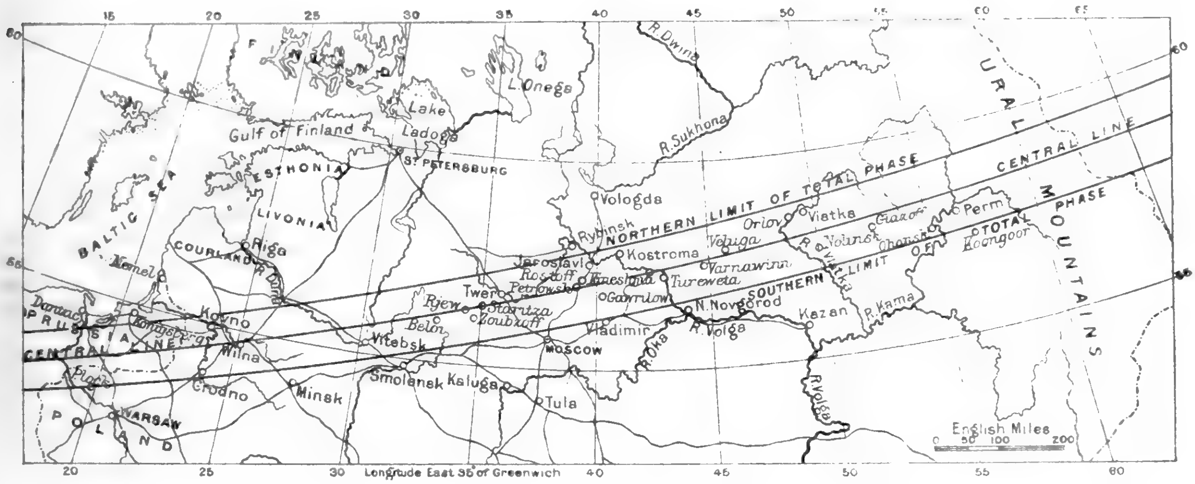
THE TOTAL SOLAR ECLIPSE OF AUGUST 19, 1887.

THE total eclipse of the sun which will occur on August 19 next, though only of average duration, will offer exceptional opportunities for observation from the circumstance that the track of the moon's shadow will be almost entirely a continental one, in striking contrast to the eclipses of the last four years, in all of which the shadow has followed a course which has been principally over the great oceans. The eclipse is technically a partial one for the principal part of Great Britain, but as it will be nearly over before sunrise, it will practically not be visible here. The middle phase will have been reached at sunrise, for places a little to the west of Berlin: and this city lying within the path of the shadow, it is just possible that it may be favoured with a sight of the phenomena of totality, though with a sun close to the horizon; for the sun will be largely obscured as it rises, and will not be quite 3° high at the end of the total phase. From Prussia the shadow track passes into Russia, and the central line does not leave the borders of the Russian Empire until it reaches East longitude 112°. It then crosses Manchuria and the Sea of Japan, and cuts the principal island of the Japanese group a little to the north of the capital. The final portion of its course lies over the North Pacific Ocean, and except for the little island of Rico de Oro, it does not touch land again. But the path of totality not only lies mainly over land, a large number of important towns are either actually included within, or lie very close to its limits. Königsberg lies just outside. Kovno, Wilna, and Vitebsk, are well within the shadow; Wilna being nearly on the central line. At these towns, however, the sun will still be too low for them to afford desirable stations for observations, and probably the neighbourhood of Moscow will be the nearest district which will be occupied by astronomers. At Moscow itself, the eclipse will not be quite total, since that city lies just outside the southern edge of the shadow-track, but three lines of railway radiating from Moscow will afford easy access to places actually on the central line. The most westerly of these three railways is that which unites St. Petersburg with the older capital, and which passes through Twer. Twer is nearly on the central line, but a little to the north of it. The sun will have an elevation of about 16° in this neighbourhood, and the maximum duration of totality is not quite two minutes and a half. At Twer itself it will be only 124 seconds. Three parties, two German, and one French, will take up positions within the Government of which Twer is the capital. The second line runs from Moscow to Vologda, passing through Jaroslavl, which lies within but near the edge of the shadow. Petrowsk on this railway is very near the central line, and here the sun will be 2° higher than near Twer, and the duration 152 seconds. The third line runs to Kineshma, which is itself very near the central line. Here the sun will be about 20° high, and the total eclipse on the central line will last 156 seconds. It will not, however, be difficult to proceed to yet more favourable positions further east. From Moscow there is a line through Nijni Novgorod to Kazan, and a service of river steamers runs thence up the River Kama to Perm. Perm lies to the south of the central line, but the totality lasts there 173 seconds, whilst the sun is 28° high at mid eclipse. If the weather should be favourable, Perm would be therefore a very suitable station for those astronomers who can spare the time to journey so far; for others the neighbourhoods of Petrowsk and Kineshma will afford readily accessible sites. Prof. Bredichin, Director of the Moscow Observatory, has his own private observatory only two kilometres from Kineshma, and very close to the central line; and he has generously offered the hospitality of his house to the Royal Astronomical Society for two English astronomers, an offer which has been gratefully accepted by the Society, on behalf of Dr. Copeland and the Rev. S. J. Perry. Prof. C. A. Young also will have his station here, and a strong party of Italian and English astronomers, consisting of Profs. Tacchini and Riccò, and Messrs. Common and Turner, will be located at no great distance away, in the neighbouring Government of Vladimir.

The eclipse being visible in Europe and from places so readily accessible from England, no Government Expedition will be sent out to observe it. It is not probable, therefore, that any English astronomers will go so far east as Siberia. It may be hoped that Russian astronomers will make good this defect, especially as four of the principal towns of Siberia lie on the shadow-track—Tobolsk, Tomsk, Krasnoiarsk, and Irkutsk; the first and third being close to the central line, and the sun being eclipsed when nearly on the meridian at Irkutsk. A series of Siberian stations is the more to be desired, since, as Prof. D. P. Todd has pointed out in the *American Journal* for March, this eclipse offers an exceptionally favourable opportunity for a concerted scheme of observation. The path of totality coincides in a most remarkable manner with the lines of the Russian overland telegraph, so that it will be perfectly possible to select a series of stations in telegraphic communication with each other, and extending over a line of 100° of longitude, with an extreme difference in the absolute time of totality of more than an hour and a half. It appears, Prof. Todd learns from a letter from Dr. S. von Glasenapp, that the Russian telegraph service may be expected to give the use of its lines at the time for astronomical purposes. It is certainly to be hoped that so unique an opportunity may not be lost; for it might well happen that some discovery, either in solar research, or of a comet or intra-Mercurial planet, might receive in this manner the most satisfactory confirmation and development.

The eclipse may also be well observed in Japan. On the west coast, Niigata, one of the Treaty ports, lies well within the shadow on the north, and Takata, a large manufacturing town, on the south, the central line passing through the large fishing-village of Idzumosaki, on the high road between the two. The Island of Sado, opposite to Niigata, which is free to foreigners, is wholly within the shadow, the central line crossing Sawa Umi Bay. The totality here lasts 198 seconds, with a sun 37° high. On the east coast the important town of Mito lies almost precisely on the central line. The duration here will be 192 seconds, and the sun 35° high. Japan, indeed, offers advantages for observing stations superior to those of Perm, as the sun will be considerably higher, and the duration 20 to 25 seconds longer.

The following formulæ, computed by Woolhouse's method (*Nautical Almanac*, 1836), from the elements of the eclipse given in the *British Nautical Almanac*, will supply the means for the computation of the beginning



and ending of the total phase for any place not far from Perm, lat. 58° 8′ N., and long. 55° 12′ E.:—

$$\text{Cos } w = 52{\cdot}926 - [1{\cdot}89540] \sin l + [1{\cdot}42842] \cos l \cos (L - 68° 25'{\cdot}1);$$

$$t = 17\text{h. } 40\text{m. } 13{\cdot}0\text{s.} \mp [1{\cdot}94168] \sin w - [3{\cdot}20536] \sin l - [3{\cdot}85031] \cos l \cos (L - 19° 47'{\cdot}9).$$

And for determination of the latitudes of the central line, and of the north and south limits of totality in the longitude of Perm:—

$$n \cos (N + l) = \begin{cases} - [1{\cdot}73180] \text{ for N. limit;} \\ - [1{\cdot}72367] \text{ for central eclipse;} \\ - [1{\cdot}71538] \text{ for S. limit.} \end{cases}$$

$$n \cos N = [1{\cdot}42842] \cos (L - 68° 25'{\cdot}1);$$

$$n \sin N = [1{\cdot}89540].$$

As in similar formulæ given in NATURE for previous eclipses l is the *geocentric* latitude, L the longitude from Greenwich counted positive towards the east, and t results in Greenwich mean solar time. Quantities within square brackets are logarithms, not simple numbers.

Similarly for places near Idzumosaki, lat. 37° 38′ N., and long. 138° 49′ E., we have:—

$$\text{Cos } w = + 53{\cdot}9763 - [1{\cdot}84932] \sin l + [1{\cdot}53239] \cos l \cos (L - 24° 51'{\cdot}1);$$

$$t = 17\text{h. } 32\text{m. } 24{\cdot}9\text{s.} \mp [1{\cdot}99243] \sin w - [3{\cdot}45091] \sin l - [3{\cdot}85537] \cos l \cos (L + 10° 4'{\cdot}7).$$

And for central line and limits:—

$$n \cos (N + l) - \begin{cases} - [1{\cdot}74018] \text{ for N. limit;} \\ - [1{\cdot}73220] \text{ for central eclipse;} \\ - [1{\cdot}72408] \text{ for S. limit.} \end{cases}$$

THE STEERING OF H.M.S. "AJAX."

WHEN H.M.S. *Ajax* was first sent to sea, her steering qualities were found to be very defective, especially at high speeds, the most objectionable and perplexing characteristic of her behaviour being a tendency to require a large angle of helm to keep her on a straight course. This helm tendency was sometimes on one side and sometimes on the other, generally remaining the one or the other for some time unchanged, but occasionally changing sides without warning or apparent cause. On such occasions, at full speed, the ship had been found to fly off her course at a right angle before she could be mastered by reversing the helm.

In a lecture on this subject, lately delivered before the Royal United Service Institution, Mr. R. E. Froude summarised as follows the causes to which such behaviour might be colourably attributed in ships of the type of the *Ajax*, namely, flat-bottomed and full-ended, particularly in the run: (1) want of "directive character" (as he phrases it) of hull, from the flatness of bottom and fullness of ends; (2) weakness of action of rudder, from its position in the dead-water; (3) an active turning force, consisting in a one-sided pressure on the stern arising out of a one-sided system of flow in the water closing in behind the full run. It was principally to the last-mentioned cause that Mr. Froude, when called upon to investigate the case of the *Ajax*, was inclined to attribute the behaviour of the ship, such a phenomenon having been some years since incidentally observed in the course of experiments made in the experiment tank at Torquay on the resistance of a model having a full run. In this case a lateral force was found to be developed upon the stern of the model, accompanied by a trailing away of the wake to one side, and a transverse flow of the water across behind the stern, in the opposite direction to that in which the lateral force was developed. This one-sidedness of flow, and consequent force (like the helm tendency of the *Ajax*), was sometimes in one direction, and sometimes in the other, and occasionally reversed its direction during any experiment; but was generally more or less persistent in direction when initiated, although the direction in which it was initiated was apparently a matter of accident. It was Mr. Froude's belief, founded on these and many other experiments of various kinds, that this species of, so to speak, spasmodic one-sidedness of flow, and consequent one-sided force, attends the motion of all, even perfectly symmetrical, bodies through water, whenever their leaving lines are blunt enough to cause a large eddy behind them.

By way of a method of experiment suitable to test the effect of remedies designed to mitigate or remove either of the three presumable causes of the behaviour of the ship, which have been enumerated above, Mr. Froude towed a model of the *Ajax* in the experiment tank at Torquay, the model being attached to the towing carriage in such a way that, while the model was free to sheer out of the straight course, any such attempted sheering motion actuated a working rudder fitted to the model, in the proper direction for frustrating the attempt. By this contrivance the model was made to steer quite straight, and the criterion of the badness of the steering qualities of the model in the several conditions of trial subjected to experiment, was the amount and the degree of unsteadiness of the helm angle administered, this helm angle being continuously recorded throughout each experiment by an automatic apparatus. Thus tested, the model was found to exhibit conspicuously what has been referred to as the predominant characteristic of the behaviour of the ship, viz. the large helm angle, sometimes persistently on one side, sometimes on the other, and occasionally changing from one side to the other.

The principal remedies tentatively applied to the model with a view to identifying the main source of the evil, and indicating the direction in which improvement was to be sought, were these: (1) a deep keel, to supply "the directive character" in which the hull itself was presumably lacking; (2) placing the rudder altogether below the keel, so as to be quite clear of the dead-water; (3) an extensive dead-wood (or fixed rudder) behind the stern-post, (the working rudder being still below the keel), to frustrate the one-sided flow behind the stern, and do away with the consequent turning force. Of these three kinds of remedy the last-named proved much the most effective, proved indeed an almost perfect cure, thereby confirming the surmise that the one-sided flow at the stern was the chief source of the evil. A minor modification of this dead-wood, with the rudder in its proper place, such as could be practically applied in the ship, likewise proved very tolerably effective, the average helm angle required being reduced to one-third of its amount.

On the strength of the results of these experiments, the Admiralty added a structure of this kind to the stern of the ship, with a result which, while it was a remarkable corroboration of the model experiments, was also on the whole a decided success from a practical point of view, the reduction effected in angle of helm being quite sufficient to qualify the ship to steam at full speed in a squadron and keep station satisfactorily.

EDWARD T. HARDMAN.

By the unexpected death of this geologist, on the 30th ult., Irish Science has been deprived of one of her most promising followers. Mr. Hardman was born in Drogheda in 1845, and distinguished himself by the position he took at the Grammar School there, gaining a Government Exhibition and an entrance to the Royal College of Science in Dublin. He soon displayed his strong natural bent towards scientific pursuits, and when he quitted the College he had gained its diploma of Associate and taken a prominent place among its foremost students, more particularly in the departments of chemistry and geology. In 1870 he was appointed to the Geological Survey, and threw himself with characteristic ardour into the prosecution of field-work, while his knowledge of chemistry and mineralogy led to his being employed in special services where this knowledge was made available in the work of the Survey. His reports on the Tyrone and Kilkenny coal-fields are good examples of the extent of his knowledge and of his powers of literary expression. He also made his mark by the publication of papers outside the limits of official work. His interesting and suggestive memoir on the origin of Lough Neagh and his papers on anthracite and chert are well known.

In 1883 the Government of Western Australia applied to the Colonial Office for the services of a trained geologist to examine and report on the mineral resources and geological structure of the colony. Mr. Hardman was selected for the post, and obtained leave of absence from the Home Government to enable him to undertake the duties. He was absent upwards of two years, during which time he effected a preliminary survey of a wide tract of unexplored country, and made known its geological structure. In particular, he indicated the presence of gold, and pointed out the areas where gold-fields might be looked for. After enduring great hardships in the bush, he returned to this country, and resumed his duties in the Geological Survey. But the exposure in the Australian climate seems to have told upon his health. He had not been quite well during the spring, and at last he rapidly fell a victim to an attack of typhoid fever. We understand that arrangements had been nearly completed for recalling him permanently to take charge of the mineral surveys of Western Australia, when his sudden death occurred. He has left a widow and two children with no adequate provision, and his friends have already begun to take steps for collecting subscriptions for their behoof. Prof. V. Ball, of the Science and Art Museum, Dublin, and Dr. Henry Woodward, of the Natural History Department of the British Museum, Cromwell Road, S.W., have kindly undertaken to receive subscriptions.

NOTES.

THE Natural History branch of the British Museum in Cromwell Road has just received a most important donation from Lord Walsingham, consisting of a collection of Lepidoptera with their larvæ, mainly British butterflies (*Rhopalocera*) and certain families of moths (*Heterocera*), including *Sphingidæ*, *Bombyces*, *Pseudobombyces*, *Noctuæ*, *Geometridæ*, and *Pyralidæ*. There is also a fine series of Indian species, collected and preserved at Dharmsala, in the Punjab, by the Rev. John H. Hocking, and specimens of exotic silk-producing Bombyces in various stages of their development, obtained mostly from M. Wailly. With very few exceptions, the British larvæ, which retain a most life-like appearance, and are placed upon models of the plants upon which they feed, have been prepared and mounted by Lord Walsingham himself; the process adopted having been inflation of the empty skin of the caterpillar by means of a glass tube and india-rubber spray-blower over a spirit-lamp guarded by wire gauze. This has been found a simpler and quicker process, and one admitting of more satisfactory manipulation, than the alternative system of baking by means of heated metal plates or ovens. The specimens have mostly retained their natural colour, but in the case of the bright green species it has been found necessary to introduce a little artificial dry pigment. The whole collection consists of 2540 specimens of larvæ, belonging to 776 species, together with a series of the perfect insects of each species. As continued exposure to light is, unfortunately, most detrimental to the colours of insects, this collection cannot be exhibited permanently, but for the advantage of those who would like to see it without any restriction, it will be placed in the entrance hall of the Museum for a period of six weeks, including the Whitsuntide holidays and the Jubilee week in June.

THE Ladies' *Soirée* at the Royal Society will be held on June 8.

THE following men of science have been elected Foreign Members of the Linnean Society :—(Botanists) Dr. George A. Schweinfurth, Professor of Botany, Cairo, Egypt, whose travels and botanical researches in Central Africa are widely known; Count H. Solms-Laubach, Professor of Botany, University of Göttingen, whose observations on the Corallines, Gulf of Naples, and investigations in plant anatomy, especially that of flowering parasites, &c., are acknowledged biological contributions of merit; M. le Dr. Melchior Treub, Director of the Jardin Botanique, Buitenzorg, Java, whose studies among the Lycopods, Cycads, Lichens, &c., and whose labour in editing the "Annales du Jardin de Buitenzorg" are highly appreciated: (Zoologists) Dr. Franz Steindachner, Conservator of Herpetology and Ichthyology, Royal Museum, Vienna, distinguished for his very numerous and important memoirs on fish and reptiles generally; and Dr. August Weismann, Professor of Zoology, University of Freiburg, Baden, noted for his studies on the theory of descent, and embryological researches on insects and hydroids. Mr. William H. Beeby and Mr. Adolphus H. Kent, of London, and Mr. J. Medley Wood, of Durham, Natal, all three worthy workers in various departments of botany, have been elected Associates of the Society.

A LINNEAN herbarium has just been presented to the Upsala University by Prof. H. Sötherstrand, by whom it was inherited. It has been found by comparison of names to be a duplicate of that possessed by the Linnean Society of London.

THE Russian traveller, General Prjevalsky, is shortly to be presented with a gold medal by the Imperial Scientific Society of St. Petersburg, which has been specially struck, by order of the Emperor, in his honour. The medal bears on the obverse the initials of the recipient, and on the reverse the inscription "To the first student of the Natural History of Central Asia."

THE Paris Society of Civil Engineers offers a prize of 3000 francs for the best *éloge* of Henry Giffard, the well-known aëronaut and inventor of the injector. This competition is open to foreigners, but the papers must be written in French.

ON Monday evening the session of the Institution of Mechanical Engineers was opened at the Institution of Civil Engineers, Great George Street, Westminster. The President, Mr. E. H. Carbutt, delivered the inaugural address, taking as his subject, "Fifty Years' Progress in Gun-making."

THE sixtieth meeting of the German Association of Naturalists will be held at Wiesbaden on September 18–24 next. A number of new scientific instruments and preparations will be shown. All inquiries are to be directed to Herr Dreyfus, 44 Frankfurterstrasse, Wiesbaden.

THE County of Middlesex Natural History and Science Society will hold their first annual *soirée* on Monday, the 23rd inst., at 11 Chandos Street, Cavendish Square. The chair will be taken at 8 p.m. by Lord Strafford, Lord-Lieutenant of Middlesex, and President of the Society. Objects of scientific interest will be exhibited.

THE total value of the fish landed upon the coasts of Scotland during the four months ended April 1887 was £343,337, being a decrease of £10,591 upon the corresponding period last year.

A COLLECTION of Indian cocoons is about to be sent by the Indian Government to Manchester, where it will be open for inspection. Infected cocoons are to be despatched to France for examination by M. Pasteur's pupils, who, it is hoped, will be able to suggest means for checking the disease which has nearly ruined the silk industry of India.

THE largest piece of amber ever discovered was recently dug up near the Nobis Gate, at Altona. It weighed 850 grammes.

THE ravages of the May-bug in Denmark have become so serious that a Bill is now under the consideration of the Danish Parliament proposing that the cost of the destruction of these insects shall be borne half by the State and half by local authorities.

THE Dutch Government intends to construct a railway in Sumatra, the cost of which will be nearly £1,400,000 (16,000,000 florins). The object is to facilitate the working of the coal-fields near the River Umbili. The coal deposit in these fields is reckoned to consist of about two hundred millions of tons.

AN interesting paper on "An Ideal Natural History Museum," read lately by Prof. W. A. Herdman before the Literary and Philosophical Society of Liverpool, has been issued as a pamphlet. Prof. Herdman calls attention to the strange fact that the Darwinian theory of evolution has had, as yet, little or no effect upon the structure and arrangement of museums of natural history. He urges that a phylogenetic arrangement would have the following advantages over the linear arrangement now employed in our museums :—(1) A phylogenetic arrangement would give a much more accurate representation of Nature. (2) While being more intelligible and instructive to the general public, it would be more in accord with the present state of biological knowledge, and could very readily be slightly altered from time to time so as to keep abreast with the progress of science. (3) It would be a perpetual illustrated lecture, of the best kind, demonstrating to everyone with ordinary intelligence the great doctrine of organic evolution.

ON April 14 about 9.15 p.m. a large meteor was observed at Throndhjem, in Northern Norway. It went in a direction from north to north-east, and during its passage the light was so brilliant that the smallest objects in the snow were visible. It burst, as it seemed, into thousands of fragments, but there was no sound or report. Before bursting, the meteor was green, but during that process it displayed colours of red, yellow, and green, chiefly the latter.

M. E. FERRIÈRE has published a book called "La Matière et l'Energie," summarising the latest results of physical investigation concerning matter and force.

THE second number of the "Jahrbuch der Naturwissenschaften" (a volume of nearly 600 pages) has just been issued. This useful periodical is edited by Dr. Max Wildermann, and published by Herr B. Herder, of Freiburg-im-Breisgau. The present volume contains a clear and popular account of the work done in each of the sciences in the year 1886.

We understand that the second part of the "Manual of Practical Botany," by Prof. Bower and Dr. Sydney Vines, will be published by Messrs. Macmillan and Co. in the course of a few weeks. It will include the Bryophyta and the Thallophyta; among the chief types used being *Polytrichum commune*, *Marchantia polymorpha*, *Polysiphonia fastigiata*, *Fucus serratus*, *Coleochæte scutata*, *Volvox globator*, *Agaricus campestris*, *Claviceps purpurea*, *Eurotium Aspergillus*, *Pythium de Baryanum*, and *Mucor mucedo*. Besides these a good many subsidiary types are used to illustrate special points.

"A Classification of Animals," drawn up by Mr. E. T. Newton for Mr. H. B. Woodward's "Geology of England and Wales," has now been issued separately. It is founded on the classifications proposed by Prof. Huxley, with such modifications as are, in the author's opinion, rendered necessary by recent discoveries.

The Annual Report of the Royal Alfred Observatory, Mauritius, for the year 1885, has been issued. The mean temperature for the year at the Observatory was 73°·6. The highest reading was 86°·7 in February, and the lowest 57°·4 in July. Rain fell on 200 days, and amounted to 44·61 inches; the fall was below the average in the usually wet months of January to April, but above the average in the usually dry months of May to October. The island has not been visited by a hurricane since March 1879, although several cyclones have passed not far from it. The Report contains observations made at various stations in the island and at the Seychelles, and notices of storms in the Indian Ocean, collected from ships' logs. Photographs of the sun were also taken daily, when the weather permitted. There were 354 days on which it is certain that spots were on the sun's disk, and eight days on which it is certain that there were none. The number of spots in May was unusually great.

In a recent book, "L'Enseignement actuel de l'Hygiène dans les Facultés de Médecine en Europe," Prof. Lœwenthal, of Lausanne, shows that the time allowed per year for the teaching of hygiene varies from 20 minutes per week in England to 9 hours per week in Spain. The other countries range between these two extremes. The average is from 2½ to 3 hours per week for the whole year.

In the May number of the *American Journal of Science* will be found a paper by Mr. Carey Lee, of Philadelphia, in which are described a remarkable series of salts of silver, which the author is attempting to make use of in obtaining photographs of objects in their natural colours. It is first shown, by an exhaustive series of experiments, that when light acts upon ordinary silver chloride, AgCl, in presence of hydrochloric acid, the darkening is due to the formation of a small quantity of subchloride, Ag_2Cl, which enters into combination with the unaltered silver chloride to form a reddish compound of a nature similar to that of a "lake." This red chloride of silver is termed protochloride, and is found to be, unlike subchloride, unattacked by cold strong nitric acid. After a certain amount of this substance is formed, the action of light appears to cease—a phenomenon which has been frequently noted by other observers. Successful efforts were then made to prepare protochlorides, bromides, and iodides of this nature, and a full description of the very numerous methods and analyses is given in the memoir. The startling fact was discovered that all varieties of tints from one end of the spectrum to the other could be obtained under suitable conditions. Normally, the protochloride of silver is red, even one-half per cent. giving to ordinary silver chloride a strong coloration; but on exposure to diffused sunlight it quickly changes to purple. On addition of mercuric chloride it becomes gray, potassium bromide changes it to a permanent lilac, potassium iodide to a bluish tint, while a mixture of potassium chlorate and hydrochloric acid causes it to pass through pink and flesh-colour to pure white. Heat, on the contrary, causes it to retake its red coloration, and on exposure to various parts of the spectrum it affects lovely shades of the most varied hues. The important observation was made that, in presence of small quantities of lead or zinc chloride, white light (which darkens the pure protochloride) bleaches it, thus producing white in those portions of the image which ought to be white; and it was also found that the addition of a little sodium salicylate enhances the sensitiveness threefold. The experiments are being continued, and appear likely to lead to important results in chromo-photography.

The current number of the *Auk* (vol. iv. Part 2) contains some interesting papers, but none of great importance. The want of finality in the system of nomenclature now practised by the American ornithologists is as marked as ever. Thus, Dr. Stejneger, having previously settled the synonymy of the redpolls, by the confounding of all received nomenclature, and the introduction of nine new synonyms into the already over-burdened literature of six species, here furnishes a tenth hitherto unrecorded title for our British redpoll; and Mr. Brewster follows suit by adding another synonym to one of the American species of the same group. It might be well for ornithologists to consider whether the best plan would not be, as Mr. Seebohm advises, to simplify matters by accepting in every case the name that happens to have been for a long time most in vogue.

In an interesting paper on the Ailsa Craig Lighthouse, read lately before the Scottish Institution of Engineers and Shipbuilders, Mr. G. M. Hunter drew attention to the admirable system of illumination and signalling in the Firth of Clyde. There are Corsewall Point light, alternating white and red, at the entrance to Loch Ryan, distant from the Craig 17 miles; Sanda Isle light, off the Mull of Kintyre, distant 18 miles; Turnberry light, off the Ayrshire coast, distant 12 miles; Pladda light and fog-signals, off the southern end of Arran, distant 12 miles; and Holy Isle green and red light, distant 18 miles—all of which are revolving lights, with the exception of the fixed lights at Pladda and Holy Isle. These lights are all under ordinary circumstances visible from Ailsa Craig.

In the *Izvestia* of the Russian Geographical Society there are some interesting remarks, by A. N. Krasnoff, on the history of the valley of the Ili River in Russian Turkestan. The Russian traveller considers that during the post-Pliocene age the valley of the Ili was nearly all occupied by water, and that the vegetation on the shores of this basin was quite different from that which exists now. It resembled, he thinks, the present vegetation of Middle Russia. There were forests of deciduous trees, among which maples, elm-trees, and apple-trees prevailed, and black-earth steppes occupied wide areas. Relics of this vegetation survive only at the foot of the snow-clad mountains, where they find the necessary moisture. Several of the species have there undergone remarkable adaptations, which permit them to support the rigorous continental climate. Deprived of the moisture of the snow-clad peaks, the vegetation of the lower ridges has completely changed since the recent desiccation of those parts of Asia. These ridges are covered now with a purely Central-Asiatic flora. As to the shores of Lake Balkhash and Ala-kul, they are either stony deserts with small grassy plants, or shifting sands covered with the characteristic Aral-Caspian bushes nearly destitute of leaves. The Balkhash was formerly a much greater lake than it is now, and it is rapidly becoming smaller. The depth of the Ala-kul Gulf has so fa minished that the Kirghizes already ford the strait which con-ects it with the lake. None of the rivers given on our maps flowing into the Ala-kul Gulf and Lake Balkhash from the uth-west were found by M. Krasnoff. They have all dried up.

THE additions to the Zoological Society's Gardens during the ast week include an Alexandrine Parrakeet (*Palæornis alex-ndri*) from India, presented by Miss Ada Marshall; two hinese Geese (*Anser cygnoides*) from China, presented by Miss loare; four Midwife Toads (*Alytes obstetricans*), South uropean, purchased; a Blue-cheeked Parrakeet (*Platycercus vanogenys*) from North Australia; a Pied Crow Shrike (*Strepera raculina*) from Australia; a Sun Bittern (*Eurypyga helias*) om South America, received in exchange; a Blood-breasted igeon (*Phlogœnas cruentata*); two Dwarf Chameleons (*Cha-æleon pumilus*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

MICROMETRIC MEASURES OF JUPITER AND SATURN.—In he recently-published "Observations," made at the Hong Kong Observatory during 1886, Dr. Doberck gives some mea-ures of Jupiter and Saturn made with the 6-inch Lee equatorial ow at Hong Kong. The measures of Jupiter, extending from August 29, 1879, to April 7, 1886, include the position-angle of he polar axis, the apparent equatorial and polar diameters, the readth of the equatorial belts and of the red spot, and the ength of the latter when on the central meridian. Dr. Doberck concludes that the equatorial and polar diameters at the mean listance of Jupiter are 38″·207 and 35″·942 respectively, and hat the equatorial semi-diameter at the mean distance of the earth from the sun is 99″·39. The measures of Saturn extend rom January 3, 1879, to April 5, 1886, and include the position-ngle of the polar axis, the external diameter of the ring, the liameter of Cassini's division, the internal diameter of the ring, nd the equatorial and polar diameters of the planet. The deduced dimensions at the mean distance of Saturn are:—External diameter of ring 40″·28, diameter of Cassini's division 34″·42, internal diameter 26″·82, equatorial diameter of Saturn 17″·22, and polar diameter 16″·53. The equatorial semi-liameter at the mean distance of the earth from the sun is 82″·11.

PRESENT APPEARANCE OF SATURN'S RING.—M. Stuyvaert, Assistant-Astronomer at the Royal Observatory, Brussels, has ecently presented a couple of drawings of Saturn to the Royal Belgian Academy. These were made on February 8 and 15 in he present year, and show the Cassinian division as encroaching on the outer ring, A, in a remarkable series of shaded indenta-ions. Ring B is nearly broken up into a series of bright white pots by a number of dusky indentations on its inner border of a imilar shape, and the dusky ring, C, likewise shows two dark notches on the inner side of the following ansa. Struve's division between B and C was also seen, and appeared on February 8 to e formed by a succession of dark gray spots. These observations re largely supported by those of Dr. Terby and Mr. Elger, published in the *Observatory* for March and April. Mr. Elger observed three or four "large re-entering angles like the teeth of a saw" on the inner margin of the dusky ring. This was on he preceding ansa, and not the following, as in M. Stuyvaert's observations, but the rotation of the ring would account for the change. Mr. Elger also noticed on February 25 that the pre-eding ansa of the dusky ring was unequally black, certain parts of its surface appearing quite black. These black spots were also noticed and drawn by Dr. Terby, who likewise remarked he unusual distinctness and breadth of Struve's division. It would appear, therefore, from these and other recent observations hat the matter composing the ring system is at present much less ymmetrically and evenly distributed than usual. Irregularities n the inner borders of the various rings, such as the above observers describe, have indeed been observed before, Trouvelot, or example, having remarked notches in Ring A, and Jacob imilar indentations in the dusky ring, but they are not ordinarily een.

THE RED SPOT UPON JUPITER.—From some recent observa-ions of this object published by Mr. Stanley Williams in the May number of the *Observatory*, it appears that the ephemeris given by Mr. Marth in the *Monthly Notices* for November 1886, is about a quarter of an hour too late. The red spot may therefore be expected to be on the central meridian at about the following times:—

	h. m.		h. m.		h. m.
May 24	21 33	June 7	23 6	June 19	23 0
,, 26	23 11	,, 10	20 35	,, 22	20 30
,, 29	0 49	,, 12	22 14	,, 24	22 8
,, 31	22 19	,, 14	23 53	,, 26	23 46
June 2	23 57	,, 15	19 43	,, 27	19 38
,, 5	21 27	,, 17	21 22	,, 29	21 16

The above times are expressed in Greenwich civil time, and are reckoned from midnight to midnight.

DISCOVERY OF A NEW COMET.—A new comet was discovered on May 12, by Mr. E. E. Barnard, Nashville, Tennessee, U.S.A. Place, May 12, 16h. 57m., R.A. 15h. 10m. 58s., Decl. 31° 25′ S. The comet was only faint.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 MAY 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 May 22.

Sun rises, 4h. 1m.; souths, 11h. 56m. 24·6s.; sets, 19h. 52m.; decl. on meridian, 20° 23′ N.: Sidereal Time at Sunset, 11h. 52m.

Moon (New on May 22) rises, 4h. 18m.; souths, 11h. 38m.; sets, 19h. 9m.; decl. on meridian, 14° 17′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	3 47	11 31	19 15	18 38 N.
Venus	6 17	14 47	23 17	25 14 N.
Mars	3 43	11 29	19 15	18 56 N.
Jupiter	16 26	21 43	3 0*	9 14 S.
Saturn	7 16	15 23	23 30	22 8 N.

* Indicates that the setting is that of the following morning.

May	h.	
26	8	Venus in conjunction with and 5° 18′ north of the Moon.
26	17	Saturn in conjunction with and 2° 45′ north of the Moon.
27	14	Mercury in superior conjunction with the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	May 24,	2 18	*m*
U Canis Minoris	7 35·2	8 39 N.	,, 22,		*m*
δ Libræ	14 54·9	8 4 S.	,, 28,	2 44	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 25,	1 48	*m*
			and at intervals of	20 8	
W Sagittarii	17 57·8	29 35 S.	May 28,	21 0	*M*
η Aquilæ	19 46·7	0 43 N.	,, 27,	3 0	*m*
R Sagittæ	20 8·9	16 23 N.	,, 28,		*m*
R Vulpeculæ	20 59·4	23 22 N.	,, 22,		*m*
δ Cephei	22 25·0	57 50 N.	,, 26,	3 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.
Near *o* Draconis	280°	54° N.
γ Cygni	301	37 N.
From Lacerta	329	48 N.

GEOGRAPHICAL NOTES.

THE new Ergänzungsheft (No. 86) of *Petermann's Mitteilungen* contains a monograph of great importance in scientific geography by Dr. Rudolf Credner, Professor of Geography at Greifswald, on "Die Reliktenseen," which he defines broadly so as to include all lakes of marine origin, whether they do or do not now contain remains of marine fauna. The author considers such lakes of so great importance in connexion with the evolution of the earth, that he thinks it the duty of physical geography to critically examine all data concerning lakes which may have a claim to be regarded as of marine origin, and decide whether such claim is justifiable. He discusses the evidences on which lakes may be regarded as of marine origin—historical, morphological, biological, and with regard to existing names. He then devotes considerable space to the discussion of the claims of lakes in all parts of the world to be regarded as of such origin; to the relations between salt- and fresh-water fauna; and to a critical examination of the faunistic argument for the marine origin of existing inland lakes. He concludes that none of the arguments derived from the considerations referred to have a convincing importance in deciding as to the marine origin of lakes. Dr. Credner is of opinion that the question can only be satisfactorily solved on the geological evidence furnished by the various lake regions; and this argument he means to develop in a second part of this very valuable monograph.

DETAILS are to hand of the recent exploration of the Mobangi tributary of the Congo, by Capt. van Gèle, which add something to the results obtained by Mr. Grenfell. Capt. van Gèle's journey was made at the end of 1886, at the time when the river is in flood, and when the current of the rapids is most powerful. At no part was the water less than 1·80 metre in depth, and the deepest did not exceed 11 metres. Not far from the embouchure of the Mobangi, on the left bank, 8′ 30″ S. lat., 17° 35′ E. long., there is a French station. Above this part the Mobangi measures 2500 metres in breadth, 11 metres in depth in the centre, with a current at the rate of 1 metre per second. At the 4th degree N., just below the rapids, there is a breadth of 1200 metres, a depth of 7·50 metres, and a current of 1·50 metre per second. Between these two points the breadth of the Mobangi constantly varies, never exceeding 4000 metres. Its waters are of a clear brown colour, and its general aspect much the same as that of the Congo, its channel studded with islands, and its banks wooded. The right bank is often marshy, while the left bank is frequently steep, and the neighbourhood hilly. The left bank is much more densely peopled than the right, which never has but a scanty population. On the left bank, especially above the 2nd degree of latitude, there is a rapid succession of villages, belonging to the Baati, the Monyembo, and the Montumbi. The people are well made and tall (mean height of men 1·80 metre), they are industrious, but at the same time inveterate cannibals. In all the course of the river which has been observed, Capt. van Gèle did not notice any affluent of importance; the only three worth mentioning are the Nghiri on the left bank, and the Ibenya and the Lobay on the right. The Nghiri winds through a very marshy country, which probably occupies the place of the conjectural lake of that name. About 4° N. lat., a mountain mass is met with, running in a north-east and south-west direction, through which the Mobangi must penetrate in making its way to the Congo, and here it is, as might be expected, that rapids are found. The river here is narrowed into a gorge, impassable at high water, but, Mr. Grenfell assures us, quite passable at low water for a suitable steamer.

THE Argentines have been very active recently in the exploration both of Patagonia and of their section (the eastern) of Tierra del Fuego. In a communication which appears in *Petermann's Mitteilungen*, Ramon Lista gives some details of a journey which he made through the centre of the large eastern island from Sebastian Bay to the Strait of Le Maire. He states that our notions of the surface and climate of this island have hitherto been entirely erroneous: it has been regarded as inhospitable, barren, and uninhabitable; its rocky mountains covered with everlasting snow. This may be so with the west part of the land, but M. Lista gives a different account of the region traversed by him. From Cape Espiritu Santo to Cape Peñas he found valleys of varying breadth, covered with luxuriant fodder plants, and abounding in rivers, some of which are navigable, and which come from a snow-covered region in the interior. South of this is found the region of Antarctic forests. Though not so rich in grass and water as the northern region, M. Lista states that it made a favourable impression on him. He saw a good deal of the native population, and collected considerable data as to their anthropology. Many other scientific observations were made by him on the geology, fauna, and flora of the country.

THE Carniola section of the German and Austrian Alpine Club has resolved to put up on the Old Posthouse at Wurzen, the favourite head-quarters of Sir Humphry Davy, a tablet to commemorate his services in making known the South-Eastern Alps of Austria, and in attracting visitors thither.

THE ROYAL SOCIETY CONVERSAZIONE.

LAST week we referred to the *conversazione* of the Royal Society, held on Wednesday, the 11th inst. It was the best which has been given for many years. A large number of remarkable objects were exhibited, and an account of some of the most important of them may be of interest to our readers.

Prof. A. W. Rücker exhibited lecture apparatus to illustrate the measurement of coefficients of expansion by means of Newton's rings. The rings are formed between a glass plate and the convex end of a glass cylinder. These are pressed together by a metal frame, the front and back of which are connected by tubes through which a current of water is passed. The rings are projected on a screen and expand or contract when the temperature of the water is altered. The apparatus was shown in operation.

Maps to illustrate the present state of the magnetic survey of the British Isles now in progress, with a set of instruments of the Kew pattern, which have been used in the survey, were exhibited by Profs. Thorpe and Rücker. (1) Large map showing the stations at which observations have been made, and the values of three magnetic elements, viz. the inclination, declination, and total force at all places for which the reduction of the observations has been completed. The epoch of the survey is to be January 1, 1886, but the values given are not as yet corrected for secular change, except in the case of stations in Scotland. (2) Three maps of Scotland showing the lines of equal dip and equal total force for 1837, 1858, and 1886, and the lines of equal declination for 1858 and 1886. Mr. C. V. Boys exhibited a radio-micrometer and spinning-pile, which is probably the most sensitive instrument for measuring radiant heat yet made. It consists of a movable circuit of copper, antimony, and bismuth hung by a quartz fibre in a strong magnetic field. One-hundred-millionth of a degree is not beyond the possible limit of such an instrument. Prof. D'Arsonval made an instrument essentially the same in principle last year. This radio-micrometer was devised by the exhibitor without knowledge of M. D'Arsonval's, from which, however, it differs in important details. The one exhibited is an experimental instrument only; but it is about one hundred times as sensitive as a thermopile. The spinning-pile is peculiar in that it will start itself and turn either way indifferently when a spark is held on one side, and will at once stop when the spark is held on the other. Mr. Boys also showed an apparatus for shooting threads of glass, emerald, quartz, &c. A thin rod of the material is fastened to the tail of an arrow and heated at the end by an oxy-hydrogen flame. The trigger of a cross-bow is immediately pulled, and the arrow shot, when a thread of extreme fineness is drawn out. These threads are far finer than spun glass, and many are finer than spider-lines. Threads of quartz are practically free from elastic fatigue, and are most suitable for the torsion threads of instruments of precision. Quartz can be drawn so fine that the thinnest parts are beyond the power of any possible microscope to define them. Experiments were made, showing the discharge by flame of electrically-spun threads.

Sir John Fowler and Mr. B. Baker exhibited a series of most marvellous photographs of the 1700-feet span cantilever bridge now in course of construction across the Firth of Forth. Some of these photographs will be exhibited to-morrow at the Royal Institution. Specimens of wire and other articles made from "platinoid," manufactured by Mr. F. W. Martino, were exhibited by the London Electric Wire Company. Platinoid is untarnishable under atmospheric influences, and is specially suited to be a substitute for platinum-silver, German silver, &c., for electrical purposes, as by experiments it has proved itself unchanged under variation of temperature (see Proceedings of the Royal Society, No. 237, 1885). Major H. S. Watkin exhibited a Watkin patent aneroid invented by himself, and manufactured by Mr. J. J. Hicks. It is well known that aneroids have been made of all sizes, from 3 feet to half an inch in diameter; the length of the divisions on the scale representing inches on the mercurial barometer have also been varied to suit different purposes; but inasmuch as there was only one circle of figures, either the number of inches, and therefore the extreme height at which the instrument was available, had to be restricted, or the dimensions of the scale contracted in order to obtain a longer range. Major Watkin's patent index gets over this difficulty, and an open scale can now be obtained, combined with great length of range. Thus, in the 4-inch patent aneroid 1 inch on the mercurial barometer can be made to represent from 4 to 10 inches, and yet be available for great heights.

Captain Wharton, Hydrographer of the Admiralty, exhibited un-signalling apparatus, designed by Mr. F. Galton, F.R.S., the use of naval surveyors. The optical arrangements are e same as those described by him in the Journal of the British ssociation, 1858, but the movements are new. Its advantage the facility it affords for accurate direction of the beam of ht; an image of the sun appearing over the object to which is desired to flash, when viewed through the telescope. apt. Wharton also showed a set of charts illustrating the drographical conditions of coral reefs and islands that stand deep water; a new chart of the south circumpolar regions, th tracks of explorers; and a chart showing sea-surface nperatures obtained off the north-west coast of Spain, June to eptember 1886. An improved pneumatic tide-gauge or level-licator was exhibited by Capt. de Wolski. This enables the gistering apparatus to be at any distance, horizontal or vertical, om the spot in the sea below low-water mark where the tide measured. Specimen charts (of which we have already given me account), exhibiting the conditions of weather over the lantic Ocean at the four seasons of the year, were shown by e Meteorological Council. (1) Daily synchronous charts of e North Atlantic in spring, February 27 to March 4, 1883. nese charts show that an anticyclone lies over Western urope, and the neighbouring part of the Atlantic, with much lm and fog prevailing at its centre; whilst the predominant nds in these islands are northerly and easterly, typical March eather. (2) Daily synchronous weather charts of the North tlantic in summer, August 1 to 6, 1882. These charts exhibit e prevailing area of high barometrical pressure on the eastern le of the Atlantic, which is related to a prevailing north esterly wind at the entrance of the Channel, changing to north f the coast of Portugal, and eventually merging in the north-st trade. (3) Daily synchronous weather charts of the orth Atlantic, in autumn, October 9 to 14, 1882. The imerous small areas of low barometrical pressure over the a on these charts, which appear at the time of year hen the region of highest pressure is about to be transferred om the ocean to the land, seem to indicate what takes place iring the change. Several small cyclonic systems are shown the southern part of the area of high pressure which prevails er the Atlantic in about 30° N. (4) Daily synchronous weather arts of the North Atlantic in winter, February 9 to 14, 1883. he part of the Atlantic north of 40° N. is affected by a large ea of low pressure, of which the centre lies somewhere to the uthward of Iceland. These conditions are usual in winter. aps of the English dialect districts, with key, by Mr. Alexander Ellis, to illustrate his "Existing Phonology of English ialects," not yet published, were exhibited by the author. Mr. rank Crisp exhibited early microscopes:—(1) Campani's icroscope. No field lens, and probably the earliest microscope tant. (2) Pope Benedict's microscope. Belonged to Cardinal ambertini, afterwards Pope Benedict XIV. Triple crown and oss keys inlaid in front of box. (3) Hooke microscope. This so belonged to the same Pope. (4) Oppelt's microscope. struments for measuring extensions and compressions in aterials subjected to stress were exhibited by Prof. W. C. nwin. (1) Apparatus to measure extensions to 1/10,000 of inch. Two clips embrace the bar, so that the movement of e middle points is the mean of the extensions on both sides. he clips are set level by sensitive levels, and the distance be-een them is measured by a micrometer screw. (2) Apparatus measure extensions. The bar is embraced by clips, so that a ean of the extensions on each side is taken. The extensions e measured by a roller and mirror. Measures to 1/100,000 an inch. (3) Similarly arranged apparatus for compressions. he strain is measured by a microscope micrometer. Measures 50,000 of an inch. Apparatus for the drawing of automatic ress-strain curves was shown by Prof. Kennedy. In this paratus the bar to be tested is extended "in series" with a uch stronger bar of steel. This bar is used as a spring, and its astic extensions, magnified by a light pointer, are taken as oportional to the stress in the test-bar, and recorded by the d of the pointer on a sheet of smoked glass which has a motion right angles to that of the points, and proportional to the elonga-on of the test bar. There is also a special arrangement of differen-al levers to eliminate any errors in this motion which might arise om the extension or "give" of other parts of the instrument.

Forty-six photographs of clouds in many parts of the orld were exhibited by the Hon. Ralph Abercromby, by whom ey were photographed. These were mostly taken during two voyages round the world for meteorological research. The pictures illustrate very clearly the identity of cloud-forms all over the world, for similar cumulus and cumulo-nimbus forms range in latitude from London to near Cape Horn—including one actually on the equator; and the stratus from Sweden to New Zealand; while the mists in the Himalayas are indistinguishable in general character from those of Great Britain. In addition to illustrations from the countries above mentioned, clouds are represented from Teneriffe, Brazil, the Falkland Islands, the Indian Ocean, and Borneo. Model of high-speed hydraulic or steam engine for driving electric light, and other purposes, was exhibited in motion by Mr. Arthur Rigg, the inventor. Reciprocation of pistons, and other moving parts, imposes an early limit to speed in engines of ordinary construction, so it has long been an unsolved problem how to produce a satisfactory engine without this evil, no rotary engines having ever yielded results encouraging their adoption. The revolving engine possesses pistons and cylinders, which are the best mechanical contrivance for remaining steam tight or water tight, and these have reciprocations relative as between each other, but only rotation in relation to the earth, while the cylinders and pistons revolve each on their own independent centres. The static balance and the dynamic balance are identical, and this engine therefore runs in equilibrium, without vibration, and in almost perfect silence. It is governed by varying the rate of expansion in the case of steam, or by varying the length of stroke in the case of water, and produces very economical results. It has none of that rhythmical variation in speed which occurs during each revolution of an ordinary engine. It is the only engine hitherto invented which can be driven at high speed by water pressures of considerable amount, and is found to give a perfectly steady incandescent light when making 250 revolutions per minute, driving a dynamo for 100 lamps, and worked by 700 lbs. per square inch water pressure. Prof. Forbes's thermo-galvanometer, made by Messrs. Nalder Bros. and Co., was exhibited by Prof. George Forbes. This consists of a ring, half of antimony, half of bismuth, one of the soldered junctions being filed thin and blackened to receive radiations. The conductivity of the ring is increased by the addition of a block of copper. A light Thomson magnet and mirror, suspended by a silk fibre, is placed inside the ring. The present form of instrument is rendered astatic by means of a second magnet. Prof. Forbes also showed specimens of electric welding by Prof. Elihu Thomson, of Boston, U.S.A. Some of Dr. J. Puluj's remarkable vacuum tubes, made by Müller, of Bonn, were exhibited by Mr. Warren De la Rue, and Dr. Hugo Müller. (1) Electrical radiometer with phosphorescent vanes. (2) Electrical radiometer with phosphorescent rotating disk. (3) Electrical radiometer with two phosphorescent rotating disks. (4) Electrical radiometer with rotating bell-glass. (5) Phosphorescent lamp. Specimens illustrating the effect of great earth-movements upon the pebbles contained in rock-masses were exhibited by Prof. J. W. Judd. (1) Series of impressed, faulted, crushed, and polished quartzite pebbles from the old red sandstone, near the great fault, Stonehaven, N.B. (2) Impressed limestone pebbles from the Swiss nagelflue (3) Faulted, crushed, and re-cemented flints from the chalk. (4) Pebbles from Bunter conglomerate, crushed and scratched by earth-movements. Specimens and microscopic sections of carboniferous chert, filled with spicules of siliceous sponges, were exhibited by Dr. G. J. Hinde. Beds of chert are largely developed in the carboniferous rocks of Yorkshire, North Wales, and Ireland, between the horizon of the carboniferous limestone and the millstone grit. In Flintshire they attain a maximum thickness of probably not less than 350 feet. These strata are of organic origin, and built up mainly of the detached skeletal spicules of siliceous sponges, which, for generation after generation, lived and died on the sea-bottom in these areas, and by the gradual accumulation of their microscopic spicules formed the rocks. Maps and sections of the Geological Survey of the United Kingdom were exhibited by Mr. Arch. Geikie, and a MS. geological map of the British Isles, for the geological map of Europe now in preparation by the International Geological Congress, was shown by Mr. William Topley, of the Geological Survey of England. Scale, 1 : 1,500,000 (1 inch to 23½ miles). The map will be in forty-nine sheets, in all about 12 feet by 10 feet. The cost of producing the map is contributed by the various Governments of Europe. England's share of the expense is £400, instalments of which are given, as required, by the Royal Society from its Government grant. For this sum a hundred copies of the complete map will be sent to the Royal Society. A drawing of a specimen showing the assumption of antenniform characters by the crustaceous ophthalmite, received from M. Alphonse Milne-Edwards, was exhibited by Prof. G. B. Howes. Mr. C. Baker showed Dr. Carl Zeiss's apochromatic objectives and eye-pieces, made of the new Abbe-Schott glass. The "Secohmmeter," a direct reading instrument for the absolute measurement of the coefficients of self and mutual induction, and for the absolute measurement of a capacity, was exhibited by Profs. Ayrton and Perry. On a future occasion we shall have something to say about this instrument. Mr. J. Norman Lockyer exhibited photographic comparison spectra of sun and metallic elements, taken at Kensington with Rowland grating. The metallic spectra were obtained in the usual way by putting metallic salts between the poles of an electric lamp. The lamp was placed at a distance of about 9 feet from the slit, and the rays of light diverging from it were rendered parallel by a lens of 9 inches focal length. An image of the sun was focused between the poles of the lamp by another lens of 10 inches focal length placed between the siderostat and the lamp. The light from the sun was thus sent through the slit under exactly the same conditions as that from the arc, so that both were brought to the same focus. The slit was covered with a piece of paper having four tongues, one of which was turned back for each exposure. The exposures varied from five to ten minutes. Mr. Lockyer also exhibited photographs of the spectra of compounds of carbon under various conditions, and a map showing the passage from flutings to lines in the spectrum of alcohol with increase of temperature, and the distribution of the various carbon flutings in the spectrum of the electric arc. The photographs, especially those of carbon dioxide, show how the spectrum of each compound depends upon the conditions of temperature and pressure to which it was subjected. A comparison of the spectra of different compounds will also show the general relations which exist between them; it will be seen that some of the flutings are special to certain compounds, while others are common to all. The accompanying map (approximately to a wave-length scale) represented the changes in the spectra of alcohol vapour produced by changes of temperature and pressure. The part of the map to the right of 4900 was mapped from eye-observations, and the remainder from the photographs. The lower half of the map shows the distribution of the carbon flutings in the spectrum of the electric arc, the spectrum of each portion of the arc being represented on the same horizon. A point of great interest is the appearance, in the flame which surrounds the negative pole, of three sets of flutings which shade off towards the red. The two most refrangible flutings shown in the alcohol spectra are apparently coincident with two of the five-membered ultra-violet group occurring in the spectra of the arc and cyanogen. Photographs of stellar spectra taken at Harvard College by Prof. Pickering (Henry Draper Memorial) were also shown by Mr. Lockyer. Spectra of α Lyræ, β Geminorum, α Cygni, α Tauri. These have already been referred to in NATURE. Twelve-inch Indian sun photographs taken at Dehra-Dun, India, March 4 and May 2, 1886, were exhibited by the Solar Physics Committee. The Rev. Dr. Pritchard showed (1) original negative of the Cluster in Perseus. Taken with the De la Rue reflector, 13 inches aperture; 120 inches focal length; exposure 30 minutes; diameter of plate-holder 6½ inches. This is one of a series of photographs taken in order to ascertain the greatest angular extent of the field, in which all the star impressions are free from deformation of circular contour. All the stars on this plate, even to the angular points, at a distance of 80′ from the centre, are sensibly free from ellipticity. Positive enlargements on glass of the above. (2) The Macromicrometer presented by Dr. W. de la Rue to the Oxford University Observatory, carrying one of the original negatives of 61 Cygni, as used for the determination of the parallax of the two components of that star. (3) Original negative showing the photographic genesis of star impressions formed during varying durations of exposure, and viewed under high magnifying power. Dr. Edgar M. Crookshank exhibited micro-organisms:—(1) Microscopical specimens, including living micro-organisms and permanent preparations. (2) Cultivations on nutrient jellies, potatoes, &c., of the following micro-organisms:—

Bacillus tuberculosis.
Micrococcus tetragonus.
Bacillus typhosus.
Koch's comma-bacillus.
Finkler's comma-bacillus.
Deneke's comma-bacillus.
Emmerich's bacterium.
Bacterium of rabbit septicæmia.
Bacillus of mouse septicæmia.
Bacillus of swine-erysipelas.
Bacterium of pneumonia (Friedländer).
Staphylococcus pyogenes albus.
Staphylococcus pyogenes aureus.
Staphylococcus pyogenes citreus.
Streptococcus of erysipelas.
Bacillus of anthrax.
Micrococcus prodigiosus.
Bacillus indicus.
Bacillus of blue milk.
Bacillus violaceus.
Bacillus pyocyaneus.
Red bacillus from water.
Red spirillum.
Black yeast.
Pink yeast.
Yellow sarcina.
Bacillus figurans.
Phosphorescent bacillus.

Water-cultures of the garden bean (*Vicia Faba*), the roots of which are infested with tubercular swellings, due to the parasitic action of a fungus, the extremely minute germs of which are common in the soil, were exhibited by Prof. H. Marshall Ward. Dr. E. Klein exhibited microscopic specimens and culture-tubes of the microbe of (1) foot-and-mouth disease; (2) scarlet fever; (3) several different forms of septicæmia; (4) swine plague.

THE METEOR OF MAY 8.

On Sunday evening, May 8, at 8h. 22m., hundreds of people witnessed the flight of the brilliant slow-moving fireball, about which three letters were printed in NATURE last week. At the time of its appearance daylight was still so strong that only Venus, Jupiter, Saturn, and a few first-magnitude stars were visible in the firmament. At stations in the eastern part of England the fireball fell in the western sky; at Bristol and the west it descended in the east; while at Stafford it is described as falling in the south.

Descriptions of the apparent path and appearance of the meteor have been received from Eastbourne, Staines, Stafford, Hartfield near Tunbridge Wells, London, Clevedon, Bristol, &c. It is referred to by most observers as a strikingly brilliant object, in comparison with which the planet Venus looked small and faint.

The following are quotations from some of the reports which have reached me from various places:—

The Rev. F. B. Allison, of Eastbourne, says:—"An exceptionally bright fireball was seen to fall to-night [May 8] at 8h. 30m. There was so much light in the sky that I could only detect α and β Aurigæ. The meteor was considerably larger and brighter than Venus, of a bluish tint, with train of sparks, slow motion: 6 seconds over the path indicated." Mr. Allison sends a diagram, in which the observed part of the course is shown extending under α and β Aurigæ, at an angle of about 42°, to a length of about 24°.

Mr. Francis Gare, of Staines, writes:—"The fireball was observed about 8h. 20m. to 8h. 25m., and was about half the size of the moon; its light was pale blue in colour, and was very bright, startlingly so. It left a train of red sparks about 6° long. The first part of its track was invisible to me as I was in a room with a S.W. window; this, too, would have prevented my hearing the detonation had there been any. The motion was slow." Mr. Gare sends a sketch, in which the fireball is represented as traversing 40° at an angle of 38°, and terminating 10° east of a line joining Venus and the horizon.

The Rev. E. Allen, of Castlechurch Vicarage, near Stafford, says:—"The time was within five minutes of 8h. 20m., May 8. It was so light that to see Spica as a reference-point, and whose place I knew exactly relatively to that of Jupiter, which was plainly visible, I had to fetch a binocular. The meteor was very large, and brilliantly white. Its light seemed to rise and fall in pulsations about two-fifths of a second in period, and its general power and effect was like what an extremely brilliant Roman candle ball would appear in somewhat deeper twilight at a distance of 50 or 60 yards from the spectator. Its motion was very slow, taking, I estimate, 5 seconds in passing along its total path of about 12° of arc. Estimating proportions of distance by eye, with the space between Jupiter and Spica as a guide, the path was something as follows: It was inclined about 25° to a perpendicular, the angle lying on the west, and fell from about R.A. 12h. 35m., Decl. 13° 30′ S., in a line nearly parallel to δ and ζ Corvi, and east of those stars."

At Hartfield, near Tunbridge Wells, the fireball was observed passing a little below Venus from right to left, and inclined 30° or 40° to the horizon. Duration, 3 or 4 seconds.

At Bristol, the meteor appears to have been pretty generally observed, and a large number of reports have come to hand. These, though differing in some essential particulars, sufficiently prove that the motion was from S.E. to E. by N. at an angle of 30°, the altitude at disappearance being about 20°. One observer describes it as being as large as a tennis-ball, and having a duration of 6 seconds. Another, who mentions the time as

20m., says that at disappearance it burst into a suppressed wer or halo of red; and a third relates that it travelled from to E. downwards, leaving two trains of sparks, and then lly bursting into fragments. It looked like an immense fire-k bomb, and many people, at the first impression, considered o near as to mistake it for a large rocket. One observer rs that as the meteor burst he found himself enveloped in a 'ave of heat" for several seconds!

Carefully comparing the descriptions of the path and direction, s found difficult to determine with precision over what point the earth's surface the fireball first became visible. Probably, vever, this occurred above the English Channel, about 25 es S.E. of the Isle of Wight, when the height of the body ıld be about 70 statute miles. From thence it slowly pursued irection to the N.N.W., and entered the English coast over sport, after skirting the eastern boundary of the Isle of ght. The meteor was descending to the earth at an angle 30°; at Gosport its height was 50 miles, and it afterwards sed over Winchester at an elevation of 38 miles, finally dis-pearing a few miles north of Swindon, when its height had ther decreased to 14 miles.

This path apparently satisfies the majority of the observations, there are, as usual, a few discordances. Thus, the Watford ervation (NATURE, May 12, p. 30) gives an altitude of 30° end-point in the W.S.W. (magnetic bearing). This seems too great; about half, or 15°, would be consistent with the er observations. At Staines, where the altitude must have n nearly the same as at Watford, it was given as 13°, and at eral places in London the altitudes are mentioned as 17° l less. Mr. Horner's observation at Montagu Street, W. ATURE, May 12, p. 30) proves conclusively that the altitudes e very low. He saw the meteor first near γ Geminorum . 23°), and it disappeared after moving slowly in the direction Jupiter. If we adopt the end-point from this description as 30° + 16°, we get the terminal altitude as only 12°, which is exact conformity with the adopted height of 14 miles at dis-pearance. The altitude of 17° from Highgate (NATURE, y 12, p. 30) is somewhat excessive, but it is well known that estimates of this character the figures are nearly always too at. Lieut.-Colonel Tupman states: "Most persons (as has n often before remarked) guess altitudes at double what they lly are, and 'the zenith' means anything higher than 45° or ' (see his paper on the great meteors of 1875, September 3, and 14, Appendix, *Astronomical Register*, vol. xiv. p. 1).

The observations at Staines, Hartfield, and Montagu Street, , are very fairly consistent as regards the direction of the teor, and, taken in combination with the especially valuable ice from Stafford and the average of the Bristol observations, radiant-point is found to have been situated in the S.S.E., tude 30°, which is about 10° N.W. of Spica Virginis, or at ° − 5°. No definite meteor-shower is known from this point May, though Heis gives a position at 191° + 7° for April 18 May 18, which can hardly be the same. The great fireball May 12, 1878, diverged from a radiant at 214° − 7°, and it scarcely be associated with that of May 8 last, as the two iants are 23° distant.

The recent fireball had a real path in the atmosphere of ut 110 miles. Its motion was very slow, but there are great cordances in the various estimates of duration. A large pro-tion of the observers only saw the latter part of the flight, it would seem that the whole duration was fully 6 seconds, bably more, in which case the velocity was certainly less n 18 miles per second. The fireball, if moving in a para-a, would have had a velocity of 13 miles per second.

As to the actual size of this brilliant visitor, nothing can be nitely concluded, because it is impossible to discriminate ween the glare and flaming effect of the nucleus and what ent of it represented the material diameter. The fireball was bably a very diminutive body, and much smaller than its spicuous aspect would lead us to suppose. Had it withstood uption and dispersion during another 1¾ second, it would e completed the remaining 28 miles of its path, and it must e fallen to the earth near Winchcombe, in Gloucestershire.

W. F. DENNING.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—We regret to hear that Prof. Prestwich has resigned Chair of Geology which he has held for the last thirteen years.

CAMBRIDGE.—Last week the grace authorizing the Vice-Chancellor to enter into negotiation with Downing College with a view to securing a site for the Geological Museum in the grounds of Downing College, opposite the New Museums, was carried by eighty to seventy-one votes. Prof. Hughes, in a previous discussion, had objected to the site on the New Museum grounds because it would soon become too crowded. The Downing College site would afford plenty of room. Whether the University and the College can agree on the question of the price to be paid remains to be seen.

The Botanic Garden Syndicate have issued a modified report, proposing a different site for their necessary new plant-houses, namely, palm-house, stove, warm fern-house, and orchid-house, and recommending that authority be given them to obtain a detailed plan and estimate for building these, together with a new propagating-pit, the cost not to exceed £3000. They also strongly recommend the erection, in connexion with these houses, of a small research laboratory.

The examiners for the Adams Prize—the Vice-Chancellor, Prof. Stokes, Prof. Darwin, and Lord Rayleigh—have given notice that the subject for the Adams Prize to be adjudged in 1889 is "The Criterion of the Stability and Instability of the Motion of a Viscous Fluid." It appears from experiment (see Phil. Trans. for 1883, p. 935) that the steady motion in a tube is stable or unstable according as the velocity is less or greater than a certain amount; and it is inferred from theory, confirmed by experiment, that in two geometrically similar systems the motion is stable or unstable according as $\mu/\rho c U$ is greater or less than a certain numerical quantity n; c, U being a length and a velocity which define the linear scale and the scale of velocity in the system, and ρ, μ the density and coefficient of viscosity of the fluid; but the quantity n has not hitherto been obtained even in a simple case except by experiment.

It is required either to determine generally the mathematical criterion of stability, or to find from theory the value of n in some simple case or cases. For instance, the case might be taken of steady motion in two dimensions between two fixed planes, or that of a simple shear between two planes, one at rest and one in motion.

Should the investigation not be found practicable for even a simple case of the motion of a viscous fluid, some substantial advance might be made in what has been done for a perfect fluid (see Proceedings of the Mathematical Society, vol. xi. p. 57), the title of the essay being modified accordingly.

The prize is open to all Cambridge graduates.

Each essay should be accompanied by a full and careful abstract, pointing out the parts which the author considers to be new, and indicating the parts which are to be regarded as of more importance than the rest.

The essays must be sent in to the Vice-Chancellor on or before December 16, 1888, privately. Each is to have some motto prefixed, and to be accompanied by a paper sealed up, with the same motto and the words *Adams Prize* on the outside, and the candidate's full name, with his College and degree, written within. The papers containing the names of those candidates who may not succeed will be destroyed unopened. Any candidate is at liberty to send in his essay either written (but not in his own hand) or printed or lithographed. The successful candidate receives about £170. He is required to print the essay at his own expense, and to present a copy to the University Library, to the Library of St. John's College, and to each of the four examiners.

SCIENTIFIC SERIALS.

The Quarterly Journal of Microscopical Science for March 1887, vol. xxvii. Part 4, contains:—On the termination of nerves in the liver, by A. B. Macallum (plate 36). These researches were made on the livers of man and Menobranchus (Necturus): the liver cells of the latter are from two to four times the diameter of those in man, and so were very favourable for these investigations; in man fibrils from the intercellular plexus of nerves give off excessively minute twigs, which terminate each in a delicate bead in the interior of the hepatic cells, near the nucleus; in Menobranchus the simple intracellular nerve-twigs always terminate in the neighbourhood of the nucleus, either singly or after branching, each terminal point being a delicate bead.—On the nuclei of the striated muscle-fibre in *Necturus* (*Menobranchus*) *lateralis*, by A. B. Macallum.—The development of the Cape species of Peripatus, Part 3: on the changes from Stage A to Stage F, by Adam Sedgwick, F.R.S. (plates

34–37). This elaborate memoir does not permit of being usefully summarised.—Morphological and biological observations on *Criodrilus lacuum*, by Dr. L. Örley.—Studies on earthworms, No. 3: *Criodrilus lacuum*, Hoffmeister, by W. B. Benham (plate 38). This little worm was first discovered by Fritz Müller in 1844, near Berlin, and was in the following year described by Hoffmeister; it was next found near Linz, and more recently in Italy and at Buda-Pesth by Dr. Örley, whose paper thereon has been translated from the manuscript by Mr. Benham. In the Danube this worm occurs, often in large numbers among the roots of *Sium latifolium*, the egg-cases looking like certain forms of Enteromorpha. The specimens dissected by Mr. Benham were sent to Prof. Lankester by Dr. Örley.—Notes on the chromatology of *Anthea cereus*, by Dr. C. A. MacMunn (plates 39 and 40). The pigments of Anthea are the pigments of certain marine Algæ, and are without doubt the pigments of the "yellow cells" which are now known to be unicellular Algæ.—On *Ctenodrilus parvulus*, nov. spec., by Dr. Robert Scharff (plate 41). This little Annelid was recently discovered by Mr. Bolton, of Birmingham, but its exact habitat is unknown.—On the relation of the Nemerteæ to the Vertebrata, by Prof. A. A. W. Hubrecht (plate 42); with permission from Prof. Hubrecht's Report on the *Challenger* Nemerteans.

American Journal of Mathematics, vol. ix. No. 3 (Baltimore, April 1887).—A memoir by Prof. Cayley on the transformation of elliptic functions, develops the algebraical theory established by Jacobi in the "Fundamenta Nova" (1829), and discusses other researches in this field by Jacobi, Brioschi, and the writer (see Brioschi's second appendix to his translation of Cayley's "Treatise on Elliptic Functions," and other papers cited in the present memoir).—Mr. G. P. Young contributes a long account of "Forms, necessary and sufficient, of the roots of pure uniserial Abelian equations"; and the number closes with some eighteen pages of tables under the heading "Symmetric Functions of the 14^{ic}," by W. P. Durfee,—these are arranged according to the second of the author's methods used in vol. v., where tables are given for the 12^{ic}. In vol. vi. it may be noted Capt. Macmahon does a similar work for the 13^{ic}.

In the numbers of the *Journal of Botany* for March and April, a species (or sub-species) of *Rubus* new to science is described by Mr. E. F. Linton, from Norfolk, under the name *R. lucens*, afterwards substituted by *R. lætus*. The remarkable *Equisetum littorale*, differing from all other species of the genus in the absence of elaters, is recorded as British (and figured) by Mr. Beeby, on the faith of specimens from Surrey. Mr. Spruce concludes his elaborate description of his new species of Hepaticæ, *Lejeunia Holtii*, from Killarney. The remaining articles are of merely local or technical interest.

The number of the *Nuovo Giornale Botanico Italiano* for April is almost entirely occupied by articles of interest to Italian botanists. In addition to those referring to the distribution of species, Sig. L. Savastano has two short papers. The first refers to the parasitism of *Agaricus melleus*. From experiments made on a number of different trees, the author concludes that this fungus does not attack healthy trees, but only those that are weakly or diseased. In the second paper, on Gummosis, he adduces facts to show that this morbid phenomenon is to a large extent dependent on temperature, being less frequent in the northern than the southern portion of the zone of cultivation of any given species.

SOCIETIES AND ACADEMIES.

London.

Royal Society, May 5.—"The Proteids of the Seeds of *Abrus precatorius* (Jequirity)." By Sidney Martin, M.D. Lond., Pathologist to the Victoria Park Hospital. Communicated by Prof. E. A. Schäfer, F.R.S.

Two proteids were found in the saline extract of the crushed seeds; one a *globulin*, identical with that occurring in papaw-juice, and belonging to the group of vegetable paraglobulins; the other an *albumose*, identical with what the author has described as α-phytalbumose in the papaw-juice. Attention was called to the differences between the class of vegetable paraglobulins and the vegetable myosins, which differ in the fact that the latter become readily changed into an albuminate when the sodium chloride holding them in solution is dialyzed away.

The investigation of the proteids is preliminary to that of their physiological action

"Note on the Microscopic Structure of Rock Specime from Three Peaks in the Caucasus." By Prof. T. G. Bonne D.Sc., LL.D., F.R.S.

These specimens are from three localities in the Caucasus, difficult of access, viz. the peaks of Tau Tetnuld, Guluku, a Elbruz. The first and second are peaks near together in t central part of the Caucasus; the specimens were collected 1886 by Mr. W. F. Donkin. (1) Tau Tetnuld: one specim from near the summit, representative of the rock forming all t upper part of the mountain. It is a mica-schist, which has be much crushed subsequent to its first crystallization. (2) Guluk a series of rocks representing the upper part of the mountain granitoid and gneissoid rocks and strong schists. These affo indications of more or less mechanical disturbance. In o the garnets have been flattened out into elongated ovals, a ultimately cracked. The specimens indicate a succession different rocks, possibly resulting from original stratificatic though true granite probably forms part of the mountain. From the western crater-peak of Elbruz, collected, in 1874, by Mr. H. Walker (from the highest rocks, more than 17,500 f above the sea). It is a hornblende-andesite, not containi quartz, and thus is different from those on the lower part of t mountain.

Linnean Society, May 5.—Mr. W. Carruthers, F.R.S., Pre dent, in the chair.—Mr. E. W. Forrest, and Mr. G. Per were elected Fellows; Mr. W. H. Beeby, Mr. A. D. Ke and Mr. J. M. Wood were elected Associates; Prof. G. Schweinfurth, Prof. H. Solms-Laubach, Dr. Franz Steindachn Dr. M. Treub, and Prof. A. Weismann were elected Forei Members of the Society.—The auditors chosen to examine t Treasurer's accounts were Mr. F. V. Dickins and Mr. G. Ma to represent the Fellows, and Mr. J. E. Harting and Mr. A. Michael for the Council.—Mr. J. W. Willis-Bund exhibit specimens of the rainbow trout (*Salmo irideus*) reared in t fish-culture establishment, Delaford Park. Though from eg of the same batch, the fish were very unequal in size. Fr the evidence of its being a migratory fish and other facts, M Bund doubts the value of its introduction into this country a stream trout.—Photos were shown and a letter read from M J. G. Otto Tepper regarding a gall formation on *Scævola sp escens* observed by him at Yorke's Peninsula, South Austral—On behalf of Mr. W. Brockbank, there was exhibited pho graphs of a series of forms of *Narcissus reflexus* of Brotero, fr Ancora, North Portugal, and grown in his garden at Didsbu *N. reflexus* is ranked as a species by Nyman; but the variati in the Portuguese plant is so great in the size and shape of t corona, that it is evident no definite line of demarcation can drawn between the Spanish *N. triandrus* and the Brittany *calathinus*. It would seem, therefore, that all the varietal for of the section Ganymedes constitute a single species.—Mr. Harris Stone exhibited the flowers of *Nicotiana glauca* fr Fuerteventura and Sanzarote, Canaries. The plant is a nat of Buenos Ayres, where it grows 10 feet high. It seems to ha been introduced into the Canary Islands about 1867–69, si which date it has run wild, and is now to be met with flouri ing as a weed on the path sides and in the villages, attaining height of 3 or 4 feet. The natives call it "mismo" (sam as spreading everywhere the same over the islands.—Pho graphs of the mud volcanoes of Trinidad, and of the Peak Rakata, volcano of Krakatão, after the eruption, were exhibit respectively by Mr. R. V. Sherring and for M. Verbeek. Mr. F. J. Hanbury called attention to specimens of hyb Primulas.—A paper was read, viz. experimental obser tions on certain heterœcious Uredines, by Mr. Chas. B. Plo right. Among these, *Puccinia phalaridis*, *P. arenariic Gymnosporangium clavariæforme*, *G. juniperimum*, and *sabinæ* more particularly engaged the author's attention; details of the cultures and analyses of the experiments be given.—There followed a paper on *Vaccinium intermedium* a new British plant, by Mr. N. E. Brown. It was discovered Prof. Bonney at Cannock Chase, August 1886, growing ple fully in certain spots; *V. myrtillus* and *V. Vitis-Idæa* be also abundant. Mr. Brown regards the plant in question a hybrid between the two latter species, and to have origina independently at Cannock Chase, and not been introduced fr the Continent.—A paper was read by Mr. R. A. Rolfe, bigeneric orchid hybrids, the subject being treated chiefly w reference to its bearing upon classification. After pointing that these hybrids, as in the case of those between species of t same genus, were more or less intermediate between the t ents, the practice was recommended of compounding a name m those of the two parent genera, so as to avoid all confusion h existing genera. With regard to orchid hybrids generally, following are the author's conclusions:—(1) Hybridization y take place not only between distinct species, but also ween distinct genera, or between plants so structurally dif- ent as to be usually regarded as such. (2) These hybrids are ierally of artificial origin, or accidentally produced, and can- be treated in the scheme of classification either as varieties, cies, or genera. (3) The possibility of hybridization taking ce between species hitherto considered as distinct does not cessarily prove them to be merely forms of the same species. The occurrence of a hybrid between two structurally different iera does not prove the necessity of uniting them in one; nor i such hybrids be arbitrarily referred to either of the parent iera. (5) Species and genera will always have to be dealt h in the scheme of classification according to their structural culiarities and differences, without reference to the possibility ybridization taking place between them.—A report was read, the Alcyonaria of the Mergui Archipelago, by Mr. Stuart O. lley, in which a considerable number of new forms were de- bed; the Burmese coast being rich in species bearing an lian facies.

Zoological Society, May 3.—Dr. E. Hamilton, Vice- sident, in the chair.—The Secretary read a report on the litions that had been made to the Society's menagerie during month of April 1887, and called attention to two young ar bears (*Ursus maritimus*) presented by Mr. Joseph Mon- h; and to two crested ducks (*Anas cristata*) from the Falk- d Islands, presented by Mr. F. E. Cobb.—Extracts were d from a letter addressed to the Secretary by Mr. Roland men, respecting the obtaining of a second example of *niarius atrocroceus* in South Africa.—Mr. J. Jenner Weir ibited and made remarks on a skull of a boar from New land.—A communication was read from Mr. G. A. Bou- ger, containing the description of a new snake of the genus *nprophis*, based on a specimen living in the Society's dens, which had been presented to the collection by the . G. H. R. Fisk.—A communication was read from Mr. I. Leech, containing an account of the diurnal Lepidoptera apan and Corea, based on a collection recently made by the hor during a recent entomological expedition to those coun- s. The total number of species in Mr. Leech's list was 155. apan, Mr. Leech had discovered one new species (*Papilio ado*), and in Corea four others.—Mr. R. Bowdler Sharpe, e an account of a second collection of birds formed by Mr. Wray in the mountains of Perak, Malay Peninsula. This ection contained samples of about fifty species, of which ten e described as new to science.—Mr. H. J. Elwes pointed the characters of some new species of diurnal Lepidoptera, cimens of which had been obtained by him during his recent t to Sikkim.—A communication was read from Mr. Lionel Nicéville, containing an account of some new or little-known lian butterflies.

Entomological Society, May 4.—Dr. D. Sharp, President, the chair.—The Rev. C. Ellis-Stevens, Mr. F. Merrifield, H. Rowland-Brown, and Mr. C. Matthews were elected lows.—Mr. Warren exhibited specimens of *Stigmonota palli- ntana*, *S. internana*, *Asthenia pygmæana*, and *A. abiegana bsequana*, Haw.).—Mr. Stainton remarked that it was for- ly thought that Haworth's *subsequana* was identical with the cies previously figured by Hübner as *pygmæana*; but now the two allied species were critically examined, it appeared the species described by Haworth as *subsequana* was not *mæana*, but another species known as the *abiegana* of Du- chel, dating only from 1842, so that Haworth's name— *equana*—had priority by thirty years.—Mr. F. Pascoe ex- ted a specimen of *Diaxines taylori*, taken out of the stem of orchid—*Saccolabium cœleste*—received from Moulmein.—Mr. Lachlan exhibited nearly 200 specimens of Neuroptera, col- ed by Mr. E. Meyrick in Australia and Tasmania, compris- about seventy species. There were between forty and fifty cies of Trichoptera, including forms from Western Australia, d to *Plectrotarsus*, and other species belonging to a group esented by *Hydropsyche edwardsii*. Among the Planipen- the most remarkable insect was a species of the singular is *Psychopsis*, from Mount Kosciusko, where it was common. Pseudo-Neuroptera there was a species of *Embiidæ* from stern Australia, and certain *Psocidæ* and *Perlidæ*. Mr. Meyrick made some remarks on the localities in which he had collected the species.—Mr. M. Jacoby exhibited a new species of *Xenarthra*, collected by Mr. G. Lewis in Ceylon.—Mr. C. O. Waterhouse exhibited a living example of an ichneumon—*Ophion macrurum*—bred from a larva of *Callosamia promethea*, a North American species. He also exhibited a number of wings of Lepidoptera denuded of the scales, in order to show the neura- tion, and explained the method he had adopted for removing them. The wings were first dipped in spirit and then placed in *eau de javelle* (potassium hyperchlorite). Mr. Waterhouse said he had sometimes substituted peroxide of hydrogen for *eau de javelle*, but the action was much less rapid, although the results were satisfactory. Mr. Poulton remarked that the discovery of some chemical for softening chitine had long been wanted to prepare specimens for the microscope.—Mr. Slater read a note, extracted from the *Medical Press*, on the subject of the poison used by certain African Bushmen in the preparation of their arrows. It was stated that the poison was prepared from a caterpillar which they called "N'gwa."—The Rev. W. W. Fowler read a note received from Mr. J. Gardner, in which it was stated that *Dytiscus marginalis* possessed the power of making a loud buzzing noise like that of a humble-bee. Dr. Sharp said he was familiar with the humming of *Dytiscus marginalis* previous to flight, and thought it might perhaps be connected with an inflation of the body for the purpose of diminishing the specific gravity of the insect; he had noticed also that it was occasionally accompanied by the discharge of fluid from the body.—Mr. Wm. White read a paper on the occurrence of anomalous spots on Lepidopterous larvæ.—Mr. Waterhouse read descriptions of new genera and species of *Buprestidæ*.

Geological Society, April 27.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—On the London Clay and Bagshot beds of Aldershot, by Lieut. H. G. Lyons, R.E. The author first described the section from Thorn Hill on the north to Redan Hill on the south, plotted from the 6-inch Ordnance Survey on a scale of 6 inches to 1 mile horizontal, and 12 inches to 1 mile vertical. This section comprises beds from the Woolwich and Reading series to the Upper Bagshot inclusive. The second section described runs from Gravel-Pit Hill on the north to Ash Green on the south. It was drawn to the same scale, and showed the beds from the Chalk to the Middle Bagshots inclusive. The third section was drawn, also on the same scale, through Alder- shot town, showing the beds from the Woolwich and Reading series to the Middle Bagshots inclusive. It was inferred from various calculations, as also from direct observations, that the thickness of the London Clay shows no diminution throughout the section, being nearly the same also at Ash and at Aldershot Place. In "Cæsar's Camp" the pebble-bed occurs at altitudes ranging from 500 to 550 feet. The author concluded that wherever we can fix the top or base of the London Clay we get a northerly dip of $2\frac{1}{4}°$ to $3°$, showing a fairly constant thickness of from 330 to 340 feet. The same thing occurs from Odiham on the west to Ash on the east, whilst at Brookwood the London Clay is thicker. He also assumed the existence of a passage from the London Clay up into the Bagshot beds in the deep wells or borings at Wellington College, at Brookwood, and at South Camp. Hence at these points there can have been no great erosion or unconformity. The overlying Bagshots lie con- formably on the London Clay and on each other. The Presi- dent congratulated the Society on the acquisition of a recruit whose carefully plotted sections did credit to his training as an officer of the Royal Engineers. The author's conclusions were discussed by Messrs. Irving, Whitaker, Monckton, Hudleston, and Herries.—Supplementary note on the Walton Common section, by Mr. W. H. Hudleston, F.R.S. The principal object of this paper was to point out the occurrence of certain beds of clay or loam in what are usually known as the "Lower Bagshot Sands" of West Surrey. The author maintained (1) that the more we study the Bagshot beds of this area the less likely are we to see a passage between the curiously diversified Lower Bagshots and the much more uniform and homogeneous London Clay; (2) that, until we realize the considerable though sporadic development of clays in the Lower Bagshots, we shall be in danger of referring beds to the Middle Bagshots which do not belong to them, and thereby give encouragement to a specu- lative stratigraphy which can only mislead. The reading of the paper was followed by a discussion, in which the President, Mr. Whitaker, Mr. Irving, and Mr. Herries took part.

Anthropological Institute, April 26.—Mr. Francis Galton, F.R.S., President, in the chair.—Mr. R. A. Cunningham exhibited some aboriginal Australians from North Queensland. The party consisted of a man, a woman, and a boy. They sang a corroborree song, and successfully showed the manner of throwing the boomerang.—Mr. C. H. Read read a paper on the ethnological bearings of the stone spinning-top of New Guinea, in which he gave a description of some spinning-tops recently presented to the British Museum.—Lieut. F. Elton, R.N., read some extracts from notes on natives of the Solomon Islands, obtained by him in reply to questions addressed to the solitary European resident on one of the islands.

PARIS.

Academy of Sciences, May 9.—M. Janssen in the chair.—A general method for determining the constant of aberration, by M. M. Lœwy. A demonstration is given of the remarkable geometrical property that the action exercised by aberration on the great arc connecting two stars is in proportion to the cosine of the angle formed between the median and the direction of the motion. It is then shown that, by effecting two conjugated observations, the constant of aberration may be determined independently of any physical corrections.—On Admiral Cloué's second memoir regarding the cyclone which swept over the Gulf of Aden last year, by M. H. Faye. Some exceptional features of this destructive cyclone are described and accounted for, and it is suggested that a regular signal service should be established in Socotra, or at some other favourable point, for the protection of shipping in these much-frequented waters.—Researches on the liberation of ammonia by vegetable soils, by MM. Berthelot and André. The experiments here described have reference mainly to the argillaceous soil on the higher plateaux in the neighbourhood of Paris. They tend to show that vegetable humus possesses the property of spontaneously liberating ammonia in proportion to the slow but certain decomposition of the starchy and ammoniacal compounds contained in it. The decomposition is effected under the influence of the purely chemical actions due to the water and the earthy carbonates, and doubtless also to the physiological actions attributable to the fermentations, microbes, and vegetation properly so-called; causes continually at work in Nature.—On a method of recording the calorific intensity of the solar rays, by M. A. Crova. A study of the curves obtained with his registering actinometer has enabled the author to estimate more accurately the value of the methods employed for determining this quantity, and to study the causes of the diurnal and annual variations of atmospheric absorption. He promises soon to communicate the method adopted by him for the study of the actinometric curves and its application to the determination of the law of atmospheric absorption.—The earthquake of February 23, 1887, by M. Albert Offret. In supplement to his previous communication, the author here gives in tabulated form the exact time when the shocks were felt in various places lying beyond the line of general movement. Appended is a corresponding table for the magnetic disturbances recorded at different observatories lying mostly beyond the seismic area, but evidently produced under the influence of the earthquake. A comparative study of these tables gives the unexpected result that the velocity of the seismic waves increases with the distance from the central area of disturbance.—Study of the effects of an electric shock felt during the earthquake of February 23, by M. Onimus. A detailed account is given of the severe shock felt by a person at Nice while working the telegraph at the moment the third seismic wave occurred. The incident seems to place beyond all doubt the fact that earthquake movements are normally accompanied by strong electric currents.—On the two species of Phylloxera of the vine, by M. A. L. Donnadieu. The two species of this organism, hitherto confused under one form, are here carefully distinguished and described under the names of *P. vastatrix* and *P. pemphigoides*.—On the direct photography of the barometric state of the solar atmosphere, by M. G. M. Stanoiéwitch. The author has made a comprehensive study of the solar photographs taken at the Meudon Observatory during the last eleven years, for the purpose of elucidating as far as possible the question of the origin of the solar photospheric network viewed in its relation with the solar pores, spots, and faculæ. The general result is that this phenomenon is nothing but the direct photograph of the barometric maxima and minima of the solar atmosphere.—On synthetic acetic acid and its derived forms, by M. Louis Henry. The author's researches show that monochlorureted acetic acid and malonic ac essentially one, always identical with themselves, forming single variety whatever be the nature of the acetic acid which they are derived.—On anemonine, by M. Hanric full description is given of the properties of this neutral nitrous substance, extracted by Heyer from different anem some forty years ago, but since then entirely neglect chemists.—On the creatines and creatinines, by M. E. Du This note deals mainly with the formation of α-amido cyamine and α-amidocaprocyamidine.—Variations of the phoric acid in cows' milk, by M. A. Andouard. The obj this paper is to complete our knowledge of the modifi which the composition of milk undergoes during lactation especially the variations occurring in the quantity of the phoric acid present during that period.

BOOKS, PAMPHLETS, and SERIALS RECEI

Anatomy of Movement: Dr. F. Warner (Paul).—Atlantic W Charts, part 2 (Eyre and Spottiswoode).—Die natürlichen Pflanzenfa 3 and 4 Lief.: A. Engler and K. Prantl (Engelmann, Leipzig).—A T on Geometrical Optics: R. S. Heath (Cambridge Press).—Sadd Mocassin: F. Francis (Chapman and Hall).—Jahrbuch der Natur schaften: Dr. Max Wildermann (Herder, Freiburg).—Proceedings Royal Society of Edinburgh, No. 122.—Naturæ Veritas: G. M. M (Macmillan).—Schriften der Physikalisch Ökonomischen Gesellsch Königsberg i. Pr. 1886 (Königsberg).—The Species of Ficus of the Malayan and Chinese Countries, part 1, Palæomorphe and Urost Dr. G. King (Reeve).—Notes from the Leyden Museum, vol. ix., (Brill, Leyden).—Journal of the Chemical Society, May (Gurne Jackson).

CONTENTS.

THURSDAY, MAY 26, 1887.

TREATMENT AND UTILISATION OF SEWAGE.

Treatment and Utilisation of Sewage. By W. H. Corfield, M.A., M.D. (Oxon.). Third Edition, Revised and Enlarged by the Author and Louis C. Parkes, M.D., Cert. Public Health (Lond.). (London: Macmillan and Co., 1887.)

OF late years, discussions about sewage have occupied a large share of the proceedings of many of the Societies concerned with the practical application of science, and recent scientific discoveries have as yet done little to modify the conclusions of the last ten or fifteen years. Dr. Farr's Report on Vital Statistics proves that increased density of population (if sanitary conditions remain constant) is itself a cause of increased ill-health and death, which can only be counteracted by increased precaution. And the prominent position occupied by England in sanitation must be ascribed to the constant efforts which have been made to cope with the increasing density of the population. A danger to health arises from density of population mainly because of the retention in our midst of the impurities which are the necessary accompaniments of the act of living, that is to say, the retention of those substances which putrefy, and from the products of the putrefaction of which various matters, or, it may be, organisms, inimical to life, become disseminated through the air.

The new, a third, edition of Dr. Corfield's record of the treatment and utilisation of sewage is a valuable contribution to the history of the development of the methods by which some of these evils have been counteracted. The first edition was the practical outcome of researches and experiments made by a Committee of the British Association, which was appointed to consider the evils arising from the unsystematic arrangements which prevailed at the inception of the water-carriage system for sewage. Before the introduction of the water-carriage system, refuse polluted the soil under and around the houses, the wells, and the air: when water-carriage was resorted to, it was thought sufficient to allow the dirtied water to flow to the nearest outfall, and the result was the pollution of our ditches, streams, rivers, and sea-shores. Our experience of the way in which these evils can be overcome has been gained slowly and tentatively; and Dr. Corfield's record of the various processes which have been tried and abandoned is not only useful as a means of preventing those methods that have been found unsuccessful from being brought forward again, but the account he gives of the causes of failure teaches important lessons to the sanitarian—lessons that may enable him to combat the insanitary conditions which a dense population is continually developing under new and unforeseen aspects.

Much as the subject of sewage disposal has been discussed, the varying conditions under which towns have to dispose of their sewage make it impossible that there should be any uniform method of disposal. It is abundantly certain that sewage contains elements of value. Dr. Tidy values the sewage at 8*s.* or 9*s.* per annum per head of the population, of which the solid part is worth 1*s.* 2*d.* Dr. Corfield estimates the sewage of London alone at between £1,000,000 and £1,500,000. Dr. Liebig estimated it at £4,000,000. But Dr. Hoffman, in 1857, summed up this question in the statement that the value in London sewage was like the gold in the sands of the Rhine—it amounted to millions, but it would not repay the cost of recovering it. Some of our first authorities on the subject, indeed, having most strongly advocated the commercial value of sewage, have ended, after years of labour, by saying, "Get rid of it in the cheapest manner; throw it into the sea if you can." The assumption that, because sewage has within it manurial value, therefore its removal ought to produce a profit, has had a most unfortunate effect on the treatment of this question. The search after the philosopher's stone of profit in sewage-disposal retarded the sanitary movement for years.

But, whilst it is easy to say, "Throw your sewage into the sea," it is rarely that we can so deal with it without injuring foreshores or tidal estuaries, and many sea-side resorts are suffering from such a method of disposal. A tidal river can only be safely resorted to under exceptional conditions. The discharge of crude sewage into a river, or, for the matter of that, on to land, is not satisfactory. For instance, the metropolitan sewage is poured into the tidal estuary of the Thames, where there is an enormous volume of water. Notwithstanding the purifying power of water, the sewage has seriously polluted the river beyond its capacity for purification in dry weather. On the other hand, it has been shown that the Barking and Halfway Reaches of the Thames, where the sewage is poured in, are really now better for navigation than they were before the metropolitan drainage outfalls were opened.

Independently, however, of the evils of the pollution of the tidal estuary of the Thames by the metropolitan sewage, we cannot conceal from ourselves that if some method of utilisation were feasible, even though it cost as much as we now pay for disposing of the sewage without utilisation, the resulting agricultural produce would be a gain to the nation. But there is not at present any generally-accepted plan for converting the metropolitan sewage into food, nor does it seem very probable that any method of treating the London sewage as a combined whole will enable us to do so usefully. The Metropolitan Board of Works, indeed, appear to consider it more prudent to submit to the known cost of loss than to embark on the more speculative course of endeavouring to rescue the valuable contents.

The most important question for the nation at the present time relates to other towns in the kingdom—that is to say, the question how, in the case especially of inland towns, can the sewage be purified so as to prevent it from damaging neighbouring properties, and make it fit to be passed into rivers. The best authorities are agreed on one point, viz. that it must not be sent crude into the rivers; and the preliminary straining off of the suspended matters is only a little less objectionable. Precipitation alone will not render the effluent water sufficiently pure; but if you let that effluent, after precipitation, flow over a small area of land, you will give the effluent the finishing touches towards purification. Precipitation by chemicals implies certain conditions. First, if you want to treat sewage properly by a precipitation process, you must treat it fresh, before active putrefaction sets in. Secondly, before you mix your chemicals with it, you should strain the sewage in some way or other. Thirdly, you should add sufficient chemicals to effect complete purification. Fourthly, there should be efficient stirring after the addition of the chemicals. Fifthly, it is essential that you should have sufficient tank accommodation, for two reasons: first, that the precipitate may subside perfectly; secondly, that the sludge may be frequently removed. If you allow the old sludge to remain in the tanks, it is perfectly certain that it will contaminate the fresh sewage when it comes in. When the sludge is taken out of the tank, the tank itself must be washed. By combining precipitation, which will produce a good effluent, with land treatment or prepared filters, you may produce the best effluent that is known.

Intermittent downward filtration through land will adequately purify sewage so as to allow the effluent to pass into a stream; but by this plan the manure which is so much wanted is almost entirely lost, the greater part escaping in solution in the effluent water in the form of nitrates and nitrites. On the other hand, if the effluent is used for irrigation farming, under necessary conditions of soil and methods of application, the sewage is purified, a certain agricultural return is obtained, and, provided the irrigated land is placed at a sufficient distance (say 500 yards) from houses, the health and comfort of the neighbourhood are not endangered.

Dr. Corfield thus sums up his conclusions:—

"Wherever it is possible, irrigation should be carried out, the sewage having been previously freed, by one or other of the methods described, from the offensive suspended matters, which must be deodorised to prevent the production of a serious nuisance. Wherever, on the other hand, irrigation is practically impossible, intermittent downward filtration through soil affords the means of satisfactorily purifying the sewage."

Drs. Corfield and Parkes say that these were the conclusions at which they arrived seventeen years ago, and that they see no reason to alter them now; but we much doubt whether finality on this question of sewage-disposal has been arrived at. The cremation of refuse on a systematic plan is of only a few years' standing, and at present of somewhat limited application. Moreover, we stand on the threshold of discoveries as to the more occult causes of infection: we are learning daily much of the history and habits of those lower forms of life which play so large a part in putrefactive changes, and which are in some cases proved to be baneful to us under the conditions in which they now occur, but whose action we might possibly learn to modify under enlarged knowledge. We have recently seen how the extraction of oxygen from the atmosphere has risen from being a toy to the position of a practical art. These discoveries may eventually have some bearing on the safe disposal of the refuse matter which is continually being formed in the midst of dense populations. During the last twenty years we have made rapid strides in the methods of removal and disposal of refuse, which have been the result of the free development of the intelligence of the community in sanitary matters: each of those years has marked some step of progress, and we continue daily to advance. It is therefore to be hoped that Parliament will not accept the views of those persons who seem now to be endeavouring to stereotype by Act of Parliament our present position in sanitation, as if it were perfection. Such a step might seriously check future progress.

THE POLYZOA OF THE "CHALLENGER" EXPEDITION.

The Zoology of the Voyage of H.M.S. "Challenger." Part XXX. "Report on the Polyzoa—Part II. The Cyclostomata, Ctenostomata, and Pedicellinea." By George Busk, F.R.S., &c. (Published by Order of Her Majesty's Government, 1886.)

THE first and second memoirs contained in Vol. XVII. of the Zoological Reports of the Voyage of the *Challenger* were reviewed in NATURE two weeks ago (p. 26). The third memoir, the subject of the present notice, formed the last piece of scientific work of the distinguished naturalist to whom the preparation of the Report on the Polyzoa had been intrusted. During a period of illness and suffering under which the energies of most men would have broken down, Mr. Busk still laboured to accomplish the task which he had undertaken, and it was only a few days before his death that he was enabled to bring it to a conclusion.

The author deemed it advisable to divide the Report on the Polyzoa collected during the great exploratory voyage into two parts. The first of these has already been reviewed in NATURE (vol. xxxi. p. 146). It is confined to the Cheilostomatous species, and includes by far the greater number of all the Polyzoa collected. There still remained for consideration such species as are referable to the three remaining sections, namely, the Cyclostomata, the Ctenostomata, and the Pedicellinea. Each of these three groups has its representatives in the present Part, but the number of these is small in comparison with those referable to the Cheilostomata, and there does not occur among them any generic form which can be regarded as new. The account of them here given, while it completes the Report on the Polyzoa collected during the expedition, is characterized by all that careful and exact work which invariably marked the scientific labours of its author.

The entire number of species included in the present Part is forty-six, of which thirteen are now described for the first time. Of these forty-six species the most interesting are probably the two referable to the section Pedicellinea, and placed by the author in his genus Ascopodaria. The Pedicellinea form a very aberrant group of Polyzoa, presenting characters which differ widely from those met with in typical Polyzoal structure. They are represented in our own seas by two or three species of the curious genus Pedicellina, with its naked pedunculated polypides destitute of the "cells" into which the polypides of other Polyzoa admit of being retracted. The genus Ascopodaria is rendered further remarkable by the flask-like dilatation with muscular walls which exists at the origin of each peduncle. The structure of this form is worked out in the Report with great care, and is illustrated by excellent figures depicting for the first time the anatomy of the genus as far as spirit specimens would admit of its demonstration.

The Report enters fully into the geographical and bathymetrical distribution of the species included in it. Of these the Cyclostomata attain the greatest depth, though only two of them extend to depths greater than 1000 fathoms; namely, *Crisia elongata*, which was obtained in the Australian region from a depth of 1450 fathoms, and *Idmonea marionensis*, which was brought up from a depth of 1600 fathoms in the region of Kerguelen Land. It is a fact, however, by no means without significance, as showing how little certain marine organisms of even complex structure are dependent on depth, that in the case of the last-mentioned species specimens have been obtained from depths varying from 50 fathoms downwards. The Ctenostomata and Pedicellinea are all from comparatively shallow water, none having been obtained from a depth greater than 150 fathoms.

No one could have been found better qualified than Mr. Busk to institute a comparison between recent and fossil Polyzoa. His work on the Polyzoa of the Crag is among the most important contributions we possess to the palæontology of this group, and gives a special value to his determination of the fossil relations of the species collected by the *Challenger*.

To the sub-order Cyclostomata belong the oldest fossil Polyzoa as yet known, and out of the thirty-three species of Cyclostomata obtained by the *Challenger* Mr. Busk has been able to identify fourteen as occurring also in a fossil state, thus proving the wide distribution in time of even specific forms of this group. No fossil species has as yet been identified with either the Ctenostomata or the Pedicellinea. The negative evidence, however, which is all that this statement expresses, proves but little, as these groups are destitute of structures which might be expected to continue recognizable in a fossil state. Barrois, indeed, contends that the larval stage of the Entoprocta (Pedicellinea) represents the primitive form from which the whole of the Polyzoa have descended. Of the Cheilostomata—the sub-order to which the former part of the Report is confined—no species has as yet been proved to belong to Palæozoic times, though this group is largely represented in Mesozoic and Tertiary strata.

The ten beautiful plates which illustrate this part of the Report contain figures of all the newly-described species of Cyclostomatous, Ctenostomatous, and Pedicellinean Polyzoa, and bear ample evidence to the conscientiousness and accuracy with which all the details of form are delineated.

The purely descriptive part of the Report is marked by all that judicious selection of characters, and succinctness yet definiteness of diagnosis, which add so much to the facility of comparison and to the practical value of any work having for its object the determination and description of specific forms. The number and variety of the species and generic types described and figured in this and the former part of the Report give to the whole a special value, not only as a record of the species collected, but as a faithful and comprehensive picture of the external morphology of the important and interesting group of organisms to which it is devoted. G. J. A.

OUR BOOK SHELF.

Dynamics for Beginners. By the Rev. J. B. Lock, M.A. Pp. 178. (London: Macmillan and Co., 1887.)

THIS book is an attempt to explain the elementary principles of dynamics in a manner suitable for school-work with boys of ordinary mathematical attainments. Accordingly it contains a great number of easy *numerical* examples, some worked out in illustration of the text, the others arranged in groups at frequent intervals. There is considerable freshness in these exercises, and they form altogether a very useful series.

The work is divided into four sections. The first treats exclusively of rectilinear dynamics, thus avoiding at the beginning of the subject all purely geometrical difficulties.

The second section introduces the notion of directed or vector quantities, and deals with the application of the parallelogram law to displacements, velocities, accelerations, and forces in succession.

Next we have a section on applications of the preceding to projectiles, oblique impact, circular motion, and relative motion, concluding with a short chapter on the hodograph.

The final section deals with energy, work, and power. These last three or four chapters read in connexion with the first section would form a suitable first course in many cases, involving no mathematics beyond a knowledge of simple equations in algebra.

The exposition throughout is remarkable for clearness and precision of statement. The definitions of terms seem particularly well worded. The names *velo* and *celo* have been adopted for the units of velocity and acceleration, and are used systematically in both text and examples; we hope these terms may win their way to general acceptance, for the language of the subject gains both in simplicity and directness by their introduction.

The debt of gratitude which many teachers and students already owe to Mr. Lock will be considerably increased by this new class-book on a difficult subject, wherein it appears to us that the skill and experience of the author are displayed with great advantage.

Journals kept in Hyderabad, Kashmir, Sikkim, and Nepal. By Sir Richard Temple, Bart., M.P. Edited, with Introductions, by his son, Richard Carnac Temple. With Maps and Illustrations. Two Vols. (London: W. H. Allen and Co., 1887.)

THE first journal contained in these volumes was written at Hyderabad during the year 1867, when the author was Political Resident at the Court of the Nizám. It is entirely political, and will interest only those who study somewhat minutely the course of recent Anglo-Indian history. The journals kept during visits to Kashmir, Sikkim, and Nepal appeal to a larger class of readers. They deal with the physical features of these countries, and to some extent with social customs and institutions. Most of the author's notes are too slight to be of much scientific importance; but all of them have the merit of being written in a clear and unpretending style, and the information contained in them is, so far as it goes, thoroughly trustworthy. The introductions which the editor has contributed to the book add very considerably to its value. They are careful essays, in which Capt. Temple has brought together a great many interesting and suggestive facts that are not readily accessible to ordinary readers.

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. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Sunlight Colours.

WILL you permit me to say, in relation to the very interesting lecture on Sunlight Colours, reported in NATURE, vol. xxxv. p. 498, that Capt. Abney does not seem to have quite apprehended my meaning, when he represents me as stating in a previous lecture at the Royal Institution, that the sun was "really blue outside our atmosphere," for I nowhere in the lecture used those words, nor intended to convey the idea which, without qualification, they must give the reader.

I recognize, however, that if my actual words conveyed it to so fair-minded a critic as Capt. Abney, they must have been open to misconstruction, and I therefore ask permission to recall in explanation an important fact referred to in the lecture, to which he does not allude. It is that the sun is surrounded by an atmosphere of its own, and that the prime modification of its actual colour at the photosphere takes place *there*. Only the secondary change of colour takes place in the earth's atmosphere. "Outside our atmosphere," accordingly, we see, not the absolute colour of the photosphere, but one already greatly modified toward white. I meant, then, when formally defining the colour of the sun outside our atmosphere, to use such qualified phrases as "tends toward blue," or "bluish," and it was for the colour of the sun itself, *i.e.* at the photosphere, and before any absorption, that I meant to reserve the word "blue." Let me hasten to add that I also tried—even to iteration—to insist that "blue" here does not and cannot mean a monochromatic blue, but a combination of all the spectral colours, in which those of the blue end appear in such immense predominance that this is the dominant effect.

Capt. Abney also says: "he" (I) "surmised the result from experiments made with rotating disks of coloured paper. He did not, I think, try the method of using pure colours."

Capt. Abney will, I think, agree on consideration that these words may be liable to convey to most readers a wrong impression of labours which began nearly fifteen years ago, with studies on the absorption of the sun's atmosphere, resting on direct and elaborate photometric comparisons of the light of its centre and edge. These have been followed by confirmatory measures with the bolometer, giving the relative proportions of the pure colours in the normal spectrum, and the tint has not been surmised, but experimentally shown by the actual combination of pure spectral colours.

The solar studies were supplemented in the four years preceding my lecture by almost unintermittent investigations on the absorption of the earth's atmosphere, in which (though considerably over 20,000 galvanometer readings were recorded) I do not recall ever making any observation by the aid of "rotating disks of coloured paper." The paper disks have been often employed in explanation of my method, to roughly show the principles involved, and to *illustrate* results, but certainly not as means by which these results were surmised or discovered.

In a communication to the British Association, published in NATURE, vol. xxvi. p. 586, after alluding to the antecedent researches of Mr. Lockyer and others, which show that certain rays of short wave-length are more absorbed than those of long, I exhibited charts showing how much each ray had grown. One of these, which suffered some curtailment at the hands of the engraver to fit it to the height of the page, was reproduced in the report of the lecture (NATURE, vol. xxxii. p. 42), and it is possibly from this that Capt. Abney derives his impression as to my results in other respects. I can only conjecture that it may be so, since in my professional memoirs there are, not only more accurate charts, but with them warnings that the figures representing the relation of the blue and red end in such drawings, or even in the tables whence they are taken, necessarily give minimum values of the blue.

The fact that this blueness was first predicated from a long and careful study of the absorption of the sun's atmosphere is a distinct one, and I am entirely disposed to admit that this point was not explained at sufficient length in my lecture, in which I had but an hour to describe the work of twelve years. Being forced to confine myself to an account of some limited portion of this long research, I chose that part of it which dealt with the absorption of the earth's atmosphere, as illustrated by the expedition to Mount Whitney, but I thought the facts just stated about the influence of the sun's atmosphere too important to go without explanation altogether, and rehearsed them substantially in other words before entering at length on the subject of the telluric absorption.

As the observations on the sun's atmosphere are still unpublished, it may be of interest if I give here, in anticipation of the final reductions, the approximate results of some made at Allegheny in 1882, and which were supplemented by others which I was enabled to make at South Kensington in the same year by the kindness of Mr. Lockyer.

This table gives the reduction to the normal spectrum at the points indicated in the first line, where λ designates the wave-length and μ = one micron = 1/1000 of one millimetre. The second line gives the approximate transmission by the solar atmosphere (not alluded to in Capt. Abney's lecture). The third line gives the approximate transmission by the earth's atmosphere alone (numbers nearly concordant with those he seems to employ for this secondary effect); and the fourth, the combined effect of the two. It is from such numbers as those in this fourth line that we have deduced the true colour of the sun at Allegheny, by methods to be presently alluded to, and which authorize us to state that its dominant tint before any absorption is not so much "bluish" as "blue."

	μ 0·40	μ 0·45	μ 0·50	μ 0·55	μ 0·60	μ 0·65	μ 0·70	μ 0·75
Transmission by solar atmosphere	·16	·24	·30	·35	·38	·41	·43	·45
Transmission by terrestrial	·31	·44	·53	·61	·68	·74	·79	·83
Resultant transmission by both atmospheres	·05	·11	·16	·21	·26	·30	·34	·37
Reciprocal of last, showing approximate brightness before any absorption...............	20·2	9·5	6·3	4·7	3·9	3·3	2·9	2·7

Thus we see that of the extreme blue or violet light, whose wave-length is $0·4\mu$, 16 per cent. (*i.e.* less than $\frac{1}{6}$) only is transmitted by the solar atmosphere, and of this 16 per cent. 31 per cent. only is transmitted by the earth's atmosphere. It is of this latter alone that Capt. Abney here takes account, but in consequence of the absorption by both atmospheres, only about 5 per cent. of the original violet light reaches us; or in other words, before the double absorption there was over twenty times as much of this sort of blue in the sun as what we now see. On the other hand, of the deep red light whose wave-length is $0·75\mu$ as much as 45 per cent. is transmitted by the solar atmosphere, and of this again 83 per cent. by the earth's; so that after the action of both atmospheres on this ray 37 per cent. is transmitted as against 5 per cent. of the violet. If we take the reciprocal of the numbers in this fourth line we have those of the fifth, which evidently show the relative intensity of the colours at the photosphere (*i.e.* before any absorption), as compared with that of common daylight. I employed in 1882 an optical arrangement, suggested by Mr. Very of the Allegheny Observatory, by which we passed from these figures to the production of the actual resultant tint of the solar photosphere; not by using pigments or revolving disks, but by the direct combination of pure spectral colours in the above proportions. The resultant colour cannot, I repeat, be exactly defined by any one spectral one, as it was not monochromatic; but the tint was, to my eye and that of others, best technically defined as that of Herschel's lavender, with perhaps a suggestion of purple; and certainly I think now, as I thought then, that "blue" is the nearest familiar word to describe it.

It was with all these facts, and many more, in my possession, that I used the language in question.

I hope after this statement that I may conclude that Capt. Abney and I have really no serious ground of difference as to the propriety of the term "bluish," or as to what it here means. I would only say that by no latitude of interpretation do I take it as meaning *white*. S. P. LANGLEY.

Smithsonian Institution, Washington, D.C., May 2.

The Eclipse of August 19, 1887.

THIS eclipse will be seen over such an extent of territory that it lesirable to make the best use of the opportunity offered. The ronomical observations I do not mention, but besides them e following would be very important, and could be made by avellers alone, and those who do not take with them heavy and oublesome instruments :—

Observations every ten minutes on the *pressure* and *tem-rature* of the air from the beginning of the eclipse to about lf an hour after its end; and, some days before and later, ery hour, at the hours of the eclipse.

The *barometer* might as well be an aneroid, but with large visions; a pocket instrument would be too small. Relative and ot absolute measures are intended, and it is especially necessary at the instrument be not sluggish.

The *thermometer* preferable for the observations should be a ng-thermometer (Frowde), as one in a thermometer-stand and t swung could not follow rapidly enough the changes of tem-rature. It would be best to swing it at the height of the oulder.

Observations on *cloud, direction* and *force of wind*, every lf hour the day of the eclipse and every hour before and er.

Some observations on the colour of the sky, &c., and on e influence of the eclipse on animals, domestic and wild, ould be useful.

The eclipse will be visible in Eastern Germany, but at so early an ur in the morning that there will be comparatively little interest meteorological observations. Russia (especially Eastern) and estern and Central Siberia give much better opportunities of servation. I give below some notices on the amount of cloud; e stations are disposed from west to east, the mean is that of ree observations, 7 a.m., 1 p.m., and 9 p.m. The conditions as to udiness will be better than those indicated here, in Eastern ıssia and Siberia to nearly Lake Baikal, as the eclipse will be n in the later morning hours, which have a smaller amount of ud than 7 a.m. and 1 p.m.

Amount of Cloud.

	Mean	7 a.m.	1 p.m.
Rjev, Government of Tver	57	49	
Moscow	57	50	
Academy of Petrovsky, near Moscow	51		
Rojdestwenskoye, Government of Kostroma	60	61	
Kasan	58	55	
Viatka	53	51	
Ekaterinburg	68	67	
Nijnetaguilsk	64		
Bogoslovsk	57		
Irbit	53		
Irbit			69
Yeniseisk	53		
Irkutsk	49	48	
Foundry of Nertschinsk	53	59	
Niigata, west coast of Nippon	55		

[give a list of some places where tolerably good accommo-ion is to be found, with the time of travel from the nearest lway-station :—

Tver, Torjok, Moscow,[1] Yaroslav,[2] Kostroma (three hours' umer from latter point), Schuja, Ivanovo-Wosnessensk, ıeschma, Vladimir, Viatka (steamer on Volga, Kama, and ıtka, from Nijni-Novgorod, in three days), Perm (steamer n Nijni-Novgorod in eighty-five hours.

Vijnetaguilsk, with important foundries, malachite mines, &c., ched by railroad from Perm in fifteen hours.

Tobolsk, by rail from Perm to Tjumen in about thirty hours, nce by steamer in two days, twice a week. It is well to graph beforehand to retain a cabin.

Tomsk by steamer from Tjumen in about eight days, by the a, Tobol, Irtysch, and Ob.

The places eastward, the most favourable for observation, can reached by road only from Tomsk. Post-horses everywhere ilable, rapid travelling in good weather, but bad carriages.

To astronomers bringing with them bulky instruments, the er-ways are to be recommended. St. Petersburg is in easy mer communication with British harbours, and thence luggage can be sent by water to all parts of the Volga basin. So far as known at present, it is intended that there shall be observations of the eclipse at five points: (1) the observatory of General Maiewsky, Government of Tver; (2) the estate of Count Olsuffiew, district Dmitrov, Government of Moscow; (3) the estate of Prof. Bredichin, district Kineschma, Government of Kostroma,—two English astronomers are expected; (4) Glasov, Government of Viatka; (5) Krasnoiarsk, on the Yenisei.

A. WOEIKOF.

[1] Just at the southern limit, it would be better to observe somewhat to north.

[2] See Mackenzie-Wallace's "Russia."

Iridescent Clouds.

THE clouds seen by Prof. Stone, as described in NATURE, vol. xxxv. p. 581, may have been of the same character (though I cannot judge positively from the description) as those so extensively observed in the Decembers of 1884 and 1885; if so, it is the only account I have read of their being seen last winter. Those described by Mr. McConnel, writing from St. Moritz, Switzerland (p. 533), are evidently of a totally different character, and I suppose simply the ordinary iridescent clouds which are common everywhere.

T. W. BACKHOUSE.

Sunderland.

Remarkable Hailstones.

MAY I ask for space to make a suggestion as to the possible cause of the banded structure of hailstones recently observed and recorded in NATURE, vol. xxxv. p. 438? It seems to me that the phenomenon may perhaps be explained by *devitrification of the ice.* We are familiar with a considerable number of bodies which assume the vitreous state by rapid solidification from the liquid state; and it seems reasonable to suppose that in the conditions under which hail is formed the ice may assume at first the vitreous state, the higher molecular structure of perfectly crystalline ice requiring more time for its full development (see paper by the writer read before Section C of the British Association last year at Birmingham). If such were the case (and the hypothesis is supported by the statement of Mr. C. S. Middlemiss in NATURE, vol. xxxv. p. 413), the observed structure (which can be actually seen to develop itself in some vitreous substances under the microscope, as a preliminary to the assumption of the full crystalline and opaque condition) would simply mark an early stage of the devitrification of the ice-glass. To bring this theory to the test of experiment it would only be necessary to observe closely the effect of keeping such hailstones for some time at a temperature rather below 0° C.

A. IRVING.

Wellington College, Berks, May 14.

The Orbit of the Minor Planet Eucharis.'

ON reading your note (p. 16) on the determination of the orbit of the planet *Eucharis*, by Dr. de Ball, and the discordances between his observations and those obtained with the Washington meridian instrument, I am reminded of an earlier case which seems to me to be analogous.

Hansen drew attention to the very material difference between the observations of *Egeria* in 1864 at Bonn and Leyden. This discrepancy between observations which otherwise harmonized well amounted to 10″ in R.A., and occasioned a protracted inquiry by Argelander (*Astron. Nachr.*, No. 1769), in which he came to the conclusion that the reason probably lay in the personal error of the Leyden observer in the observation of bright and faint stars. As I am not acquainted with Dr. de Ball's treatise, I cannot judge whether respect was paid to such differences in isolated cases.

W. VALENTINER.

Karlsruhe Observatory, May 8.

A Question for Chemists.

YOUR correspondent, Mr. West, will find reference to the fact that a mixture of glycerine and potassium permanganate is liable to spontaneous combustion in the "Extra Pharmacopœia" of Martindale and Westcott, fourth edition, p. 292.

Dublin.

HARRY NAPIER DRAPER.

"A Junior Course of Practical Zoology."

IN a recent notice of "A Junior Course of Practical Zoology" (NATURE, vol. xxxv. p. 506) the reviewer expresses surprise that anyone should, in a text-book for students, "discard the ophthalmic somite of their seniors, and press the telson into the service," a procedure on which he comments thus:—"The introduction of so sweeping a change into a book for juniors, without due comment is, under these circumstances, a false step, especially when it is considered that the precise converse is stated in all other books current."

Now Claus in his text-book says (I quote from the English edition):—"The facetted eyes are borne on two movably separated stalks. These were for a long time considered as the anterior pair of appendages, while in fact they are merely lateral portions of the head which have become jointed"; and elsewhere: "The last abdominal segment, which is transformed into a telson."

Gegenbauer in his text-book says:—"The projecting character of the eye, owing to its curvature, may lead to a stage in which the eye is stalked. When still more developed, this stalk may become movable"; and nowhere speaks of the stalk as the homologue of an appendage.

Prof. Lankester's pupils are all taught to regard the telson as a somite and the "ophthalmic somite" as an erroneous interpretation of parts.

I fail to see, therefore, that Prof. Marshall need offer any excuse for his method of counting the segments, nor, in an elementary text-book, discuss a question on both sides of which there is avowedly much to be said.

I may note with regard to one other criticism that, although there is nothing "irrelevant or absolutely fantastic" about the term commissure, it is convenient to distinguish between "commissures connecting two ganglia of the same pair" and "connectives connecting ganglia of dissimilar pairs" ("Encycl. Brit.," ed. ix. Art. "Mollusca"). The "word-mongerers" are here marking "a turning-point in advance."

Madras, April 20. A. G. BOURNE.

"On the Establishment of the Roman Dominion in South-East Britain."

IN my article on the above subject printed in NATURE, vol. xxxv. p. 562, I have briefly alluded to the ridiculous mutiny of the Roman soldiers. I ought to have added (from Dio) the relation of the following incident, which terminated the mutiny:—

"Taking courage, because a brilliant meteor rising in the east passed across to the west, to the part to which they were making their course, they descended on the island."

That is, the Romans descended from an easterly part of Europe upon Britain.

This agrees with the course which in my former letter I assigned as most probable; namely, that the Romans sailed from the mouth of the Scheldt to Southend. G. B. AIRY.

The White House, Greenwich, May 18.

FLORA OF CHRISTMAS ISLAND.

THE Hydrographer of the Admiralty has kindly forwarded to Kew, as he has stated in his note in NATURE for May 5, p. 12, the botanical specimens collected during the visit of H.M.S. *Flying-Fish* to Christmas Island. They were unfortunately, as explained by Capt. Maclear, a mere residue of the collection which was obtained. The examination of a better preserved and more extensive one would be interesting, as the flora is evidently of a less common-place kind than that generally met with in coral islands.

In all, twenty-four species admitted of approximate determination. Of these five were ferns, all widely-spread species. Of the remaining nineteen flowering plants five are also probably identical with widely-distributed species, and they occur in the Cocos or Keeling Islands between which and Java Christmas Island lies. The much more limited flora of these islands is only known from the collections of the late Mr. Darwin, and of Mr. H. O. Forbes. Of the remaining fourteen species at least six must be set aside, the specimens being too imperfect to be more than approximately determinable. Of the rest, two, a *Vitis* near *V. pedata*, Vahl, and an *Ehretia*, may, in Prof. Oliver's opinion, possibly be new; the teak (*Tectona grandis*, L. f.) occurs generally in the Malayan Archipelago; *Euphorbia Chamissonis* is interesting as a Polynesian type; fruits of *Barringtonia* are thrown up universally on shores in the Malayan waters; *Terminalia Catappa*, L., is found pretty well everywhere in the tropics; the remaining two suggest no special remark.

The collection unfortunately throws little light on the composition of the dense arborescent vegetation with which Capt. Maclear found it to be covered. Teak probably forms large trees. *Cordia subcordata*, Lam., which occurs also in the Cocos-Keeling Islands, and, according to Mr. H. O. Forbes, originally covered them abundantly,[1] is known there as "iron-wood," and is no doubt one of the iron-wood trees recognized by Capt. Maclear in Christmas Island. It is widely distributed throughout the Malayan Archipelago, and extends to the Philippines and some of the Pacific islands.[2]

On the whole, it can hardly be doubted that Christmas Island has been stocked with its flora by the agencies described by Dr. Guppy, and worked out by Mr. W. B. Hemsley in the "Botany Report of the Voyage of H.M.S. *Challenger*" (vol. i. part 3, p. 310): "Winds and currents drift to their shores the fruits and seeds of the littoral trees which ultimately form a belt, whilst the fruit-pigeons disgorge the seeds or fruits of those often colossal trees which occupy the interior."

The former agencies brought no doubt *Barringtonia*, *Hibiscus tiliaceus*, *Terminalia*, *Cordia subcordata*, *Ochrosia parviflora*, and *Pandanus*. Carpophagous birds are elsewhere known to bring a profusion of fruits of palms, nutmegs, *Euphorbiaceæ* and *Laurineæ*, and other arborescent species. Upon this element in the flora of Christmas Island the collection, as already remarked, throws insufficient light. The flora of Java is still but imperfectly known, and though there is no reason to believe that that of Christmas Island contains any absolutely endemic species, it would not be surprising if a careful examination yielded many plants new to science which have yet to be ascertained from the larger contiguous island, from which they have been derived.

W. T. THISELTON DYER.

THE JOURNAL OF THE ROYAL MICROSCOPICAL SOCIETY—RETROSPECTIVE AND PROSPECTIVE.

THE month of March 1878 will ever remain memorable in the annals of microscopy in this country, for it marked the regeneration of the Journal of the Royal Microscopical Society, the most conspicuous feature of which was the introduction, for the first time, of a systematic record of current researches under the title of "Notes and Memoranda." Now that the period of editorship which worked the change is fast approaching its decade, we would wish to review the position, in anticipation of the introduction of still further modifications which, it is to be assumed, the editors will adopt on entering upon a second period.

We read in the preface to the first volume that the "Notes and Memoranda" are intended to present a summary of what is doing throughout the world in all branches of microscopical research. Whilst extracts from English publications will not be excluded, preference will be given to those of foreign countries, as being less easily accessible. Amongst these will be included the Transactions and Proceedings of the Academies of the United

[1] "Naturalist's Wanderings in the Eastern Archipelago," pp. 28, 29.

[2] Mr. H. O. Forbes (*l.c.* pp. 26, 27) gives a curious account of the way in which the labours of a crab turn the white calcareous fore-shore of coral islands into "a dark vegetable mould." They do this by burying systematically particles of vegetable *débris*; by scooping away the soil beneath them they lower down even large branches of trees. The ground thus enriched is fitted for occupation by plants; and as Mr. Forbes particularly noticed that they carry "down also the newly-fallen seeds of the iron-wood" these industrious factors in the economy of a bare coral island not merely prepare the soil but also plant it.

tates, France, Belgium, Germany, Austria, Italy, and ussia, together with the microscopical, botanical, and ological journals of those countries. It will be obvious anyone who will compare the last few numbers of the ournal with the first volume, from which we have just uoted, that the former are no less superior to it in general xcellence than it was to its immediate predecessors. The ditors have elaborated their scheme with the growth of e Journal, and have, in their desire to satisfy the public, one beyond the prescribed limits, and incorporated bstracts of all the more important papers in certain ranches of the science, whether microscopical or not.

In no period of the history of biological science has dvance been so rapid as within the last decade, and it no exaggeration to say that the Journal before us is a ithful historical record of the work done during that eriod, in those branches with which it professes to deal. 'o him who would labour in earnest at a given subject the riginal monographs are indispensable; but even the arrowest of specialists must obtain some knowledge of e advance made in cognate branches of his science, and ready means of acquiring this, as it applies to micro-:opy, has been provided by the Journal named during e period of which we write.

It might naturally be supposed that the increase in ative workers, whose labours have so far extended e literature of the science and consequently swelled e pages of the Journal in which that literature has een abstracted, must have resulted in a corresponding crease in the circulation of the Journal itself. This, we e informed, has not been the case. In reflecting upon is fact we must remember that during the past de-ade many changes have been wrought in the literature of ological science. Anzeigers and Records have been tablished and augmented. But withal the "Notes and emoranda" of the Society's Journal have made a place r themselves in the library of the working biologist; e abstracts are up to date, and frequently fairly detailed, d they are invaluable to workers who, though not tual specialists, are so placed as to be beyond reach of good reference library.

The Journal is primarily a microscopical one, and such must continue to be under the Charter of the Society hose organ it is. Supplemental matters are added by urtesy; but we believe the editors would do well to strict themselves to purely microscopical matters. In ese days of profuse literature showered upon us from l parts of the globe, it is highly desirable that the aims d scope of all journals should be clearly defined and lhered to, if only by way of enabling the worker to ow approximately where to turn in search of informa-on upon a given subject. Much has been done of late this direction by other Societies, and we submit the ggestion to the executive of the one whose Journal we e considering, in full assurance that in restricting their bours as indicated they will be still further contributing the utility and success of their venture. We would so suggest that pains might occasionally be taken to t forth more fully than hitherto the precise vantage ined by authors quoted, to the exclusion of purely his-rical *résumés* and details of minor importance. The al points of a paper are occasionally sacrificed to the producing of descriptions of insignificant structural tails; and attention to this point would, we believe, hance the value of the abstracts without in any way ngthening them. Further, work in the native tongue s not always received that attention which it merits.

The editorship of the Journal could not be in better nds than at present. Officers of the Society and all gaged have laboured indefatigably, and they deserve stinted praise in the execution of their somewhat thank-s task. Under the present editorship the Journal s attained a definite and responsible position, beyond at which it occupies as the organ of a chartered Society; its pages are quoted as authoritative records, and we would fain see it more widely disseminated than at present. It is pre-eminently a microscopical journal for workers; it stands unique in its combined features, and is second to none extant in its dealing with the *technique* and optics of the subject. If it is deemed worthy of the formulæ of Abbé, and of orginal articles by the President of the Royal Society, it is deserving of maintenance at the hands of English-speaking people.

BRIDGING THE FIRTH OF FORTH.[1]

DURING the past four years many thousands of visitors from all parts of the United Kingdom, and, indeed, I may say from all parts of the world, have more or less carefully inspected the works now in progress under the superintendence of Sir John Fowler, the engineer-in-chief, and myself, for bridging the Firth of Forth. All classes of visitors, whether possessed of technical knowledge or not, have found at least something to interest them amongst the multifarious operations incidental to carrying out so gigantic an undertaking; and I should have little fear of interesting my present audience if I could change the scene from Albemarle Street to the shores of the Forth. That is impossible, so I must rest content with an imperfect attempt to convey to you, by description and illustration, some notion of the magnitude of the proportions and difficulties of construction of what is generally admitted to be one of the most important engineering works yet undertaken. A "personally conducted" tour over the work would be far more congenial to me than giving a lecture, and infinitely more effective. Photographs, and even the highest efforts of pictorial art, are a poor substitute for the reality. The smallest street accident witnessed by ourselves affects us more than a description or picture of the greatest battle, and for similar reasons I well know that when I speak of men working with precarious foothold at dizzy heights in stormy weather my words will sound very different in this room to what they would were my listeners standing beside me in an open cage hanging by a single wire rope, in appearance like a packthread, and swinging more or less in the wind at a height of between three and four hundred feet above the ground; or were they following me up a ladder as high as the golden cross on the top of St. Paul's Cathedral, with the additional excitement of the rungs of the ladder being festooned with icicles a foot long. You will lose a great deal in vividness of impression necessarily by the substitution of a lecture for a personal visit to the works, but there are some compensating advantages, as you will be saved between eight and nine hundred miles of railway travelling, and a good deal of clambering of the kind shadowed forth.

I should not have thought it necessary to preface my remarks by the statement that the Forth Bridge has nothing to do with the Tay Bridge, had not my four years' experience informed me that about one-half of my fellow-countrymen labour under that singular hallucination. Even at this date I fully expect every second Britisher (of course Americans and foreigners are better informed) to say: "How are you getting on with the Tay Bridge?" I suggest "Forth Bridge," and the correction is generally accepted as a mere refinement of accuracy on my part. As a matter of fact, however, the Tay Bridge which was blown down in 1879, and has since been rebuilt, is at Dundee, whilst the Forth Bridge is near Edinburgh; and as regards type of construction there is nothing in common between the two. If my lecture serves no better purpose, it will at least help, therefore, to disseminate a little useful geographical knowledge respecting the Firths of Forth and Tay.

[1] Lecture delivered at the Royal Institution, on Friday, May 20, by B. Baker, M. Inst. C.E.

And yet the Forth which "bridled the wild Highlander," and especially that part of it where the bridge crosses, should be well enough known to every reader of fiction, for it has been made the scene of many adventures. Mr. Louis Stevenson's thrilling story, "Kidnapped," will have been read by most of you; the hero of that story was kidnapped at the very spot where the bridge crosses, so I can describe the point of crossing in David Balfour's own words:—

"The Firth of Forth (as is very well known) narrows at this point, which makes a convenient ferry going north, and turns the upper reach into a land-locked haven for all manner of ships. Right in the midst of the narrows lies an island with some ruins; on the south shore they have built a pier for the service of the ferry, and at the end of the pier, on the other side of the road, and backed against a pretty garden of holly-trees and hawthorns, I could see the building which they call the Hawes Inn."

Such was the appearance of the spot 150 years ago. The middle pier of our bridge now rests on the island referred to, and the Hawes Inn flourishes too well, for being in the middle of our works its attractions prove irresistible to a large proportion of our 3500 workmen. The accident ward adjoins the pretty garden with hawthorns, and many dead and injured men have been carried there, who would have escaped had it not been for the whisky of the Hawes Inn.

I would wish if possible now to convey to my hearers some clear impressions of the exceptional size of the Forth Bridge, for even those who have visited the works and noted the enormous gaps to be spanned on each side of Inch Garvie, may yet have gone away without realizing the magnitude of the Forth Bridge as compared with the largest railway bridges hitherto built. For the same reason that architects introduce human figures in their drawings to give a scale to the buildings, do we require something at Queensferry to enable visitors to appreciate the size of the Forth Bridge. If we could transport one of the tubes of the great Britannia Bridge from the Menai Straits to the Forth, we should find it would span little more than one-fourth of the space to be spanned by each of the great Forth Bridge girders. And yet it was of this Britannia Bridge that Stephenson, its engineer, thirty years ago said:—"Often at night I would lie tossing about, seeking sleep in vain. The tubes filled my head. I went to bed with them, and got up with them. In the gray of the morning, when I looked across Gloucester Square, it seemed an immense distance across to the houses on the opposite side. It was nearly the same length as the span of my tubular bridge!"

Our spans, as I have said, are each nearly four times as great as Stephenson's. To get an idea of their magnitude, stand in Piccadilly and look towards Buckingham Palace, and then consider that we have to span the entire distance across the Green Park, with a complicated steel structure weighing 15,000 tons, and to erect the same without the possibility of any intermediate pier or support. Consider also that our rail level will be as high above the sea as the top of the dome of the Albert Hall is above street level, and that the structure of our bridge will soar 200 feet yet above that level, or as high as the top of St. Paul's. The bridge would be a startling object indeed in a London landscape.

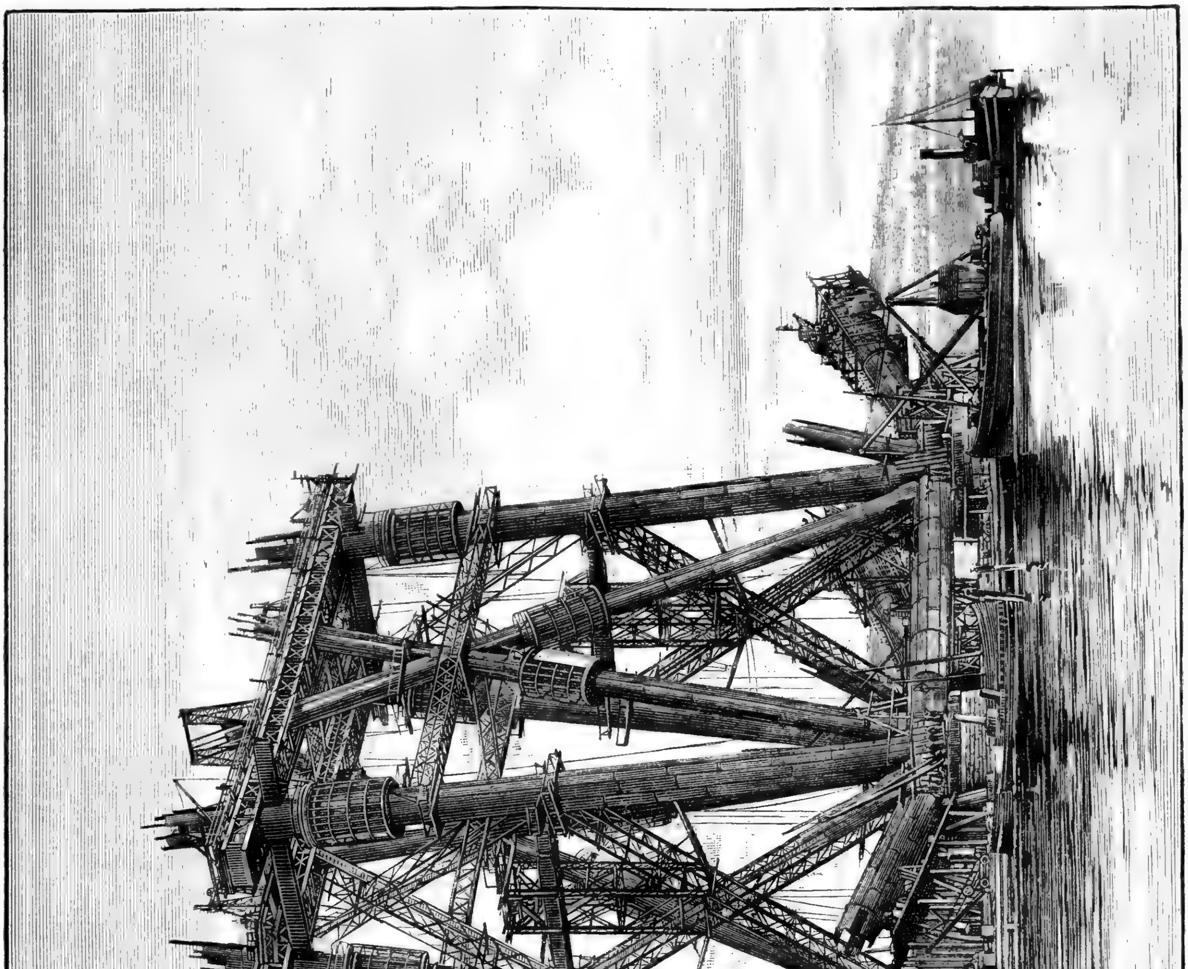
It is not on account of size only that the Forth Bridge has excited so much general interest, but also because it is of a previously little-known type. I will not say novel, for there is nothing new under the sun. It is a cantilever bridge. One of the first questions asked by the generality of visitors at the Forth is, Why do you call it a cantilever bridge? I admit that it is not a satisfactory name and that it only expresses half the truth, but it is not easy to find a short and satisfactory name for the type. A cantilever is simply another name for a bracket. The 1700-feet openings of the Forth are spanned by a compound structure consisting of two brackets or cantilevers and one central girder. Owing to the arched form of the under-side of the bridge, many persons hold the mistaken notion that the principle of construction is analogous to that of an arch. In preparing for this lecture the other day, I had to consider how best to make a general audience appreciate the true nature and direction of the stresses on the Forth Bridge, and after consultation with some of our engineers on the spot a living model of the structure was arranged as follows:—Two men sitting on chairs extended their arms and supported the same by grasping sticks butting against the chairs. This represented the two double cantilevers. The central girder was represented by a short stick slung from one arm of each man, and the anchorages by ropes extending from the other arms to a couple of piles of brick. When stresses are brought on this system by a load on the central girder, the men's arms and the anchorage ropes come into tension and the sticks and chair legs into compression. In the Forth Bridge you have to imagine the chairs placed a third of a mile apart and the men's heads to be 360 feet above the ground. Their arms are represented by huge steel lattice members, and the sticks or props by steel tubes 12 feet in diameter and $1\frac{1}{4}$ inch thick.

I have remarked that the principle of the Forth Bridge is not novel. When Lord Napier of Magdala accompanied me over the works one day he said: "I suppose you touch your hat to the Chinese?" and I replied "Certainly," as I knew that a number of bridges on the same principle had existed in China for ages past. Indeed, I have evidence that even savages when bridging in primitive style a stream of more than ordinary width, have been driven to the adoption of the cantilever and central girder system as we were driven to it at the Forth. They would find the two cantilevers in the projecting branches of a couple of trees on opposite sides of the river, and they would lash by grass ropes a central piece to the ends of their cantilevers and so form a bridge. This is no imagination, as I have actual sketches of such bridges taken by exploring parties of engineers on the Canadian Pacific and other railways, and in an old book in the British Museum I found an engraving of a most interesting bridge in Tibet upwards of 100 feet in span, built between two and three centuries ago, and in every respect identical in principle with the Forth Bridge. When I published my first article on the proposed Forth Bridge some four years ago I protested against its being stigmatized as a new and untried type of construction, and claimed that it probably had a longer and more respectable ancestry even than the arch.

The best evidence of approval is imitation, and I am pleased to be able to tell you that since the first publication of the design for the Forth Bridge, practically every big bridge throughout the world has been built on the principle of that design and many others are in progress.

PIERS.—Having referred thus briefly to the general principle of the Forth Bridge, I will now describe more particularly the details of the structure, commencing with the piers.

There are three main piers, known respectively as the Fife pier, the Inch Garvie pier, and the Queensferry pier, and upon each of these there are built huge cantilevers stretching both ways. The Fife pier stands between high and low water mark, and is separated by a span of 1700 feet from the Inch Garvie pier, which is partly founded upon a rocky island in mid-stream. Another span of 1700 feet carries the bridge to the Queensferry pier, which is at the edge of the deep channel. The total length of the viaduct is about $1\frac{1}{2}$ mile, and this includes two spans of 1700 feet, two of 675 feet, being the shoreward ends of the cantilevers, and fifteen of 168 feet. Including piers, there is thus almost exactly one mile covered by the great cantilever-spans, and another half-mile of via-



FORTH BRIDGE.—BUILDING THE GREAT STEEL CANTILEVERS.

duct-approach. The clear headway under the centre of the bridge is 152 feet at high water, and the highest point of the bridge is 360 feet above the same datum.

Each of the main piers includes four columns of masonry founded on the rock or boulder-clay. Above low water the cylindrical piers are of the strongest flat bedded Arbroath stone set in cement and faced with Aberdeen granite. The height of these monoliths is 36 feet, and the diameter 55 feet at bottom and 49 feet at top, and they each contain forty-eight steel bolts $2\frac{1}{2}$ inches in diameter and 24 feet long to hold down the superstructure.

Below low water the piers differ somewhat in character, according to the local conditions. On the Fife side, one of the piers was built with the aid of a half-tide dam, and the other with a full-tide dam. The rock was blasted into steps, diamond drills and other rock-drills being used. Even this comparatively simple work was not executed without considerable trouble, as the sloping rock bottom was covered with a closely-compacted mass of boulders and rubbish, through which the water flowed into the dam in almost unmanageable quantity. After many months' work the water was sufficiently excluded by the use of cement-bags, and liquid grout poured in by divers under water, and other expedients, and the concrete foundation and masonry were proceeded with.

At Inch Garvie, the two northernmost piers were founded like the preceding, but the two others presented greater difficulties, owing to the depth of water, and had to be dealt with in a different way. Several designs were prepared for these foundations, but it was finally decided, and, as experience proved, wisely, to put them in by what is known as the pneumatic or compressed air process. The conditions of the problem were a sloping, very irregular, and fissured rock bottom, in an exposed seaway, and with a depth at high water of 72 feet. Anything of the nature of a water-tight cofferdam, such as was used at the shallow piers, was out of question, and the plan adopted was as follows:—

Two wrought-iron caissons, which might be likened to large tubs or buckets, 70 feet in diameter and 50 to 60 feet high, were built on launching-ways on the sloping southern foreshore of the Forth. The bottom of each caisson was set up 7 feet above the cutting edge, and so constituted a chamber 70 feet in diameter and 7 feet high, capable of being filled at the proper time with compressed air to enable men to work as in a diving-bell below the water of the Forth. The caisson, weighing about 470 tons, was launched, and then taken to a berth alongside the Queensferry jetty, where a certain amount of concrete, brickwork, and staging was added, bringing the weight up to 2640 tons. At Inch Garvie a very strong and costly iron staging had previously been erected, alongside which the caisson was finally moored in correct position for sinking. Whilst the work described was proceeding, divers and labourers were engaged in making a level bed for the caisson to sit on. The 16-feet slope in the rock bottom was levelled up by bags filled with sand or concrete. As soon as the weight of caisson and filling reached 3270 tons, the caisson rested on the sand-bags and floated no more. The high ledge of rock upon which the northern edge of the caisson rested was blasted away, holes being driven, by rock-drills and otherwise, under the cutting edge, and about 6 inches beyond for the charges. After the men had gained a little experience in this work, no difficulty was found in under-cutting the hard whinstone rock to allow the edge of the caisson to sink, and, of course, there was still less difficulty in removing the sand-bags temporarily used to form a level bed. The interior rock was excavated as easily as on dry land, the whole of the 70-feet diameter by 7-feet high chamber being thoroughly lighted by electricity. Access was obtained through a vertical tube with an air-lock at the top, and many visitors ventured to pass through this lock into the lighted chamber below, where the pressure at times was as high as 35 lbs. per square inch. Probably the most astonished visitors were some salmon, who, attracted by the commotion in the water caused by the escape of compressed air under the edge of the caisson, found themselves in the electric lighted chamber. When in the chamber the only notice of this escape of large volumes of air was the sudden pervadence of a dense fog, but outside a huge wave of aërated water would rise above the level of the sea, and a general effect prevail of something terrible going on below. No doubt the salmon thought they had come to a cascade turned upside down, and, following their instinct of heading up it, met their fate.

Another astonished visitor was a gentleman who took a flat-sided spirit-flask with him into the caisson, and emptied it when down below. Of course the bottle was filled with compressed air, which exploded when passing through the air-lock into the normal atmospheric pressure, the pressure in the bottle being 33 lbs. per square inch. The Garvie piers, notwithstanding the novelties involved in sinking through whinstone rock, at a depth of 72 feet below the waves of the Forth, were completed without misadventure, in less than the contract time. The first of the deep Garvie caissons was launched on March 30, 1885, and both piers were finished to sea-level or above by the end of the year.

At Queensferry all four piers were founded on caissons identical in principle with those used for the deep Garvie piers. The deepest was 89 feet below high water, and weighed 20,000 tons; the shallowest of the four was 71 feet high, the diameter in all cases, as at Garvie, being 70 feet at the base. Some differences in detail occurred in these caissons as compared with Garvie, owing to the differences of the conditions. Thus, instead of a sloping surface of rock the bed of the Forth was of soft mud to a considerable depth, through which the caissons had to be sunk into the hard boulder-clay. Double skins were provided for the caissons, between which concrete could be filled in to varying heights if necessary, so that greater weight might be applied to the cutting edge where the mud was hard than soft. This annular wall of concrete also gave great strength to resist the hydrostatic pressure outside the caisson, for it must be understood that the water was excluded both below and above the working chamber.

The process of sinking was as follows:—The caisson being seated on the soft mud, which, of course, practically filled the working chamber, air was blown in, and a few men descended the shaft or tube of access to the working chamber in order to clear away the mud. This was done by diluting it to the necessary extent by water brought down a pipe under pressure, and by blowing it out in this liquid state through another pipe by means of the pressure of air in the chamber. It was found that the mud sealed the caisson so that a pressure of air considerably in excess of that of the water outside could be kept up, and it was unnecessary to vary the pressure according to the height of the tide. In working through this soft mud both intelligence and courage were called for on the part of the men, and it is a pleasure and duty for me to say that the Italians and Belgians engaged on the work were never found wanting in those qualifications. There was always a chance of the caisson sinking suddenly or irregularly, and imprisoning some of the men; and, indeed, on one occasion a few men were buried up to their chins in the mud, and on another the caisson gave a sudden drop of 7 feet. Happily no serious accident happened, although I confess that I felt a little apprehensive myself, as I was familiar with the details of an accident with a similar caisson sunk in the bed of the Neva, at St. Petersburg, in 1876. In that case the wet mud rose rapidly in the working chamber when the caisson sank suddenly 18 inches one day, and of the twenty-eight men in the chamber nine remained imprisoned. Of these, two managed to get their heads into the shaft of access, and were taken out alive after twenty-eight hours, and the remaining seven were smothered in the mud. It was nearly a year before sinking was renewed. Again, in 1877, one of the air-locks suddenly gave way, and of the men then in the chamber, three escaped uninjured, nine were blown out by the rush of air, and, falling into the water and on craft, were mortally injured, whilst twenty were smothered in the caisson. It was thirteen months before the chamber was accessible, and then the vitiated atmosphere in the charnel-house below rendered it very difficult to work. Happily we had no such experiences at the Forth.

With one of our caissons we unfortunately had an accident and loss of life, which, although it had nothing to do with the sinking of the caisson, as in the Neva Bridge, was indirectly due to the same cause, viz. the softness of the mud bottom. On New Year's Day, 1885, the south-west Queensferry caisson, which had been towed into position, and weighted with about 4000 tons of concrete, stuck in the mud, and, instead of rising with the tide, remained fixed so that the water flowing over the edge filled the interior. The 4000 tons of water caused the caisson to sink further in the mud, especially at the outer edge, and to slide forward and tilt. The contractors determined to raise the skin of the caisson until it came above water-level, and then pump out and float the caisson back into position. About three months were occupied in doing this, but when pumping had proceeded a certain extent the caisson collapsed, owing to the heavy external pressure of the water, and two men were killed. It was necessary then to consider very carefully what had better be done, as the torn caisson was difficult to deal with. Finally it was determined to case it in "tubbing" of whole balks of timber strutted with ring girders and rakers This was a very tedious work, as every balk had to be fitted water-tight to its neighbours by divers. Finally, on October 19, 1885, or between nine and ten months after the first accident, the caisson, to the relief of everyone, was floated into position and the sinking proceeded without further difficulty, this, the last of the main piers, being completed in March 1886, or almost exactly two years after the first caisson was floated out. No doubt some of my hearers have passed through air-locks and experienced the physiological effects of compressed air, one of the first of which is a painful pressure on the drums of the ears. It is necessary to restrict the hours of work, and even then most men suffer more or less inconvenience. Pains in the limbs are generally relieved by galvanism; a long continuance often leads to paralysis if the depth is great. At the St. Louis Bridge in America, for example, out of 600 workmen who worked in the compressed air, 119 were attacked, 16 died, and 2 were crippled. We had no deaths directly attributable to the air-pressure. Personally I felt no inconvenience whatever. Photographs were taken in the caisson, a total lighting power of 6000 candles and an exposure of as much as 15 minutes in some cases being given. Owing to the fog formed when the air blew under the edge the results were not so good as could be wished, the eyes especially coming out in glaring spectral fashion.

SUPERSTRUCTURE.—I must now say a few words respecting the design, manufacture, and erection of the superstructure.

Design.—I have already illustrated the principle of the cantilever bridge, and need only deal with the details. At the Forth, owing to the unprecedented span and the weight of the structure itself, the dead load is far in excess of any number of railway trains which could be brought upon it. Thus the weight of one of the 1700-feet spans is about 16,000 tons, and the heaviest rolling load would not be more than a couple of coal trains weighing say 800 tons together, or only 5 per cent. of the dead weight. It is hardly necessary therefore to say that the bridge will be as stiff as a rock under the passage of a train. Wind, even, is a more important element than train weight, as with the assumed pressure of 56 lbs. per square foot the estimated lateral pressure on each 1700-feet span is 2000 tons, or two and a half times as much as the rolling load. To resist wind the structure is "straddle-legged," that is, the lofty columns over the piers are 120 feet apart at the base and 33 feet at the top. Similarly, the cantilever bottom members widen out at the piers. All of the main compression members are tubes, because that is the form which with the least weight gives the greatest strength. The tube of the cantilever is, at the piers, 12 feet in diameter and 1¼ inch thick, and it is subject to an end pressure of 2282 tons from the dead load, 1022 tons from the trains, and 2920 tons from the wind; total, 6224 tons, which is the weight of one of the largest Transatlantic steamers with all her cargo on board. The vertical tube is 343 feet high, 12 feet in diameter, and about ⅝ inch thick, and is liable to a load of 3279 tons. The tension members are of lattice construction, and the heaviest-stressed one is subject to a pull of 3794 tons. All of the structure is thoroughly braced together by "wind bracing" of lattice girders, so that a hurricane or cyclone storm may blow in any direction up or down the Forth without affecting the stability of the bridge. Indeed, even if a hurricane were blowing up one side of the Forth and down the other, tending to rotate the cantilevers on the piers, the bridge has the strength to resist such a contingency. We have had wind-gauges on Inch Garvie since the commencement of the works, and know, therefore, the character of the storms the bridge will encounter. The two heaviest gales were on December 12, 1883, and January 26, 1884. On the latter occasion much damage was done throughout the country. At Inch Garvie the small fixed gauge was reported to have registered 65 lbs. per square foot, but I found on inspection that the pointer could not travel further, or it might have indicated even higher. I did not believe this result, and attributed it to the joint action of the momentum of the instrument, and a high local pressure of wind too instantaneous in duration to take effect upon a structure of any size or weight. The great board of 300 square feet area on the same occasion indicated only 35 lbs. per square foot, and I doubt much if the pressure would have averaged more than 20 lbs. on so large a surface as the bridge.

Manufacture.—The bent plates required for the tubes of the Forth Bridge would, if placed end to end, stretch 42 miles. Special plant had to be devised for preparing these plates. Long furnaces, heated in some instances by gas-producers, and in others by coal, first heated the plates, which were then hauled between the dies of an 800-ton hydraulic press, and bent to the proper radius. When cool, the edges were planed all round, and the plates built up into the form of a tube in the drilling-yard. Here they were dealt with by eight great travelling machines, having ten traversing drills radiating to the centre of the tube, and drilling through as much as 4 inches of solid steel in places. A length of 8 feet was drilled in a day of twenty-four hours. When complete, the tubes were taken down, the plates cleaned and oiled, and stacked ready for erection.

The tension members and lattice girders generally are of angle bars, sawn to length when cold, and of plates planed all round. Multiple drills tear through immense thickness of steel at an astonishing rate. The larger machines have ten drills, which, going as they do, day and night, at 180 revolutions per minute, perform work equivalent to boring an inch hole through 280 feet thickness of solid steel every twenty-four hours. About 4 per cent. of the whole weight of steel delivered at the works leaves it again in the form of shavings from planing-machines and drills. The material used throughout is Siemens's steel of the finest quality, made at the Steel

Company's Works in Glasgow, and at Landore in South Wales. Although one and a half times stronger than wrought iron, it is not in any sense of the word brittle, as steel is often popularly supposed to be, but it is tough and ductile as copper. You can fold half-inch plates like newspapers, and tie rivet-bars like twine into knots. The steel shavings planed off form such long, true, and flexible spirals, that they are largely used for ladies' bracelets when fitted with clasps and electro-plated.

Erection.—Facility of erection is one of the most important desiderata in the case of the Forth Bridge. Owing to the 200 feet depth of water, scaffolding is impossible, and the bridge has to constitute its own scaffolding. The principle of erection adopted was, therefore, to build first the portion of the superstructure over the main piers, the great steel towers, as they may be called, although really parts of the cantilever, and to add successive bays of the cantilever right and left of these towers, and therefore balancing each other, until the whole is complete. This being the general principle, a great deal yet remained to be done in settling the details. What was finally settled, and is now in progress, is as follows:—

After the skewbacks, horizontal tubes, and a certain length of the verticals as high as steam-cranes could conveniently reach were built, a lifting-stage was erected. This consisted of two platforms, one on either side of the bridge, and four hydraulic lifting-rams, one in each 12-feet tube. To carry these rams cross-girders were fitted in the tubes capable of being raised so as to support the rams and platform as erection proceeded, and steel pins were slipped in to hold the cross-girders. Travelling cranes are placed on the platforms, and these cranes, with the men working aloft, are of course raised with the platforms when hydraulic pressure is let into the rams. The mode of procedure is to raise the platform 1 foot, and slip in the steel pins to carry the load whilst the rams are getting ready to make another stroke of 1 foot. When a 16-feet lift has been so made, which is a matter of a few hours, a pause of some two or three days occurs to allow the riveting to be completed. The advance at times has been at the rate of three lifts, or 48 feet in height, in a week.

The riveting appliances designed by Mr. Arrol are of a very special and even formidable character, each machine weighing about 16 tons. It consists essentially of an inside and outside hydraulic ram mounted on longitudinal and annular girders in such a manner as to command every rivet in the tubes, and to close the same by hydraulic pressure. Pipes from the hydraulic pumps are carried up inside the tubes to the riveters, and oil furnaces for heating the rivets are placed in convenient spots, also inside the tubes. By practice, and the stimulus of premiums, the men have succeeded in putting in 800 rivets per day with one of the machines, at a height of 300 feet above the sea, which, in fact, is more than they accomplished when working at ground level. Indeed, by the system of erection adopted, the element of height is practically annihilated, and with ordinary caution the men are safer aloft than below, as in the former case they are not liable to have things dropped on their heads.

The cantilever will be erected and riveted in precisely the same manner as the great towers, but owing to the overhang temporary ties will occasionally be required. The centre girder itself will be similarly erected, one half being temporarily added on to the extremity of each cantilever, and when the two ends meet at the centre of a 1700-feet span they will be connected, and the temporary joints, with the cantilevers, released. Roller joints are provided at the cantilever ends for expansion, and at the main piers the whole superstructure rests on lubricated sliding bed-plates.

The system of erection by overhanging offers great advantages as regards safety, as each successive part of the superstructure is riveted up and completed before a further portion is added. In the case of an ordinary bridge the whole superstructure must first be temporarily bolted up on scaffolding, and in that condition is liable to be swept away by flood or hurricane at any moment.

There is nothing new under the sun, and therefore you will not be surprised to hear that in 1810, a certain Mr. Pope proposed to construct a cantilever bridge, of 1800 feet span, across the East River, in New York, and, indeed, exhibited a 50-feet model of the same.

I have described the process of erecting the Forth Bridge in sober prose; if I had thought of doing it in verse I should have appropriated bodily Mr. Pope's lyrical version of his intended operations at the East River, of which the following is a sample:—

> "Each semi-arc is built from off the top,
> Without the aid of scaffold, pier, or prop;
> By skids and cranes each part is lowered down,
> And on the timber's end grain rests so sound.
> Sure all the bridges that were ever built,
> Reposed their weight on centre, pier, or stilt;
> Not so the bridge the author has to boast,
> His plan is sure to save such needless cost;
> A ladder on each side is lowered down,
> And shifted from the fulcrum to the crown."

To carry out the work at the Forth Bridge there is an army of 3500 workmen, officered by a proportionate number of engineers. Everything, except the rolling of the steel plates, is done on the spot, and consequently there are literally hundreds of steam and hydraulic engines and other machines and appliances too numerous to mention, many of them being of an entirely original character.

It is, of course, impossible to carry out a gigantic work of the kind I have had the honour of bringing before the Institution without paying for it, not merely in money, but in men's lives. I shall have failed in my task if you do not, to some extent, realize the risks to which zealous and plucky workmen will be sure to expose themselves in pushing on with the work of erecting the Forth Bridge. Speaking on behalf of the engineers, I may say that we never ask a workman to do a thing which we are not prepared to do ourselves, but of course men will, on their own initiative, occasionally do rash things. Thus, not long ago a man trusted himself at a great height to the simple grasp of a rope, and his hand getting numbed with cold he unconsciously relaxed his hold and fell backwards, a descent of 120 feet, happily into the water, from which he was fished out little the worse after sinking twice. Another man going up in a hoist the other day, having that familiarity with danger which breeds contempt, did not trouble to close the rail, and, stumbling backward, fell a distance of 180 feet, carrying away a dozen rungs of a ladder with which he came in contact, as if they had been straws. These are instances of rashness, but the best men run risks from their fellow-workmen. Thus a splendid fellow, active as a cat, who would run hand over hand along a rope at any height, was knocked over by a man dropping a wedge on him from above, and killed by a fall of between one and two hundred feet. There are about 500 men at work at each main pier, and something is always dropping from aloft. I saw a hole 1 inch in diameter made through the 4-inch timber of the staging by a spanner which fell about 300 feet, and took off a man's cap in its course. On another occasion a dropped spanner entered a man's waistcoat and came out at his ankle, tearing open the whole of his clothes, but not injuring the man himself in any way.

Happily, there is no lack of pluck amongst British workmen; if one man falls another steps into his place. Difficulties and accidents necessarily occur, but like a disciplined regiment in action we close up the ranks, push on, and step by step we intend to carry on the work to a victorious conclusion.

UPPER WIND-CURRENTS NEAR THE EQUATOR, AND THE DIFFUSION OF KRAKATÃO DUST.

THE crude idea that the trade-winds on either side of the equator met in the doldrums, and that then the air rising upwards flowed backwards as a return south-west current in the northern hemisphere, and as a north-west current in the southern hemisphere, has been modified by the more modern discoveries that the atmosphere is not composed of horizontal layers of air moving in different directions, but that, as a rule, there is a regular continuous successive veering of the wind as we ascend. It is not usual to find a southerly wind on the surface, and for some height above, and then abruptly a westerly current also of a certain thickness; for cloud-observations show that over a surface south wind the upper currents may be from south-by-west at quite a low level, from south-south-west a little higher up, then successively from south-west and west-south-west as we ascend, and perhaps from west at the altitude of the highest cirrus. We must, in fact, look upon the atmosphere as circulating in the form of a continuous complex screw.

Innumerable observations show that, as a rule, there is a very definite law of the vertical succession of the upper currents. Stand with your back to the surface wind, and the upper currents will come successively more and more from your left hand the higher they are. The rule is reversed in the southern hemisphere, for there the upper currents come successively more and more from the right. For instance, with a southerly wind in London, the clouds will come more and more from the west the higher they are, while in Australia they would come more and more from the east.

But during my two meteorological voyages round the world I have discovered some very remarkable exceptions to this law in that interesting part of the world, meteorologically, that lies between the equator and the doldrums. These have so important a bearing, not only on the whole problem of the circulation of the atmosphere from the equator to the Pole, but also on the remarkable diffusion of dust from the volcano of Krakatão, that I propose to give a short account of these researches in this article.

In the Atlantic, the doldrums lie north of the equator at all seasons of the year. Between 20° and 30° W. longitude they range from about 3° N.; in winter, to 11° N. in summer time. In the Gulf of Guinea, the position of the doldrums cannot be accurately defined, but they probably range from about 5° to 10° N. of the equator. There is, however, a striking difference in the direction of the winds between the equator and the doldrums in the eastern and western portions of the Atlantic. In the Gulf of Guinea, the south-east trade turns to south-west and forms a south-west monsoon; while west of about 15° W. longitude the south-east trade remains from nearly in the same direction as before it crossed the line.

In the Indian Ocean, the position of the doldrums varies enormously at different seasons. From about November to March the doldrums are 5° to 10° S. of the line, and the north-east monsoon draws into the well-known north-west monsoon as it crosses the equator. From April till September the doldrums must be somewhere to the north of the line; but, contrary to the opinion of Dove and others, the Indian meteorologists now believe that the south-west monsoon is not linked up regularly with the south-east trade. As the evidence for this belief is not yet published, we can say nothing about it, though we shall have to refer to the point later on.

The words rotation, circulation, and veering of wind are unfortunately used so vaguely by different writers that it may be well to define them more precisely before we discuss the general circulation of the equator.

"Rotation" should be confined exclusively to the manner in which the surface wind blows round areas of high or low pressure. For instance, we may say that the wind rotates counter-clockwise round a cyclone in the northern hemisphere.

"Circulation" should be applied to the general movement of a whole mass of air extending over a certain breadth and height. For instance, the air in a cyclone rotates round and in below, round and out at high levels; and the whole system makes up the general circulation of a cyclone. Or, again, the whole motion of the atmosphere into the doldrums, then upwards and more or less polewards, should be called the general circulation in the vicinity of the equator.

"Sequence" of wind should be applied to the changes in the direction of the wind which take place during any interval of time as a cyclone passes over a station. This veering or backing of the wind has unfortunately been too often called the rotation of the wind. The confusion here, as often, arises from sometimes talking of the motions of a mass of air extending over a large area at a given moment, and sometimes of the successive motions of the air at a single station during a certain interval of time. Some writers make matters worse by not only drawing the plan of a cyclone on a chart, but also by putting in small circles at different stations to show how the wind would go round as the cyclone drifted past. The confusion is direful.

"Vertical succession" should be used to denote the gradual successive directions of the upper currents, as has been already explained.

The two sections of weather across the Atlantic which I made were taken, the one in July, between Rio Janeiro and Teneriffe; the other in December, between Teneriffe and Capetown. A short account of the observations were published in NATURE, vol. xxxiii. p. 294, so that the general results only need be stated here.

Between Rio and Teneriffe, while south of the line, the low or middle clouds over the south-east trade invariably came from some point to the right of the surface when you stood with your back to the wind; *i.e.* if the surface wind was south-east the low clouds would drive from about east-south-east. This is the usual vertical succession of the southern hemisphere

But north of the line, when, for reasons which cannot be discussed here, the south-east trade did not turn into south-west, as might have been expected, the upper currents continued to follow the succession of the southern hemisphere, instead of conforming to the law of the northern hemisphere. In the doldrums, which extended from about 8° to 13° N., the same rule obtained, and the middle layer of cloud over some "cats' paws" of south-east wind drove from the east. To show how difficult it is to get cirrus observations in the tropics, I may mention that this was the highest layer I was able to record during this part of the voyage.

In the north-east trade I only got one unsatisfactory observation in 22° N., 19° W., which gave a middle layer of north-north-east wind over an east-north-east surface trade. This is the contrary of what might have been expected.

In the second section, between Teneriffe and Capetown, the lower layers of cloud in the north-east trade—from 30° N. to the doldrums in 5° N.—invariably came from some point to the left of the surface wind, generally from south or south-by-west. This is the usual vertical succession of the northern hemisphere.

But as we entered the doldrums, in 5° N., a totally different wind-system became apparent. Over the oily calm of that district I could just detect, through the universal haze and gloom, a middle current from the east; and when a few hours later we picked up the south-west monsoon of the Gulf of Guinea—here coming from south-by-west—the low clouds came from south-east. This continued till we reached the line, and the single observation that I got of high cirrus in 1° N. lat. showed an easterly current at that level. The above results are very curious, for the usual idea is that the south-east trade on crossing the equator becomes a south-west wind under the influence of the earth's rotation; but here, though the surface wind appears to conform to the usual rotation of the air round a low pressure, the upper currents maintain the vertical succession of the southern hemisphere.

After crossing the equator ourselves, the wind turned to south-by-east, or south-south-east; and as far as 18° S., beyond which we need not follow them, the lower currents were either identical in direction with the surface winds, or else a very little more easterly—that is, they followed the normal vertical succession of the southern hemisphere.

I have endeavoured to show all this in a diagrammatic form in Fig. 1, where the full arrows denote the surface wind; the broken-lined arrows the direction of the low or middle-level clouds; the dotted arrows that of the highest cirrus. A couple of arrows to denote the typical succession over an extra-tropical south-west wind are inserted on the upper left-hand corner of the figure.

The observations in the Indian Ocean have also been published in NATURE, vol xxxii. p. 624, and vol. xxxiii. p. 460. They were taken during two trips—one from Aden to Cape Lewin, in February, the other from Cape Lewin to Colombo, in the same month of the following year.

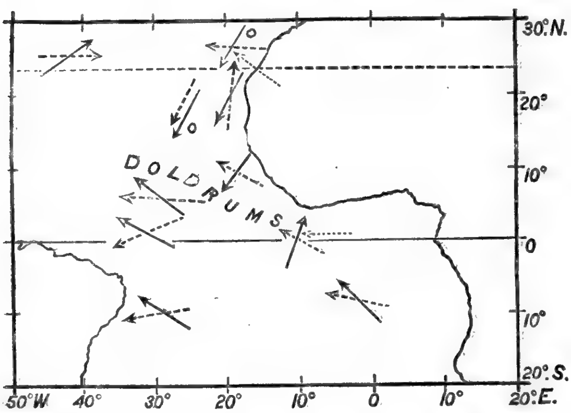


FIG. 1.—Surface and upper currents in the Atlantic.

This is the season of the north-west monsoon. The results were very accordant, and showed conclusively that in vertical succession the upper currents over the north-east monsoon were always more from the east than the surface wind, as is usual in the northern hemisphere. No high cirrus was ever observed in this part of the ocean.

In the north-west monsoon, between the equator and the doldrums, low and middle currents came from north or north-east, and the only two observations on the highest cirrus showed an easterly current at that level.

In the south-east trade, the low or middle currents were generally about from the same direction as the surface wind, or else a little more from the east. This is the normal succession of the hemisphere. Thus we see an extraordinary analogy to what occurs in the Atlantic. When the north-east monsoon is drawn across the equator, towards the low pressure of the doldrums, the surface wind seems to pick up the westerly component of the earth's rotation in the usual manner; but the upper currents retain the vertical succession of the northern hemisphere.

The results of all the observations are given in a diagrammatic form in Fig. 2. It will be remarked how few observations there are of cirrus; this is because it is so rarely seen in those latitudes. It is certain that, on the southern and western edges of the great anticyclone which controls the south-east trade, the highest currents would have come more or less from the north-west, as is marked at Mauritius.

I have unfortunately got no equatorial observations from the Pacific, but they would most likely be very similar to those in the Atlantic.

However, the upshot of all those which we have just described is to show that when the trades or monsoons meet they do not interlace, as has been suggested by Maury, but that the upper winds combine in a generally easterly current, and probably diverge only slightly polewards on either side. I am unable to form any opinion as to the velocity of this current.

I have often been asked how far all this bears on the remarkable distribution of dust from Krakatão, and the answer is very simple. The great explosion of Krakatão took place on August 26; that is to say, during the south-west moonsoon of the Indian Ocean. The distribution of the surface and upper currents is then very different from that in our last diagram. The south-east trade, with its upper easterly currents, extends all over the Indian Ocean, and Malaysia south of the line, including Krakatão, marked K in the figure. North of the equator the surface wind turns to the south-west, but we know nothing of the motion of the upper currents.

Now, assuming that the blue sun and unusual twilight

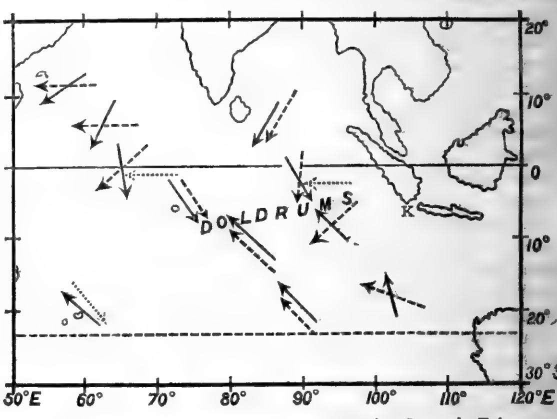


FIG. 2.—Surface and upper currents in the Indian Ocean in February.

glows were equally the product of volcanic dust as the fall of actual ashes, the sequence of the first appearance of these phenomena in different parts of the world, dating from August 26, was as follows:—

On August 26, the day of the eruption, ashes, lofty haze, or red twilights are reported south of the equator, nearly 20° of longitude west of Java, from one station in Sumatra, just north of the line, and from near Formosa.

Next day, the 27th, similar phenomena were reported from many stations in the Indian Ocean, south of the equator, as far west as Mauritius and the Seychelles; while north of the line, strange appearances were reported for the first time in Borneo and Ceylon.

On the 28th, the haze and abnormal glows had extended to Natal on one side and Japan on the other.

No important extension of the area is reported on August 29; but by the 30th unusual coloration of the sky is reported from various parts of the South Atlantic, and Guiana, and, what is specially noticeable, from about the Cape Verd Islands, north of the equator.

On the 31st, another station in Brazil, and also a West Indian island, report a strange look about the sun or sky; while on September 1 the same was noted at Guayaquil, on the west coast of South America; and in a quite unexpected locality, far away from there,—New Ireland.

September 2 was characterized by an outburst of coloured suns all over the northern provinces of South America; while between the 3rd and 4th of the same month the glows extended across the Pacific both north and south as far as the Society and Gilbert Islands, and were reported from two stations in New Britain.

By September 5 the Sandwich Islands were reached; while the outburst of glows in Southern India did not commence till about the 6th to 8th of the same month.

The northward extension of the dust all this time was very small, and not widespread. Isolated phenomena are reported from Formosa on August 26, and from Japan on the 28th, but I am unable to say whether the glows which appeared in the Sandwich Islands on September 5, had come *viâ* Japan, or across South America.

Thus the general system of the dust-flow appears to have been very simple. The great dust-stream was carried for the first twenty-four hours by the normal easterly upper currents over the south-east trade, at the extraordinary rate of more than 120 miles an hour, but hardly extended north of the line. Three days after the eruption we find the products of Krakatão in Guiana, the South Atlantic and also north of the line in the Cape Verd Islands. Just to the south of the latter we know that the south-east trade with its attendant upper currents crosses the equator. Then all the north of South America was invaded; and six or seven days after the first outburst, the Pacific Islands—south of, or on the line—were also overshadowed.

In fact we may say that the great stream of Krakatão dust was carried nearly round the world by the usual upper winds of the south-east trade, in which the dust was first ejected, at a rate of about 120 miles an hour, and that the dust spread very slowly either north or south of the main current.

There is one inference from this which is very important in any theory of the general circulation of the atmosphere from the equator to the Poles. The main body of the equatorial circulation is in an easterly direction, so that the whole mass of air going towards the doldrums does not rise up and flow backwards on itself directly towards the Pole; and though the highest currents over the Polar limit of both the south-east and north-east trades are from north-west and south-east respectively, still the poleward motion near the equator is very small.

The high velocity of 120 miles an hour is certainly more than would have been expected; but we have very few observations on the rate of motion of the highest clouds. Hildebrandsson has, however, reported from Upsala one velocity of about 112 miles per hour for a cirrus at 28,000 feet (50 metres per second at 8559 metres); and several velocities ranging between that figure and 90 miles per hour.

There would be nothing, then, outrageous in the assumption of a velocity of 120 miles an hour for the easterly current over the equator to account for the high speed of the diffusion of Krakatão dust; and it is also satisfactory to know that the general direction of the flow is in accordance with the most recent researches on the vertical succession of the upper currents near the equator.

RALPH ABERCROMBY.

BERNARD STUDER.

AMONG the magnates of Swiss geology, no name has held a more honoured place than that of Bernard Studer, who now at the ripe age of ninety-three years has passed away. Upwards of sixty years ago he began his scientific career by the study of some of the geological problems presented by the rocks of his native country. From the molasse of the lower grounds he soon climbed into the higher Alps, and distinguished himself as one of the foremost pioneers who grappled with the intricate problems in stratigraphy which these mountains present. With patient toil he extended year by year his acquaintance with the various portions of the chain, publishing from time to time notices of his labours, and preparing materials for a geological map of the whole region. In association with A. Escher von der Linth he pursued these labours until the two fellow-workers were enabled to give to the world their great map of Switzerland, which, though only an outline of the geology of the Alps, will remain as an enduring monument of the geological prowess of its authors. No one who has not climbed the mountains with that map in hand can form any adequate conception of the physical labour, mental exertion, and happy geological intuition which its preparation required.

Studer's contributions to the glacial geology of the Alps brought him into intimate personal relations with many English geologists. All who passed through Berne tried to see the venerable Professor, who retained, in spite of his weight of years, his keen interest in the progress of his favourite science. His papers, published in various scientific journals, make a long list. But he was also the author of some separate works. Besides the great map of Switzerland, he published several volumes on Swiss geology, the most important of which was his "Geologie der Schweiz," which appeared in two volumes in 1851-53. Less known perhaps, but full of suggestive matter, is his "Lehrbuch der physikalischen Geographie und Geologie," which was issued as far back as 1844. This work was one of the earliest in which the processes of physical geography were discussed from the geological side, and showed how wide and thoughtful had been the observations of the author, especially among the phenomena to be witnessed in Switzerland. Another of the old lights of geology has been extinguished by the death of Bernard Studer, whose kindly presence and helpful courtesy will be affectionately remembered by everyone who has been fortunate enough to come in contact with him.

NOTES.

AN important Bill dealing with the question of technical education has been introduced into the House of Commons by Sir Henry Roscoe. The Bill empowers any School Board, local authority, or managers of a public elementary school, to provide day technical and commercial schools and classes for the purpose of giving instruction in any of certain subjects. These include the several science subjects which are specified in the Directory of the Science and Art Department, and in which that Department undertakes to examine. The following subjects are also included: the use of ordinary tools, commercial arithmetic, commercial geography, book-keeping, French, German, and other foreign languages, and freehand and machine drawing. The addition of other subjects may be sanctioned from time to time by the Committee of Council on Education or by the Science and Art Department. For the purpose of carrying on these schools and classes, the power of School Boards, other local authorities, and school managers is to be in every respect the same as for providing ordinary elementary schools. They are to have power to provide, or contribute to the maintenance of, laboratories and workshops in endowed schools for the purpose of carrying on classes or instruction under the Bill. All these schools and classes are to be subject to the inspection of the officers of the Committee of Education or of the Science and Art Department. Before a scholar is admitted he must have passed the Sixth Standard or some equivalent examination. The Education Committee and the Science and Art Department are authorized to give grants on such conditions as they may lay down for any of the subjects taught. For the purpose of obtaining grants a technical school or class must be one carried on under minutes to be made by the

Science and Art Department, and laid on the table of the House of Commons in the same way as the minutes that regulate the grants of the Education Department.

ON Tuesday afternoon the new Science and Art Schools which have been erected (at a cost of £17,000) in connexion with Sir Andrew Judde's School, Tunbridge, were formally opened by the Lord Mayor of London. Among those who addressed the company present at the ceremony was Prof. Judd, who referred to Mr. John Morley's idea that science is likely to retaliate on literature for the subservient position it has been so long compelled to occupy. All who had any authority to speak on behalf of science, said Prof. Judd, fully recognized the value of a true literary training, which taught men how best to express the thoughts arising from the trained mind, and the store of facts which science supplied. People must recognize, however, that the time had come when science must take her proper place in the education of the country; for if they did not, other nations would, to the destruction of all our pre-eminence.

ANOTHER serious loss has been inflicted on the cause of geology in this country by the death of Mr. Champernowne, of Dartington Hall, Totnes. Representative of an old Devonshire family, and living in the ancestral home, he was a country gentleman of the best type. At the same time, he devoted himself with singular ardour to geology and especially to the study of the complicated structure of the Devonian rocks of his own district. He had with his own hand mapped the subdivisions of these rocks in far ampler detail than had ever before been attempted, and with a skill and success worthy of the best-trained professional geologist. He lately generously presented his maps to the Geological Survey, with a view to the projected publication of a new edition of the Survey maps of the district. It was while engaged in revising some of his lines for this purpose that he aggravated a previous cold, and brought on congestion of the lungs, which carried him off after an illness of only a few days. His geological ardour was fully equalled by his courtesy and kindness, which endeared him to a wide circle of friends who mourn his early death.

PROF. VULPIAN, the eminent French physiologist, died of pneumonia on the 18th inst. He was sixty-one years of age. For some time he was Assistant Professor in the Museum of Natural History in Paris, but afterwards he was made Professor of Pathological Anatomy in the Paris Medical School. He was appointed Member of the Academy of Sciences in 1876, and Perpetual Secretary in 1886. His principal works relate to the vasomotor system, the diseases of the nervous system, the digestive process, and the physiological action of curare, strychnine, and some other drugs. Before the short illness of which he died, he was giving lectures on the respiratory function; and he was about to publish an important book on the cerebral functions. He was a most conscientious investigator, and his death is greatly regretted by French men of science. The funeral took place on Saturday last, and was attended by a large crowd of friends and students.

THE anniversary meeting of the Royal Geographical Society was held on Monday, when General R. Strachey, who has been elected President, delivered the annual address. After paying a high tribute to his predecessor, Lord Aberdare, and noticing the losses of the Society by death during the past year, General Strachey went on to speak on the subject of geographical education. He then reviewed the chief geographical events of the year, and traced the progress that has been made in geographical knowledge since the beginning of the Queen's reign.

AT the last meeting of the Council of University College, Liverpool, Mr. R. J. Harvey Gibson was appointed to the Lectureship of Botany, vacant by the resignation of Dr. Shearer.

MR. EDGAR M. CROOKSHANK, M.B. (Lond.), has been appointed Lecturer on Bacteriology by the Council of King's College, London.

RATHER more than a year ago (vol. xxxiii. p. 361) we reviewed a "Manual of Bacteriology," by Mr. Edgar M. Crookshank. A new edition has now been issued, and the author has increased the value of the work not only by revising it throughout and bringing it up to date, but by recasting the systematic part. He has written new chapters on the general morphology and physiology of Bacteria, on antiseptics and disinfectants, and on immunity; and seventy-three illustrations have been added. He also gives a useful list of references to works on bacteriology.

IN the Annual Report of the Belfast Naturalists' Field Club for the year ended March 31, 1870, it was stated that the Committee considered it advisable that the Club should prepare complete lists of the fauna, flora, geology, and archæology of Ulster. This purpose has been kept steadily in view, and twenty-one separate papers have been issued, illustrated by twenty-seven plates. These papers have now been brought together in what is intended to be the first of a series of volumes. The volume is entitled "Systematic Lists illustrative of the Flora, Fauna, Palæontology, and Archæology of the North of Ireland," and contains the results of much careful and conscientious work. Among the papers is a very useful one by Mr. William Gray on the cromlechs of Antrim and Down. The list is complete, and each of the monuments has been examined by the author, and, as far as possible, measured and sketched. Mr. Charles Elcock contributes notes on the prehistoric monuments at Carrowmore, near Sligo. Both of these papers are accompanied by good illustrations.

A GEOGRAPHY of the Malay Peninsula, Indo-China, the Eastern Archipelago, the Philippines, and New Guinea, by Prof. A. H. Keane, has just been published by Mr. Edward Stanford. The author's primary aim has been to produce a work which may meet the requirements of teacher and pupil in the Straits Settlements, and in the other colonies directly interested in the regions dealt with. The book ought, however, to be of considerable service to students at home. Prof. Keane knows his subject thoroughly, and his treatment of it is in accordance with the methods of the highest authorities on geographical science.

THE Deutsche Seewarte and the Danish Meteorological Institute have just issued the first quarter of a fresh series of daily synoptic charts, commencing with December 1883. The charts show the conditions of weather over the North Atlantic and a part of the adjacent continents, on each morning, from the data collected by both institutions. The period now embraced by the charts is from September 1873 to September 1876, and from December 1880 to February 1884—excepting from September 1882 to August 1883—being part of the thirteen months selected by the Meteorological Council for their synchronous charts; and the whole work forms a valuable contribution to our knowledge of the causes of the weather changes which generally affect this country. A work of a similar nature is being carried out for the Indian Ocean by Dr. Meldrum, of the Mauritius, for the year 1861, but only the charts for the first three months have yet appeared.

THE *Annalen der Hydrographie und maritimen Meteorologie* of Berlin for April last contains the results of observations taken on board the German warship *Habicht* during her stay at Cameroon from April 1885 to September 1886. The maximum temperature in the harbour was 88°·0 in May 1885, and the minimum 71°·2 in June 1885. The amount of rainfall is not stated, but the number of wet days was most frequent between

April and October; the wet season began in June and ended in October. Lightning was observed almost daily, accompanied at times by heavy thunderstorms. Tornadoes were most frequent in June, October, and November. They lasted about fifteen to twenty minutes, and were generally preceded by a threatening bank of clouds rising over the land, accompanied by a heavy downpour lasting for several hours. The tides were irregular; for instance, there was scarcely any tidal current in the harbour in the beginning of October 1886, but only a slight rise of the water, so that often the ship did not swing.

ON the night of March 15, at about 8 p.m., a most remarkable display of aurora borealis was observed at Throndhjem, in Norway. It first appeared in the east in the form of a streamer, which suddenly flashed upwards to the zenith, and joined another one shooting up from the west; a perfect and symmetrical arc being thus formed right across the sky, one of the bases of which rested on the horizon at the ridge Stenobjerget, and the other at the fortress of Christiansten. The arc emitted a steady white brilliant light, in the centre of which a nucleus of light appeared, more brilliant still. After remaining perfectly stationary and without any undulating motion along it for about a quarter of an hour, the arc moved slowly a little to the northwards, parallel with its former position. The light then began to be diffused, but the aurora remained in the sky till nearly 10 o'clock, although with less intensity.

DR. M. A. VEEDER writes to us from Lyons, New York, that an aurora was seen there on Saturday evening, April 23. Streamers were numerous and active from 10 to 10.30 p.m. There were magnetic perturbations of marked extent at intervals on Saturday and on Sunday. These perturbations became very decided during the evening of Sunday, when there was again an appearance of the aurora, which was, however, very faint.

Science says that the U.S. Geological Survey proposes to collect all attainable information regarding the recent earthquakes in Arizona. Circular letters of inquiry will be sent to residents on the area affected. The disturbed area seems to be a circle of some 400 miles radius, fully one-quarter as large as the Charleston earthquake, and nearly one-third of the area of the Riviera earthquake of last February.

A PAPER by Prof. Milne, containing an interesting account of a series of observations made upon earthquakes in Tokio between March 1884 and March 1885, has lately been printed. Prof. Milne arrives at the practical conclusion that there are at least three ways in which an ordinary building may be to a considerable extent protected from earthquake motion. The first method in a given district is, he says, to make a seismic survey of that area, and then select the locality where the least motion is experienced. A second method is to rest the building at each of its piers upon layers of cast-iron shot. A third method, which is applicable to heavy structures of stone or brick, is to allow them to rest upon foundations on hard ground rising from a deep pit or series of trenches.

AT a meeting of the Council of the National Fish-Culture Association, on Thursday last, a resolution approving of the proposed North Sea Fisheries Institute was unanimously carried. At the same meeting the Secretary reported that a large quantity of trout fry had been presented to various public waters in London and the provinces by the Association. The rest of the fish hatched out this season had been deposited in ponds at the Delaford Park establishment, the aim of the Association being to raise fish entirely from the ova taken from stock bred therein, not from eggs obtained from outside sources. He further stated that the rainbow-trout of California, bred by the Association, had all been maliciously poisoned by arsenic. A severe loss had thus been sustained, the fish being very valuable.

ANOTHER establishment for fish-culture has just been formed at Dulverton, in Somersetshire, by Mr. Frank Langdon. A series of ponds has been made near a tributary of the Exe, which affords an ample supply of pure water. Trout culture will constitute the chief business of the establishment, and a hatchery is being erected to receive the ova, which will be laid down for incubation at the end of the present year.

A *soirée* will be given at University College by the University College Biological Society on Monday, June 6, when Prof. Moseley, F.R.S., will lecture on "Life on the Ocean Surface." Cards of invitation will be sent on application to the Secretary, Mr. Bruce G. Seton.

M. E. QUINQUAUD has been investigating the influence of baths on the chemical phenomena of respiration and nutrition. He finds, by experiments on dogs, that cold baths increase the consumption of oxygen, the consumption being on the average ten times more abundant after the bath than before. Very hot baths exert a like influence, but in a less marked manner. Cold baths (and hot as well, but in a less degree) increase pulmonary ventilation: the quantity of air passed through the lungs is double or treble after the bath. At the same time a greater quantity of carbonic acid is expelled. By the analysis of arterial and venous blood it is shown that the respiratory combustions are very much increased under the influence of cold or hot baths, and it is also shown that the production of blood sugar is greater.

DR. T. MITCHELL PRUDDEN, of New York, has been making some important experiments with a view to determining the effect of freezing on Bacteria. In the case of the *Bacillus prodigiosus*, there were 6300 Bacteria in a cubic centimetre of water before freezing; after being frozen 4 days, 2970; after 37 days, 22; and none after 51 days. Of the *Staphylococcus pyogenes aureus*, there were a countless number before freezing; after 18 days of freezing, 224,598; after 54 days, 34,320; and after 66 days, 49,280. Of the typhoid-fever Bacillus, innumerable before freezing; 1,019,403 after being frozen 11 days; 336,457 after 27 days; 89,796 after 42 days; and 7348 after 103 days. These facts show that certain Bacteria have a remarkable power of resisting the temperature at which ice forms. Dr. Prudden, therefore, recommends that the New York State Board of Health, or other authority, should have power to determine which, if any, of the sources of ice-supply are so situated as to imperil the health of consumers of ice.

AN important new reaction is described in the current issue of the *Comptes rendus* by MM. Varet and Vienne. A current of acetylene was passed through 200 grammes of benzene containing 50 grammes of aluminium chloride during 30 hours, and the oily liquid remaining after removal by washing of the unaltered aluminium chloride was found to yield, on fractional distillation, three distinct products. The first, which came over between 143° and 145°, and which amounted to 80 per cent. of the whole, consisted of pure cinnamene or styrolene, $C_6H_5—CH = CH_2$, a substance occurring in liquid storax (*Liquidambar orientale*), and which was synthetized by M. Berthelot by passing acetylene together with benzene vapour through a tube heated to redness. The second fraction, coming over at 265°–270°, consisted of diphenyl ethane $(C_6H_5)_2 = CH—CH_3$, and the third fraction, boiling at 280°–286°, was found to consist entirely of dibenzyl, $C_6H_5—CH_2—CH_2—C_6H_5$, a solid substance isomeric with diphenyl ethane. These syntheses are extremely interesting, and afford another instance of the singular action of aluminium chloride in enabling us to attack the benzene nucleus.

A CURIOUS series of experiments has just been completed by Drs. Emil Fischer and Penzoldt (*Liebig's Annalen*, B. 239, i. 131) upon the sensitiveness of the sense of smell. These chemists used mercaptan and chlorphenol as their odoriferous substances, and experimented in a room of 230 cubic metres capacity. A gramme of the substance was dissolved in a litre of alcohol; 5 c.c. of the solution were again diluted to a known volume, and 1-3 c.c. of the latter solution measured out into a flask from which a fine jet could be directed by the experimenter to all parts of the room, the air of which was subsequently agitated by the waving of a flag. At a given signal a second experimenter stepped into the room, and took his olfactory observation, which was checked by the independent observation of a third person. The astonishing result was arrived at that our olfactory nerves are capable of detecting the 1/4,600,000 part of a milligramme of chlorphenol and the 1/460,000,000 part of a milligramme of mercaptan. The quantity of mercaptan present in the air of the room was 250 times less than the amount of sodium present in the air of the room in which Bunsen and Kirchhoff made their experiments upon the sensitiveness of the spectroscope, when the sodium lines were just perceptible.

IN our note last week (p. 64) on Mr. Carey Lee's paper in the *American Journal of Science*, for "protochloride" read "photochloride."

THE additions to the Zoological Society's Gardens during the past week include a Brown Capuchin (*Cebus fatuellus*) from Guiana, presented by Mr. George Doddrell; a Rhesus Monkey (*Macacus rhesus*) from India, presented by Mrs. Livingstone; a Common Marmoset (*Hapale jacchus*) from South America, presented by Mr. J. H. Hallett; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. Robert R. MacIver; a Brown Bear (*Ursus arctos*) from Russia, presented by Mr. John Rhind; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. J. W. Deacon; a Bare-eyed Cockatoo (*Cacatua gymnopis*) from South Australia, presented by Sir Nathaniel Barnaby; two Daubenton's Curassows (*Crax daubentoni* ♂ ♀) from Venezuela, presented by Mr. F. G. Thompson; two Madagascar Porphyrios (*Porphyrio madagascariensis*) from Mozambique, presented by Capt. J. C. Robinson, s.s. *Roslin Castle*; a Western Slender-billed Cockatoo (*Licmetis pastinator*) from West Australia, presented by Miss Streeter; a Horse-shoe Snake (*Zamenis hippocrepis*); an Ocellated Sand Skink (*Seps ocellatus*) from Tripoli, North Africa, presented by Mr. George Russell; two Hawk's-billed Turtles (*Chelone imbricata*) from the East Indies, presented by Mr. J. A. Wilson; a Collared Fruit Bat (*Cynonycteris collaris*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

PHOTOGRAPHY THE SERVANT OF ASTRONOMY.—As an instance of the ease with which relative motions of stars can be detected by the aid of photography may be cited the case to which attention is drawn by M. de Gothard in *Astronomische Nachrichten*, No. 2777. On examining a photograph of the cluster G. C. 4440, taken at the Herény Observatory in 1886, M. de Gothard found that a small star of the eleventh magnitude had changed its position relatively to the other stars in its neighbourhood by a considerable amount since the date of Herr Vogel's measurements of the relative positions of several of the components of this cluster executed with the Leipzig equatorial in 1867–69. The star in question is No. 48 of Herr Vogel's list, and it appears to have a proper motion of 2″·3 per annum. No. 46 of the same list appears also to have changed its position.

A NEW MINOR PLANET.—No. 266, if, as it would appear, Dr. Luther's discovery of April 11 be really Hesperia, was discovered by Herr Palisa, at Vienna, at midnight on May 17; magnitude 12.

COMET 1887 *e* (BARNARD, 1887 MAY 12).—Dr. E. Lamp supplies the following elements and ephemeris for this object (*Astr. Nach.* No. 2786) from observations obtained at Cambridge, Mass., on May 12, and at Kiel on May 14 and 16:—

T = 1887 June 18·85945 Berlin M.T.

$$\left.\begin{aligned} \omega &= 17^\circ\ 24'\ 50'' \\ \Omega &= 245\ \ 8\ \ 81 \\ \iota &= 17\ 31\ 09 \end{aligned}\right\} \text{Mean Eq. 1887·0.}$$

log *q* 0·13822

Ephemeris for Berlin Midnight.

1887.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
May 28	15 39 11	20 20·6 S.	0·1492	9·6024	1·54
30	15 43 19	18 45·8			
June 1	15 47 35	17 8·6	0·1456	9·5919	1·64
3	15 51 56	15 29·8			
5	15 56 22	13 50·1 S.	0·1426	9·5854	1·72

The brightness on May 12 is taken as unity. Dr. Lamp describes the comet as being faint and round on May 14, and as showing a nucleus.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 MAY 29—JUNE 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 May 29.

Sun rises, 3h. 53m.; souths, 11h. 57m. 6·9s.; sets, 20h. 1m.; decl. on meridian, 21° 37′ N.: Sidereal Time at Sunset, 12h. 29m.

Moon (at First Quarter on May 30) rises, 10h. 30m.; souths, 17h. 48m.; sets, 0h. 56m.*; decl. on meridian, 11° 40′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	3 55	12 7	20 19	22 49 N.
Venus	6 30	14 54	23 18	24 26 N.
Mars	3 28	11 22	19 16	20 9 N.
Jupiter	15 55	21 13	2 31*	9 3 S.
Saturn	6 53	14 59	23 5	22 2 N.

* Indicates that the setting is 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. ° °
29	45 Leonis	6	20 37	21 34	69 314
29	ρ Leonis	4	23 10	0 2†	88 311
30	σ Leonis	4	19 46	20 6	152 188
June.					
2	94 Virginis	6	20 29	20 39	335 319
4	49 Libræ	5½	19 55	20 30	345 284

† Occurs on the following morning.

May.	h.	
29	2	Mercury at least distance from the Sun.
30	17	Venus in conjunction with and 2° 15′ north of Saturn.
June.	h.	
2	12	Jupiter in conjunction with and 3° 22′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	May 29,	1 57	*m*
			June 3,	1 37	*m*
δ Libræ	14 54·9	8 4 S.	„ 4,	2 18	*m*
U Coronæ	15 13·6	32 4 N.	„ 1,	2 6	*m*
W Herculis	16 31·2	37 34 N.	„ 4,		*M*
U Ophiuchi	17 10·8	1 20 N.	May 30,	2 34	*m*
			and at intervals of	20 8	
U Sagittarii	18 25·2	19 12 S.	May 29,	2 0	*m*
			June 1,	1 0	*M*
β Lyræ	18 45·9	33 14 N.	May 31,	23 0	m_2
R Lyræ	18 51·9	43 48 N.	„ 31,		*M*
S Aquilæ	20 6·4	15 17 N.	„ 29,		*m*
δ Cephei	22 25·0	57 50 N.	June 2,	3 0	*M*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers.

	R.A.	Decl.	
Near β Coronæ	227° ...	30° N.	
	290 ...	60 N.	Rather slow.
From Vulpecula ...	303 ...	24 N.	Swift.
Near ι Pegasi	333 ...	27 N.	Very swift.

GEOGRAPHICAL NOTES.

PROF. GUIDO CORA has constructed a map of the district around Massowah on a scale of 1 : 200,000. He has been able to make use of a variety of original material, and the map will be of use to those interested in the events which are taking place in that region.

THE May number of *Petermann* begins with a short paper on the ethnology of British Columbia, by Dr. Fr. Boas (with map). Dr. H. Polakowsky contributes a paper on the proposed Nicaragua Canal from Greytown to Lake Nicaragua; Paul Emmrich, a paper on the Transvaal gold-fields; and Dr. Rink, on recent Danish exploration in Greenland. Herr Wichmann gives an abstract report of the proceedings of the German Geographentag at Karlsruhe.

THE new number (Band v. Heft 2) of the *Mittheilungen* of the German African Society contains the results of several recent important African expeditions. There is a large-scale map and sections by R. Kiepert, of Böhm and Reichard's journey from Lake Tanganyika to the Lualaba, with remarks by Herr Reichard, observations on altitude, and on the meteorology of the region, by Danckelman. We have also a map of Standinger and Hartert's journey to Kano, Sokoto, and Gandu, with their journals, and remarks on the hypsometrical observations by Danckelman. Prof. Zöppritz contributes a paper on the late Herr Flegel's thermo-barometrical observations on the Niger. There is a letter from Lieut. Tappenbeck on his Congo exploration of 1885, and an analysis of Lieut. Kund's boiling-point observations by Danckelman.

ON ZIRCONS AND OTHER MINERALS CONTAINED IN SAND.

OWING to recent attempts to employ the brilliant light of incandescent zirconia and allied earths instead of that of gas or electricity, attention has been drawn to the sources of the rarer earths.

It occurred to me that an inexhaustible supply might be obtained from the small crystals of zircon so widely distributed in rocks, sands, and soils, if an easy method of concentration could be found. I wished particularly to examine some of the larger deposits of sand, especially the fine-grained deposits, but was stopped by want of the requisite appliances. The matter having been mentioned by a friend to the Director-General of the Geological Survey, I at once received permission to use, for this investigation, the petrological laboratory attached to the Survey. The following results were obtained therein.

The denser minerals were separated from the sand by means of Sonstadt's solution of density 2·86, and borotungstate of cadmium of the density of 3·2.

		Per cent.
Dense minerals in	Lower Bagshot sand, Hampstead	3·85
,,	,, another sample	3·60
,,	,, another sample from apparently a different deposit near the Fire-engine Office	1·04
,,	,, High Beeches, Essex	1·32
,,	Glacial drift sand, Bagillt, North Wales	0·30
,,	Casting sand, Bullwell, near Nottingham	0·16
,,	Lower Bagshot sand, near Otterburne, Hants ...	0·12

These heavy minerals were found to be composed principally of matter attracted by a strong magnet, and of zircons, rutiles, tourmalines, and other grains, the relative proportions varying considerably. It will be observed that the richest and the poorest samples were from deposits of the Lower Bagshot sands. The drift sand from North Wales contained, in addition, many garnets. [Since this was written I have received a sample from High Beech Reservoir, 359 feet above Ordnance datum, containing 4 per cent. of dense minerals.]

The sand to which I devoted most attention, and of which I propose to give an account, is represented in the foregoing table by the first two samples. The samples were taken by me from a cutting recently made for the construction of a drain to connect "The Spaniards" and North-End, Hampstead. It was 2 or 3 feet wide, and 10 or 12 feet deep through great part of its length. It passed through a fine-grained yellowish sand, which was thrown out of the cutting or tunneled through in order to lay the drain-pipes. I made an average sample with as much care as though the sand had been a valuable ore which I was sampling for sale or purchase. It represented at least 1000 tons of sand in sight or cut through, and probably many acres of such sand on either side of the cutting. It consisted of grains so small as to pass almost entirely through a piece of cambric, 120 holes per square inch, stretched on a frame and used as a sieve. The average size of the grains was about the two-hundreth of an inch.

By means of dense solutions, vanning, analysis, and the constant use of the microscope, it was found that the sand had the following composition. It is given as an approximation to the truth, and does not, I think, vary more from it than must be expected in cases where it is not possible to get the minerals pure enough for weighing. Only under the microscope could the mixtures and impurities be estimated.

		Per cent.
Quartz, with 1 or 2 per cent. of flint fragments ...	about	75
Feldspar	,,	20
Grains attracted by a strong magnet	,,	2
Clay	,,	1
Zircons in recognisable crystals	,,	$\frac{1}{2}$
Grains, more or less opaque, probably zircons	,,	$\frac{1}{4}$
Rutiles	,,	$\frac{1}{2}$
Tourmalines	,,	$\frac{1}{10}$

In addition to the above, there were about 1 per cent. of grains over the density of 3·2, in regard to the composition of which I cannot at present say anything. A few appeared to be cleavage flakes of cyanite, but the majority were opaque earthy-looking bodies of various colours.

Feldspars.—These were nearly all of density equal to or lower than that of quartz. The majority were more or less cloudy, but some were quite transparent, showing the structure of microcline. A few showed the banded structure of plagioclase. Owing to the small size of the particles, and to their cavities and inclosures, I found it impracticable to get a satisfactory separation by dense solutions; and as it was impossible to distinguish the grains in all cases under the microscope, recourse was had to analysis of grains (consisting principally of quartz and feldspar) floated off from the denser minerals in a solution in which quartz floated and anorthite sank. It was found that they contained 94 per cent. of silica and 4·6 of alumina, with an unweighable trace of lime. The alkalies were not determined. This would correspond to about 20 per cent. of feldspar, but the estimate is perhaps rather high, as amongst the matter floated by the solution were found some particles which looked like glauconite covered by a transparent covering of varying thickness; also a little mica and some opaque grains of doubtful origin. It was interesting to note how the small fragments of feldspar have remained unchanged since the parent rocks were formed, and as they survived the disintegration of those rocks so they have continued unchanged in the sands. I saw several containing each a small zircon, and some contained what I think were microliths of apatite. The feldspars were mostly in angular fragments like the quartz.

The zircons are generally transparent and colourless prisms with double terminations of various kinds; many are more or less rounded and some wholly rounded as by attrition.

The rutiles are oblong and rounded grains, but many are sharply edged prisms, and a few have double terminations. Twins are not common, but both knee-shaped and kite-shaped twins occur. These rutiles resemble those of the metamorphic rocks, in which rounded grains and sharply defined crystals are met with side by side.

The tourmalines are generally in flat plates, more or less rounded, but some are perfect crystals with double terminations. They vary much in colour and power of absorption. Various methods of concentrating the zircons were tried. The simplest found was to sift the sand in air or water through a sieve with 120 holes to the inch, that being the smallest mesh I could meet with. In coarse-grained sands, such as the drift sand of North Wales, a considerable enrichment is thereby at once effected, but only a partial enrichment takes place in working with sands so small-grained as to pass entirely through the sieve.

Thus when 8 ounces of the Hampstead sand were sifted till 4 ounces had passed, it was found that the zircons and rutiles, being smaller, smoother, and heavier than the sand, passed through the sieve faster. By again sifting the 4 ounces which had passed till 2 ounces passed, a further enrichment was found to have taken place. The 2 ounces were again sifted on the same sieve till 1 ounce had passed, which was again sifted till ¼ of an ounce passed. This was examined quantitatively, and found to consist of 54 per cent. of quartz and feldspar, and 46 per cent. of dense minerals. By vanning and submitting the residue to the action of a strong magnet, almost nothing but zircons and rutiles remained. It is needless to say that much remained with the sand, especially in the latter parts of the operation.

A current separater was tried, but it seemed more difficult to work, though perhaps it might answer better on a larger scale, where it might be set to work automatically. There was no difficulty in getting a considerable enrichment, but it was evident that a great deal of care would be required in "sizing" the particles before a good separation could be effected. The most hopeful method seemed to be that of washing away the medium-sized grains of sand and afterwards sifting the sediment.

Perhaps in some of the streams running through the Bagshot or other sands natural eddies may be found or formed artificially from which enriched sand may be dredged, or it may be got on the sea-shore under sand-cliffs.

The object of the present communication is to draw attention to the matter in hopes that some deposit richer in zircons than the Hampstead sand may be found. Much care must be taken in sampling, because the sands, having been deposited from currents, must vary in composition. A trial is easily made by anyone accustomed to use a microscope and who knows the minerals by sight under such circumstances. A thimbleful of such sand as exists at Hampstead is enough for a trial by vanning, but if one of the dense liquids be used, from 10 to 20 grains by weight of the sand will give a good microscopic slide of the dense minerals. It may of course prove that the Hampstead sand is a residue of denudation in which the denser minerals have accumulated. In that case it is not improbable that other similar deposits may be found, some, perhaps, much more zirconiferous.

On the whole it appears that the matter is worthy of further attention. In some future communication I hope to be able to give an account of the composition of the matter attracted by a strong magnet, and also of the grains of earthy-looking minerals over the density of 3·2, and of any richer deposit of zirconiferous sand of which I can obtain reliable samples.

ALLAN B. DICK.

THE ROLLING CONTACT OF BODIES.[1]

WHEN two solid bodies roll upon each other, points in the surface of one successively come into contact with corresponding points in the surface of the other in a way which differs essentially from that which occurs in sliding contact, and it is the nature of this rolling-contact that the lecturer proposed to discuss in an experimental manner.

In the first place, it is well to understand clearly the nature of the relative motion of the two points which come into contact when the surfaces are such that no appreciable distortion of them takes place, and for this purpose one of the two bodies must be at rest. These may respectively be taken as the plane surface of the ground and a circular disk rolling upon it. An approximate representation of this motion is given by the end of the spokes of a wheel without its tyre. In this case it is seen that a point of the rolling body, when it is just coming into contact with the fixed surface, does so in a direction at right angles to the surface at rest, and also leaves it in the same direction. This action is very similar in kind to that which occurs with the continuous circle formed by the tyre. The path of a point in the rim can be drawn in a way visible to the audience by means of a piece of apparatus consisting of two circular glass plates held together by a hollow brass spindle in which slides an arm carrying a brush. The brush traces the well-known cycloid, of which the only portion now to be considered is that where it directly approaches the surface beneath. This part is perpendicular to that surface, and when epicycloids are drawn, by rolling the disk upon the arc of a circle, the same fact is brought out.

One body may, however, not merely roll upon another, and a normal pressure be exerted, but they may exert a tangential force upon each other. It is convenient to keep these two cases separate; examples of them being respectively the wheels of a railway carriage and those of the locomotive which draws it along. It is to be noted that the object in the former case is to permit one body to move relatively to another without permitting sliding contact of their surfaces, whilst, in the latter case, in addition to this, the object is to obtain such motion. There are, however, many cases in which it is merely the motion of a body about one point which is required, such as when motion is transmitted from the edge of one rotating disk to another, and then this distinction still more closely holds, as the normal pressure is only obtained so as to insure the necessary tangential resistance. Thus the objects of rolling motion may be classed as being—

(1) To *allow* the relative motion of one body to another with which it is in contact without permitting relative motion of that part of their surfaces in actual contact.

(2) To *obtain* the relative motion of such parts of the surfaces of bodies as are not in contact by means of statical contact of the parts which are.

The lecturer then proceeded to consider the practical proofs of the smallness of the resistance to rolling in cases where the distortion of the surfaces in contact is very small, as illustrated by the small tractive force required for heavy bodies properly mounted on wheels or on roller-bearings; mentioning the case of a 12-horse-power engine, the shaft of which continued to rotate for three-quarters of an hour after the motive power was withdrawn; and another case, of a turntable weighing 14 tons, which was kept in motion by a weight of 3½ pounds acting upon it by means of a cord passing over a pulley. The small distortion of such surfaces when transmitting motion requiring expenditure of energy to maintain, was next made clear by giving certain facts as to the accuracy with which one surface was developed or measured out upon another. An account was given of experiments made with apparatus specially prepared by the lecturer to investigate this point. This apparatus consisted of two accurately turned brass disks properly mounted upon a frame, and the relative positions of these disks could be interchanged so that any minute differences in their peripheries could be detected. The experiments, which were very difficult to carry out accurately, showed that under the best circumstances, motion with an error of only 1 in 300,000 of the distance passed over could be obtained. This accurate measuring out of the surfaces one upon another was employed in various ways for purposes of measurement, and these, by means of models and diagrams, were briefly explained.

Although the foregoing facts prove that, under suitable conditions, distortion at the points of contact is very small, yet some resistance at these points *always* occurs, because no bodies are perfectly hard; and the nature of this distortion and consequent resistance was next discussed.

The explanation of the resistance opposed by a soft surface to a hard body rolling upon it, as first given by Prof. Osborne Reynolds, was applied by the lecturer to account for a very remarkable effect produced in the disk, globe, and cylinder integrator of Prof. James Thomson. This effect, which was the turning of the cylinder when the sphere was rolled along it in a horizontal direction, was reproduced by means of a large model. The action of a soft body rolling upon a hard surface was next considered, with the result of showing that the same reasoning would not account for the turning of the cylinder in the same direction as before with the above model, and the lecturer then proceeded, by means of diagrams, to offer an explanation of this and other phenomena. The various effects obtained with bodies of different relative degrees of hardness were discussed at length, but figures would be needed to make these points clear. Finally, an explanation was given of the cause of an error which always appeared in a certain important class of integrators caused by the slipping of the edge of a disk over a surface on which it rolled in circumstances under which it had apparently never been suspected that slipping did actually take place. This the lecturer had been enabled to discover and measure by means of a special piece of apparatus, a model of which was exhibited and the effects shown by its means.

The facts and reasoning, which were given in the lecture, all related to the rolling contact of bodies, and the lecturer ventured to think that, imperfect as the treatment of the subject had been, it was one of such importance, not merely from the point of view of the practical applications he had mentioned, but in its scientific aspect, dealing as it did from a novel point of view with the nature and properties of solid bodies, as to be worthy of being thus brought before the Royal Institution.

[1] Abstract of Lecture delivered at the Royal Institution, by Prof. Hele Shaw, on April 29.

A REMARKABLE METEOR.

ON March 17 last, about 4.15 p.m., the track of a brilliant meteor in the southern heavens, at an altitude of 30°, was observed by Mr. R. Brough Smyth, of Sandhurst, Victoria, Australia. Writing to us on March 19, Mr. Smyth says:—

"The line was silver-white and of considerable breadth. The sun was shining in a clear sky. Owing to the view being intercepted by large gum-trees growing in the grounds around my house, I could see only a portion of the arc described. Subsequently, a little after 5 o'clock p.m., the sky was obscured by a kind of mist or vapour at a great height—in colour between steel-grey and lead-grey, and with tints similar to those of the metal bismuth over the whole. All objects looked green or greenish in the strange light. The meteor was observed at Salisbury in South Australia, at Coleraine in the extreme west of Victoria, and at various places eastward—say over a distance of 400 miles. It travelled apparently from east to west, and as far as known was visible in the southern part of Australia only. In some places it presented the appearance of a blood-red ball, and at Beaufort the ball is said to have exploded with a loud report, sending up a streak of fire, accompanied with the hissing of escaping steam, as from an engine. It left a cloud of greyish smoke. This smoke-like cloud was observed in other places. At Warrnambool on the west coast, and at Terang, twenty-five miles north-eastward, shocks of what were supposed to be earthquakes were felt at the time of the disappearance of the meteor. Cattle and horses galloped about in alarm, houses were shaken, windows rattled, and the wild fowl in the lakes were disturbed, and took wing. I inclose cuttings from the *Argus* containing accounts of this phenomenon."

The "cuttings" inclosed by Mr. Smyth are a series of telegrams, describing the phenomenon as seen in various parts of Australia. At Coleraine, "a brilliant ball of fire shot from the zenith in a clear sky to 30° above the horizon, and then disappeared as it exploded, leaving a large cloud of white smoke, which was visible for half an hour. Exactly six minutes subsequently, two distinct shocks like cannon reports were heard, with a perceptible tremor of the atmosphere. The phenomenon was witnessed by 500 persons." At Merino, "a most unusual phenomenon appeared in the eastern sky. A streak like smoke from a volcano appeared. Immediately after the appearance, a report like distant thunder was heard from the same direction. It was thought that an aërolite of immense size had fallen between Merino and Hamilton." At Stawell the "meteor appeared to burst just beyond the town in a cloud of smoke, which was immediately followed by a loud crash like thunder." From Terang it is reported that at Lake Keilambete "the black swans were noticed to rise suddenly off the lake. A rumbling noise appeared to pass under, causing the cattle grazing on the banks of the lake to scamper away, and on gaining some distance they were seen to look back. The noise was heard in other places, and seemed to pass to the south-west." At Portland, "three distinct reverberations like the booming of artillery were heard about 4 p.m." The people at Warrnambool, hearing, shortly after 4 o'clock, loud detonations like a volley of musketry, with subsequent dropping shots, rushed out of their houses; and "the cattle were paralyzed with fear at the sounds." The disappearance of the meteor over Beaufort, where it is said to have exploded, "was followed by earth tremors and a rumbling sound as of the firing of heavy artillery. The vibrations lasted for ten seconds. Several houses were shaken severely. No substance appears to have fallen to the earth."

SCIENTIFIC SERIALS.

THE contents of vol. lv. part 2, No. 4, of the *Journal of the Asiatic Society of Bengal*, are varied. They commence by a memoir on the land shells of Perak, by Dr. O. F. v. Möllendorff, in which 58 species (many new) are enumerated or described. Then follows an account of solar thermometric observations at Allahabad, by S. A. Hill, Meteorological Reporter to the Government of the North-West Provinces. The third memoir is an historico-geographical study on probable changes in the Punjab and its rivers, by R. D. Oldham, of the Indian Geological Survey, a paper on which much research has been expended, tending to prove that a second large river, independent of the Indus, once existed in the Punjab, and that the geological changes which converted a once fertile district into a desert probably date so recently as the early centuries of the Christian era. The next is a very important entomological investigation of the butterflies of Cachar, by Prof. Wood-Mason and Mr. L. De Nicéville, enumerating no less than 247 species obtained between the end of March and the beginning of October. A remarkable feature is the large number of *Hesperiidæ*, of which 53 distinct species were obtained. There are valuable notes on seasonal and local variation, and a considerable number of new species are described, and mostly figured on four plates, one of which is a chromo-lithograph executed in London, the others "autotype," and apparently very successful examples of what may be produced by the process as applied to natural history subjects. Dr. King follows with a short paper on some new species of *Ficus* from New Guinea, in which the author largely quotes from and anticipates a monograph on Indo-Malayan and Chinese figs prepared for the Linnean Society; the remarks are worthy of very careful study, and open up much new light on the somewhat obscure subject known as "caprification." The concluding paper is a very short one by Mr. J. S. Baly on a new species of *Hispa* destructive to the "dahn" crops in Chittagong. On the whole this part is one of the most valuable that have been issued by this long-established Society.

Proceedings of the Linnean Society of New South Wales, 2nd series, vol. i., part 4, February 22, 1887 (Sydney), contains:—*Zoology*: George Masters, catalogue of the described Coleoptera of Australia, part 6.—E. Meyrick, descriptions of new Lepidoptera. A large number of new species and several new genera are described; a new species of Thalpochares is given the name of Coccophaga, from the singular habits of the larva, which feeds solely on a Coccus infesting a Macrozamia.—E. P. Ramsay, notes on the eggs of various Australian birds; list of Western Australian birds collected at Derby; on the nest of *Pycnoptilus floccosus* (plate xx.); on a new species of Hapalotis (*H. boweri*) (plate xviii.).—E. P. Ramsay and J. Douglas-Ogilby, on a new species of Apogon (*A. roseigaster*).—William Macleay, on a new species of Hoplocephalus (*H. collaris*).—C. W. De Vis, on new or rare vertebrates from the Herbert River; describes a new Pseudochirus (*P. mongan*), a new Dromicia (*D. frontalis*), and records the occurrence of some rare species.—A. J. North, notes on the bower birds, and some references to authentic descriptions of Australian birds' eggs.—*Botany*: E. Haviland, flowering seasons of Australian plants.—J. Stirling, on the Rutaceæ of the Australian Alps.—Baron von Mueller, on some hitherto undescribed plants of New South Wales. *Grevillea renwickiana* is described as quite procumbent, with elongated branches, being in this respect like *G. laurifolia* and *G. repens*, but differing from both in the larger and much less numerous flowers; also new species of Melaleuca, Bossiæa, and Pultenæa.—*Palæontological*: F. Ratte, notes on Australian fossils.—W. J. Stephens, on some new Labyrinthodonts (plates xiv. and xxii.).—J. Mitchell, on the geology of Bowning, N.S.W.

Zeitschrift für wissenschaftliche Zoologie, vol. lv. Part 2, April 13, 1887, contains:—Dr. O. Schultze, researches on the ripening and the fertilization of the amphibian ova, part 1 (plates xi. to xiii.).—Dr. Wilhelm Roux, on a fungus living parasitically in bones (*Mycelites ossifragus*) (plate xiv.). The author gives an account of the filaments of this fungus occurring in the bones of a large number of extinct forms of mammals, reptiles, and fishes.—Dr. Otto Zacharias, contributions to the pelagic and littoral fauna of the German Ocean. In this paper are described a large number of Entomostraca, Rotatoria, Hydrachnida, and Turbellaria, some new. In an appendix, S. A. Poppe describes a new species of Temorella from Holstein and Mecklenburg (plate xv.).—Dr. H. Strahl, on the walls of the yolk-sac and on the parablast in lizards (plate xvi.).—Dr. Joseph Heinrich List, on the glandular structures in the foot of *Tethys fimbriata*, L. (plate xvii.). These glands are found both on the upper and under side of the feet, and are of four different sorts; while some are slime organs, others may be phosphorescent organs.—Dr. Eugen Korschelt, on some interesting phenomena in the formation of the eggs of insects (plates xviii., xix.).

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, April 28.—"On the Homologies and Succession of the Teeth in the Dasyuridæ, with an Attempt to trace the History of the Evolution of Mammalian Teeth in general." By Oldfield Thomas, British Museum (Natural History). Communicated by Dr. Albert Günther, F.R.S.

The true homologies of the different teeth in the Marsupialia, and especially in the *Dasyuridæ*, have long been in a state of confusion, a confusion that has been chiefly in regard to the premolars, of which some members of the family have two, others three, while generalized Placentals have four, and it is therefore necessary to prove which teeth have been successively lost in order to find out the correct homologies of the remainder.

Firstly, as to which of the three premolars of ordinary Marsupialia has been lost in *Dasyurus*, with only two, it is shown that it is the last premolar, or pm.^{4}, that is missing in this genus.

Next, it was necessary to find out which of the original four premolars had disappeared in the ordinary three-toothed genera of the Polyprotodonts, and this has been able to be done by the fortunate discovery of a specimen of *Phascologale* in which there are four premolars on one side, the additional tooth being inserted behind the first premolar. The missing premolar is therefore pm.^{2}, the resulting premolar formula of *Phascologale* and *Thylacinus* being P.M. $\frac{1 \cdot 0 \cdot 3 \cdot 4}{1 \cdot 0 \cdot 3 \cdot 4}$ and of *Dasyurus* P.M. $\frac{1 \cdot 0 \cdot 3 \cdot 0}{1 \cdot 0 \cdot 3 \cdot 0}$.

The milk dentition in several of the *Dasyuridæ* is then described, and also that of the Mesozoic *Triacanthodon serrula* (Owen), which is definitely proved to have a true Marsupial milk dentition.

An attempt is then made to trace out the history of the evolution of mammalian teeth in general, and it is suggested that the process by which a milk tooth was developed consisted of two stages, firstly, a preliminary retardation of the permanent tooth, and secondly, of the development of a temporary tooth in the gap in the tooth-row caused thereby; the retardation in the first case being useful for packing purposes in a large-toothed animal, while in a small-toothed form the same retardation, if present by inheritance, would cause a more or less disadvantageous gap, best filled by the assumption of a milk tooth.

Following out this idea, it is shown how easily the transition from the Metatherian to the Eutherian state of tooth-change may have taken place, a transition by the help of which a complete series of diagrams can be drawn up, following the history of each individual tooth, from the dentition of the earliest mammals, homodont and monophyodont, down to the varied forms of dentition, heterodont and diphyodont, existing at the present day.

For the Edentates alone it is necessary to draw up a special branch of tooth development arising directly from the Prototheria, a branch for which the name of "Paratherian" is proposed.

Physical Society, May 14.—Prof. W. E. Ayrton, Vice-President, in the chair.—Mr. T. Mather was elected a member of the Society.—The following papers were read:—On a modification of a method of Maxwell's for measuring the coefficient of self-induction, by Mr. E. C. Rimington. The method referred to is given in Maxwell's "Electricity and Magnetism," § 778, vol. ii., and is called "comparison of the electro-static capacity of a condenser with the electro-magnetic capacity of a coil." The apparatus used consists of a Wheatstone's bridge having the coil in one, and the condenser as a shunt to the opposite, arm. In order that no deflection may be produced, either for steady or unsteady currents, a troublesome double adjustment of the resistances is necessary, and to obviate this the modification was devised. It consists in placing the condenser as a shunt to only part of the arm, and this part can be varied by sliding contacts without altering the whole resistance of the arm. An ordinary resistance balance for steady currents is first obtained, and the sliders are then adjusted until no deflection is produced on breaking the battery circuit. Under these circumstances it is shown that $L = Kr^2\frac{D}{B}$, where K is the capacity of the condenser, r the resistance between the sliders, and D and B the resistances of the arms in which the coil and condenser are placed. The conditions of maximum sensibility are investigated, and also those under which a telephone may replace the galvanometer; in the latter it is shown that the only possible solution is when $r = B$, *i.e.* Maxwell's arrangement. The author believes his modification would be made much more sensitive by adopting the "cumulative" method used by Profs. Ayrton and Perry in their secohmmeter; and in his case neither the speed nor the "lead" need be known. Mr. W. N. Shaw asked whether any serious difficulties were experienced with telephones, owing to electro-static capacities of wires, &c., and Mr. W. E. Sumpner pointed out that the particular arrangement given in Maxwell is not always the most sensitive, as was shown in his remarks at the last meeting of the Society of Telegraph-Engineers. Mr. Bosanquet thought the method a valuable one, and hoped many experiments would be made on coils whose coefficients were calculable, in order to find out the differences between calculated and observed results. Prof. Ayrton referred to the paper by Prof. J. J. Thomson in the Philosophical Transactions, and pointed out that the formula there given for the capacity of a condenser in electro-magnetic measure, is identical with that given in Maxwell, § 776, when the printer's error of interchanging a and α in the denominator for R_2 is corrected.—On the production of sudden changes in the torsion of a wire by change of temperature, by Mr. R. H. M. Bosanquet. A very fine hard-drawn platinum wire, four or five feet long, was used as a suspension for a ballistic galvanometer, and exhibited peculiar phenomena. The steel needles were replaced by brass ones, and the peculiarities investigated. When the room was warmed, the needles swung round nearly 70° for a few degrees rise of temperature, and remained in about the same position for further rises. If it was now cooled a few degrees (3° or 4° F.), they quickly returned to their initial position. The author has not found a complete explanation, but believes it to be due to unequal expansion, and loose contact amongst molecules, and has devised a simple mechanism to illustrate his meaning. Remarks and suggestions were made by Prof. Perry, Mr. Lant Carpenter, and the Chairman.—On a magnetic potentiometer, by Mr. A. P. Chattock, read by Prof. Reinold. The "so-called" magnetic resistance between two points on a magnetic circuit may be expressed as the ratio of the difference of potential to the total induction passing from one to the other (provided there be no magnetomotive force between them). From the fact that the volume integral of induction through a wire helix of constant cross-section is proportional to the average difference of potential between its ends, it follows that any alteration in that difference of potential will give rise to an E.M.F. in the helix proportional to that alteration. Hence, if the wire be connected to a ballistic galvanometer, the combination may be called a magnetic potentiometer. A helix is formed by winding wire uniformly on a piece of solid india-rubber, or canvas gas-tubing, of constant cross-section, using an even number of layers to avoid external inductive effects, and leaving a small space between the turns so as to allow the tube to bend without elongating. Experiments made to measure the difference of potential between the ends of a magnet gave satisfactory results. One end of the helix was held stationary at one end of the magnet, whilst the other was moved quickly to the other end of the magnet, and the resulting throw of the galvanometer observed. This was next done at two operations, and the sum of the two throws was very nearly equal to the first. The results can be reduced to absolute measure by passing the helix through a coil of n turns, bringing its ends together, and starting or stopping a current, C, in the coil, the resulting throw of the galvanometer being noticed. The magnetomotive force used in this experiment is $4\pi n$C. An interesting discussion followed, in which Prof. Perry, Mr. Shaw, Prof. Ayrton, and Mr. Bosanquet took part, the latter mentioning a measurement of magnetic potential made by himself some years ago.—In consequence of the absence of Prof. S. P. Thompson, his paper on secondary generators was postponed till next meeting.

Geological Society, May 11.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—Further observations on *Hyperodapedon gordoni*, by Prof. T. H. Huxley, F.R.S. The author briefly noticed the circumstances under which he first described the occurrence of Lacertilian and Crocodilian fossils in the Elgin sandstones, and the confirmation which his views as to the Mesozoic age of these remains had received from the discovery of *Hyperodapedon* in English Triassic rocks and in India. The original type of *Hyperodapedon gordoni* from Elgin was, however, in bad condition, and the receipt at the British Museum of a second much better preserved skeleton, found in the Lossiemouth quarries of the same neighbourhood, had enabled him to add considerably to the known characters of the genus, and to compare it more thoroughly both with the recent *Sphenodon* (or *Hatteria*) of New Zealand and with the Triassic *Rhynchosaurus articeps*, several specimens of which are in the British Museum palæontological collection. The recently discovered *Hyperodapedon* skeleton was of nearly the same size as that formerly described, and must have belonged to an individual about 6 or 7 feet in length. The specimen was exposed by the splitting of a large block of sandstone, and comprised the skull, the vertebral column as far as the root of the tail, all the bones of the left and of part of the right fore-limb, and those of the right hind-limb, the whole almost in their original relations. The bones were described in order and compared with those of *Sphenodon*, the most important differences in *Hyperodapedon* being the following:—(1) The centre of the presacral vertebræ are ossified throughout and more or less opisthocœlous, especially in the cervical region. (2) The anterior cervical vertebræ have long and strong ribs. (3) The external nares are not separated by bone. (4) Conjoined premaxillary bones form a long, conical, curved, pointed rostrum, which is received between the rostral processes of the mandible. All these were devoid of teeth and probably sheathed in horn. (5) The palatal area is very narrow in front and wide behind, with strongly curved lateral boundaries. (6) The posterior maxillary and palatal teeth are multiserial. (7) The rami of the mandible are united in a long symphysis, behind which they diverge widely, and the dentigerous edges are strongly concave upwards as well as outwards. (8) The mandibular teeth in front are set into a close, apparently continuous palisade, and become distinct and conical only at the posterior end of the series. (9) The fore-foot is remarkably short and stout, with metacarpals of equal length. The relations of *Rhynchosaurus* to *Hyperodapedon* and *Sphenodon* were then dealt with, the first-named being shown to occupy in some respects an intermediate place between the two others. The skull of *Rhynchosaurus* resembles that of *Hyperodapedon* in its single anterior nasal aperture, its premaxillary and mandibular rostral processes, and in having more than one series of palatal teeth; but in general form and in the shape of the maxillæ, palatal bones, and rami of the mandible it departs far less from *Sphenodon* than *Hyperodapedon* does. Some comparisons of the limb-bones were also made. The three genera mentioned were shown to form a particular group, which, however, had no claim to ordinal distinction, and appeared to form a family, Sphenodontidæ, of the Lacertilia, comprising two sub-families, Rhynchosaurinæ (including *Rhynchosaurus* and *Hyperodapedon*) and Sphenodontinæ. The fact that in this Lacertilian group the highest known degree of specialization, as shown in *Hyperodapedon*, was attained as early as the Triassic epoch, showed that in Permian times, or earlier, Lacertilia existed which differed less from *Sphenodon* than either of the Rhynchosaurinæ did. Not only was the Lacertilian type of organization clearly defined in the Triassic epoch, but it attained a degree of specialization equal to that exhibited by any modern lizard. The reading of this paper was followed by a discussion, in which the President, Dr. Geikie, Prof. Seeley, Mr. Lydekker, Prof. Boyd Dawkins, and others took part.—On the rocks of the Essex drift, by Rev. A. W. Rowe.—On Tertiary Cyclostomatous Bryozoa from New Zealand, by Mr. Arthur W. Waters.

EDINBURGH.

Royal Society, April 18.—Sir W. Thomson, President, in the chair.—Prof. Rowland's photographs of the solar spectrum were exhibited.—The President read a paper on ship-waves, and another on the instability of fluid motion. Both papers appear in the *Phil. Mag.*—Mr. D. S. Sinclair gave a communication on an experimental research in magnetism.—A paper by Mr. A. H. Anglin on the summation of certain series of alternants was submitted.—Prof. Crum Brown read a paper by Mr. H. Marshall on cobaltic alums.—Mr. G. N. Stewart submitted a synopsis of researches on the effect produced on the polarization of nerve by stimulation.

May 2.—Sir Douglas Maclagan, Vice-President, in the chair.—Prof. J. B. Haycraft read the third part (on the sense of smell) of a paper on the objective cause of sensation.—Prof. Crum Brown read a paper on the physics of noise. His object is to investigate the various components which make up ordinary noises, such as a hissing sound.—Prof. Dittmar and Mr. C. A. Fawsitt communicated a paper on the physical properties of methyl alcohol.—Prof. Dittmar also discussed the instability of the double salts of $M''SO_4.R'_2SO_4 + 6H_2O$ of the magnesium series.—Mr. J. Rattray described a diatomaceous deposit found at North Tolsta, Lewis.

PARIS.

Academy of Sciences, May 16.—M. Janssen in the chair.—Obituary notices of the late M. Boussingault, member of the Section for Rural Economy, who died on May 11, by MM. Schlœsing, Troost, and the President.—On some deviations from the normal direction of sound calculated to render ineffective the fog-signals and similar appliances employed in navigation, by M. H. Fizeau. The paper, written with reference to some recent shipping disasters during foul weather, shows on theoretical grounds that, the surface of the sea being at times warmer than the surrounding atmosphere, the aërial strata must in calm weather decrease in temperature upwards to a certain height above sea-level. This occurs not only at night, but also frequently during the day in foggy weather. Hence the sounds of the fog-signals, intended to be propagated horizontally, are necessarily affected by the differences of atmospheric temperature, those nearest the surface of the water acquiring greater velocity than those traversing the higher strata. Thus is at times produced a sort of "sound mirage," perfectly analogous to the well-known corresponding phenomena of light. Once the cause of the deviations is understood, the means of counteracting them will easily suggest themselves.—Effects of earthquakes on magnetic instruments, by M. Mascart. The reports of magnetic disturbances received from various stations in France, England, Germany, Russia, and other European countries, show great discrepancies as to the time and intensity of the shocks; but whether these discrepancies are to be attributed to possible errors of observation, to the difference in the character of the instruments, or to physical causes, cannot at present be determined. If the cause of the disturbances is really electric, its very mechanism is absolutely unknown.—Observations of Barnard's new comet, *e* 1887, made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan. This comet, discovered on May 12, at Cambridge, in the United States, was seen at Paris on May 14, when it presented the appearance of a round nebulosity of 1′ diameter, and of the thirteenth magnitude, with considerable central condensation, notwithstanding its slight altitude above the horizon.—On the direct determination of the differential coefficient $\frac{dp}{dt}$, relative to saturated vapours, by M. A. Perot. It is shown that the mechanical equivalent of heat may be determined by the well-known relation—

$$L = \frac{1}{E} T (u' - u) \frac{dp}{dt},$$

which is obtained by applying to a liquid mixture and its vapour the principle of equivalence, and that of Carnot. In order to approximately determine this quantity, the author has undertaken to measure on the same sample of pure ether, at a temperature of 30°, the different parameters entering into the preceding relation—u', u, L, and $\frac{dp}{dt}$. To determine $\frac{dp}{dt}$ he employs a special method, which enables him to measure separately the two corresponding quantities dp and dt. The determinations have been made for the temperatures 29° to 31° inclusive, within which interval they may be represented by the formula—

$$\frac{dp}{dt} = 2{\cdot}2750 + (t - 29)\,0{\cdot}0834.$$

—Intertropical diurnal and annual variations of terrestrial magnetism (second note), by M. Ch. Lagrange. By comparing the observations recorded at two stations on either side of and equidistant from the equator, such as Bombay and St. Helena, Hobartown and Toronto, the author finds that there exists in the atmosphere and in the earth a system of currents moving east and west, whose strata of greatest intensity penetrate the atmosphere, descending in the hot season below the surface of the earth and again rising in the cold season. This system seems to prove the reality of Ampère's general system of currents extended to the earth and the atmosphere. From this it also follows that the existence of these aërial magnetic currents involves a diminution of temperature with elevation. Consequently these currents are one of the factors, possibly the chief factor, in the thermic system of the globe, so that a fundamental connexion exists between meteorological phenomena and those of terrestrial magnetism.—On the reproduction of alabandine, by M. H. Baubigny. By the process here described the author has obtained some beautiful octahedric crystals, presenting all the characteristics of alabandine (MnS): the same crystalline form, colour, and density, about 4.—Contribution to the study of the alkaloids, by M. Oechsner de Coninck. Having in a previous paper described the reaction of potassa on a combination of the iodide of ethyl with nicotine, the author here confirms by a fresh line of observation the relation of nicotine to the pyridic and dipyridic series.—On some fossil woods found in the Quaternary formations of the Paris basin, by M. Emile Rivière. These specimens were found associated with the animal remains already frequently described by the author. A microscopic study has enabled him to determine three different vegetable species: Palm, Cedroxylon, and Taxodium. The last-mentioned was especially abundant in the Miocene epoch, and appears to be older than the non-fossilized specimens from time to time discovered in the boggy districts of Switzerland.

BERLIN.

Physical Society, April 22.—Prof. Du Bois-Reymond, President, in the chair.—Dr. Gross explained his theoretical views on the heat of solution of magnetised iron, and showed why, in accordance with these, the heat of solution of magnetised iron must be greater than that of unmagnetised. One result of these views was that a piece of magnetised and unmagnetised iron in a conducting fluid capable of dissolving the iron must give a current; this he has already demonstrated two years ago (see NATURE, vol. xxxi. p. 596). The current in such an element as this flows across the fluid from the magnetised to the unmagnetised pole, and is independent of the nature of the magnetisation. The source of the electric current is in this case, according to the views of the speaker, to be sought for in the loss of specific magnetisation which the molecules of iron undergo as they pass from the solid to the fluid condition. Of the various solutions of salts of iron which were used in these experiments, only neutral salts of ferric oxide were found to yield a result, while the salts of ferrous oxide gave no current. The cause of this is, according to the speaker, that only the ferric salts lead to a solution of the magnets. Dr. Nichols has quite recently carried on some experiments on the heat of solution of magnetised iron, and has obtained the same experimental results, namely that the heat of solution of magnetised iron is greater than that of unmagnetised, although he starts with theoretical views respecting the magnetic potential of solid iron and iron in solution which are diametrically opposed to those of Dr. Gross.—The President exhibited a Bourdon's manometer, and explained its use for the measurement of alterations of blood-pressure in living animals. In connexion with this the President gave a full account of the physical portions of the research which Dr. Grunmach has carried out on the influence of elasticity on the rate of progression of the pulse-wave. The most important points of this research have already been communicated in the report of the last meeting of the Physiological Society on April 15 (NATURE, May 12, p. 48).

Physiological Society, April 29.—Prof. Du Bois-Reymond, President, in the chair.—Dr. Onodi, of Buda-Pesth, gave an account of the anatomical investigations which he carried on during his two visits to the Zoological Station at Naples. In the first place he busied himself with the anatomy of the ciliary ganglion, which he examined microscopically in twenty-five different species of Selachians. From what he found in these lower vertebrates, as well as from observations which he had an opportunity of making on the embryos of cartilaginous fishes and chicks, he has come to the conclusion that the ciliary ganglion must be reckoned in with the sympathetic plexus. In addition to the above researches Dr. Onodi was occupied with investigations on the roots of the vagus, and he communicated a number of interesting details on their relations in the Selachians. —Dr. König spoke on Newton's law of colour-mixing, explaining its principle, and illustrating it with the aid of a Newton's colour-chart. He then developed the three propositions which Grassmann has deduced from the Newtonian law, and which, as is well known, are as follows: (1) when two spectral colours are mixed the resulting compound colour is a spectral colour lying between the other two, but mixed with white; (2) when one of the two colours which is being mixed is continuously changing, then the resulting compound colour also changes continuously; (3) similar colours when mixed give similar compound colours. Of these three propositions the first has not been confirmed by later experimental researches, but this does not diminish the value of Newton's law of the mixing of colours: it only becomes necessary to substitute a triangular colour-chart for the circular one put forward by Newton. The second proposition was fully confirmed by experience. The third proposition, which may also be expressed by saying that the compound colour is independent of the intensity of its separate constituents, was not confirmed by experiments. The speaker has alone, and in conjunction with Herr Breduhn, carried out careful measurements on trichromatic and dichromatic eyes, and has always observed a difference in the compound colour as the result of marked differences in intensity of the compounded colours. The validity of Newton's principle in its general form is therefore considerably shaken by this discovery, and must be confined to narrow limits of variations of intensity.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Agricultural Pests of India: Surgeon-General E. Balfour (Quaritch).—Schriften der Naturforschenden Gesellschaft in Danzig (Danzig).—The Storage of Electrical Energy: G. Planté (Whittaker).—Manual of Bacteriology: E. M. Crookshank (Lewis).—Chance and Luck: R. A. Proctor (Longmans)—Manual of Scientific Inquiry, 5th edition: edited by Sir R. S. Ball (Eyre and Spottiswoode).—Elementary Trigonometry: Rev. T. Roach (Clarendon Press, Oxford).—Our Bird Allies: T. Wood (S.P.C.K.).—Dandelion Clocks: J. H. Ewing (S.P.C.K.).—Agriculture in some of its Relations with Chemistry, 2 vols.: F. H. Storer (Low).—The Fungus-Hunter's Guide: W. D. Hay (Sonnenschein).—Forestry of West Africa: A. Moloney (Low).—Shores and Alps of Alaska: H. W. Skarr (Low).—The Races of the British Isles (Quaritch).—Rousdon Observatory, vol. iii., Meteorological Observations for the Year 1886: C. E. Peek.—Transactions of the Seismological Society, vol. x. (Yokohama).—New Commercial Plants and Drugs, No. 10: T. Christy.—Quarterly Journal of the Geological Society, vol. xliii., part 2, No. 170 (Longmans).—Bulletin of the American Geographical Society, 1886, No. 3 (New York).

CONTENTS.

THURSDAY, JUNE 2, 1887.

THE PRE-HISTORY OF THE NORTH.

The Pre-History of the North, based on Contemporary Memorials. By the late Chamberlain J. J. A. Worsaae, &c. Translated, with a brief Memoir of the Author, by H. F. Morland Simpson, M.A. (London: Trübner and Co., 1886.)

IT was a happy thought to offer as a tribute of respect to the memory of one who had done so much for English history and archæology as the late Mr. Worsaae, an English translation of one of the latest, as well as one of the most important, of his archæological essays. The Danish original, of which the volume before us is a translation, is prefaced by an introduction dated December 1880, but a still later work of Worsaae's, and one which may practically be regarded as his last, required no translation, as it was originally written by him in the English language, and published in 1882. This work is entitled "The Industrial Arts of Denmark, from the Earliest Times to the Danish Conquest of England," and forms one of the series of hand-books issued in connexion with the South Kensington Museum. A fellow volume on "The Industrial Arts of Scandinavia in the Pagan Time" appeared in 1883 from the pen of Dr. Hans Hildebrand, of Stockholm.

But not only did one of Worsaae's latest works make its first appearance in the English tongue, but thirty years previously, in 1852, one of his earliest works—indeed one of his most important independent works—appeared in an English garb almost at the same time as it did in the Danish and German languages. This, his "Account of the Danes and Norwegians in England, Scotland, and Ireland," was partly the result of a lengthened stay in the British Isles, and contains a vast store of historical information, to which perhaps too little recourse has been had by English students. It was, however, as an archæologist rather than as an historian that Worsaae merited and obtained the highest distinction. A remarkable linguist, a man of high organizing power, of indefatigable industry, and endowed with the most amiable disposition and the most charming manners, the record of all that he was able to accomplish is absolutely amazing. At the age of eighteen he had already begun to write on archæological subjects, and his important work on the "Primæval Antiquities of Denmark," written by him at the age of twenty-two, and published in 1843, was translated into English by the late Mr. Thomas, and published in 1849. From that date to the day of his death his pen was never idle. This, however, is not the place to attempt an account of Worsaae's contributions to archæology. They have already been recorded by Dr. Sophus Müller in the *Mémoires* of the Society of Northern Antiquaries. Those who, from time to time, have attended the Congresses of Prehistoric Archæology and Anthropology will have been able to form some idea of the versatility of Worsaae's mind and the vast extent of his archæological acquirements; and those who have visited the Museum of Northern Antiquities and the Rosenborg Castle at Copenhagen will have been impressed with his wonderful powers of organization and arrangement. The formation of an historical museum like that of Rosenborg was the result of a happy inspiration, and the difficulties that attended it were by no means slight. Worsaae's own account of them in his "Optegnelser om Rosenborg-Samlingen i 25 Aar" is of the highest interest, though perhaps it would have been wise on the part of his executors to have postponed the publication of this autobiographical memoir for a few years. His relations to the Court of two successive Kings of Denmark were of an intimate kind, and occasionally great tact had to be exercised in carrying out his views as to the requirements of the Rosenborg Museum, which illustrates in such a remarkable manner the successive reigns of the Danish monarchs from the fifteenth century downwards. The estimation in which he was held in his own country was evinced in 1874 by his being appointed Minister of Worship and Public Instruction, but, fortunately for archæological science, his tenure of office was not of long duration.

It is, however, time to turn more immediately to the work the title of which heads this notice. Its object is to trace the prehistoric settlements and the development of civilization in the Scandinavian North; and the phases under which these are considered, and the approximate chronology assigned to them are as follows:—

I. The early Stone Age, at least 3000 B.C., when portions only of Southern Scandinavia seem to have been inhabited.

II. The later Stone Age, about 2000 to 1000 B.C., contemporaneous with the Bronze Age on the shores of the Mediterranean.

III. The early Bronze Age, about 1000 to 500 B.C., when a Stone Age existed to the north, and an Iron Age had already come in to the south.

IV. The late Bronze Age, about 500 B.C. to the time of Christ's birth, when a pre-Roman Age of Iron was developed in Central and Western Europe.

V. The early Iron Age, from A.D. 1 to 450, when bronze was still in use in parts of the Scandinavian peninsula.

VI. The middle Age of Iron, about A.D. 450 to 700, when foreign Romano-German influence predominated.

VII. The later Iron Age, or Viking Period, about A.D. 700 to 1000, when a Stone Age still lingered in the extreme north of Finland and Lapland.

The characteristic relics of all these stages of culture are discussed, and their extension in time and space and the sources whence the various phases of civilization were more immediately derived to the north are indicated With regard to these general considerations not much need be said, unless it be to observe that, with regard to the Danish shell-mounds, or Kjökken-möddings, all antiquaries and naturalists are not of one accord in assigning to them an antiquity beyond that of the ordinary forms of polished stone implements.

Two principal points on which Worsaae insists are the religious origin of many of the deposits of prehistoric periods, and the religious signification of many symbols, which at first sight would seem to be but of a conventional character. The two remarkable golden horns found in 1639 and 1734 at Gallehus, in Slesvig, buried but a few yards apart, belonged apparently to the middle Iron Age;

and though they were stolen and melted down in 1802, faithful representations of them have been preserved. The horns were divided by projecting rings into a series of compartments, in nearly all of which there were groups of human and animal figures accompanied by various symbols. The meaning of these, Worsaae, by the light of northern mythology, has undertaken to interpret; and though it is impossible here even to attempt to reproduce his interpretation, it may fairly be said that it is one that commends itself for its consistency, and which the correspondence between the subjects on the two horns tends strongly to corroborate. There can, as he says, be scarcely a doubt that these gold horns, unique both in size and embellishment, originally formed a pair, and that, like other heathen representations in metal, stone, bone, or wood, they were a sort of sacred picture-book kept in a temple and intended to preserve the kernel of the old theology for the people. Accepting the view of certain marks and symbols being especially those of Thor, Odin, Frey, and other divinities, it is found that not only in later times did the Northmen cling with tenacity to their ancestral reverence for the images and sacred marks of their gods, but that in early times, even in the Bronze Age, traces may be discovered of similar objects of reverence, and that the whole system of northern mythology, such as it existed at the time of its supersession by the Christian faith, was but the development of religious ideas that had subsisted in the same regions in remote prehistoric times. Some speculations with regard to these sacred signs will also be found in our author's "Danish Arts" (p. 65 *et seq.*, p. 114).

The same may be said as to his views with regard to many of the deposits of arms and implements, both of stone and bronze, having originated from religious motives ("D. A." p. 63). It is certainly the case that considerable hoards of large flint axes, crescent-shaped knives of flint, and lance- or spear-heads of the same material, have been found deposited under large stones in fields and bogs, the uniformity of the objects in the deposit raising a presumption that it was not due to the mere hiding away of the private property of one individual, but rather to the fact that some offering to the gods was intended. In the case of hoards of bronze objects, there are some which comprise lumps of rough metal, old and worn-out inplements, and even moulds. Such must, with all probability, be regarded as the property of bronze founders, hidden in the ground for the sake of security, and, from some cause or other, never afterwards recovered by the owners. There are, however, other deposits which, like those of stone already mentioned, would appear to have been due to a religious motive. In some instances the objects have been purposely broken and rendered useless, in the same manner as the gold Gaulish coins found in the Seine, which appear to have been offerings to the *Dea Sequana*, have been so constantly defaced. Such offerings to the divinities of springs and rivers were not unfrequent in Roman times, and continued in vogue even in later ages. The subject of the religious rites of the early prehistoric ages is, then, one the investigation of which has been fairly started by Worsaae, and offers a field in which future research may profitably be prosecuted.

The meaning and derivation of the devices on Scandinavian bracteates, which to a certain extent correspond with the *bullæ* of the Romans, is also discussed in the book before us, which, though extending to little more than 200 pages, contains the result of much thought on the part of the author, and is suggestive of much more for the attentive reader.

It remains to say a few words with regard to the translator, who on the whole has done his work in a satisfactory manner, though probably a more intimate acquaintance with the Danish language and with Scandinavian archæology would have been advantageous. Such terms as grave-heights for barrows, or grave-mounds, and the mention of a Society of Ancient Northern Texts, and the account of a discovery of relics of a primitive Stone Age in ancient chalk deposits under the earth's surface might have been avoided. But the most provoking part of the book is the author's or printer's misplaced economy in the matter of commas. Such a sentence as the following may serve as an example (p. 146): "Many other objects have been discovered in bogs and fields as well as in skeleton-graves from the close of the early Iron Age and from the middle Iron Age in Denmark, as for instance an angel of gold in deacon's robes an armlet with Christian symbols a ball of crystal a jewel carved with Christian Gnostic inscriptions in Greek ("Ablanathanalba," *i.e.* Thou art our Father) brooches mountings with barbarized semi-Christian ornaments, known also in other countries, and many others." But with all these slight defects the work of Worsaae still retains its full value, and English archæologists should gladly welcome its appearance in what Prof. Stephens, of Copenhagen, would call their "mother-tung."

JOHN EVANS.

PROFESSOR STOKES ON LIGHT.

Burnett Lectures. Third Course: On the Beneficial Effects of Light. By G. G. Stokes. (London: Macmillan and Co., 1887.)

THIS volume completes the course of the First Burnett Lecturer on the New Foundation. We have already (vol. xxix. p. 545, and vol. xxxii. p. 361) noticed the first two volumes; and we are now in a position to judge of the work as a whole. But we must first speak of the contents of the present volume.

The author commences by extending the term "Light" to radiation in general, and proceeds to a consideration of the effects which (unlike vision) are not merely beneficial to living things, whether plants or animals, but absolutely essential to their existence. Here, so far at least as matters suitable for an elementary work are concerned, there is not much room for novelty:—for the subject has of late been pretty well threshed out by various writers. Still, the mode of treatment adopted is of interest, especially that of marshalling our reasons for regarding all forms of radiation as due to one and the same agent.

"When we stand by some mighty waterfall, such for example as Niagara, and are struck by the grand exhibition of power that we see before us, we do not perhaps reflect that while it is through light that we are enabled to see what is going on, it is from light also that the energy is derived that we thus see in action."

Next comes a curious suggestion of analogy between the behaviour of fluorescent bodies (which always *degrade* the refrangibility of the light they give off) and the heat-radiation from bodies which have been exposed to sun-light. Sunlight, as it reaches us after passing through the atmosphere, is less rich in ultra-red rays than is the radiation from the majority of terrestrial sources; while the radiation from bodies which have been heated by direct sunlight is entirely ultra-red. Here we have, for the terrestrial atmosphere, the "green-house theory" which, in the second course, was applied to explain some of the singular phenomena exhibited by comets.

This is followed by an extremely interesting discussion of the functions of the colouring-matters of blood and of green leaves:—with the contrasted effects, upon plants, of total deprivation of light, and of continuously maintained illumination. A particularly valuable speculation, as to the probable nature of the behaviour of chlorophyll, is unfortunately too long for extraction.

So far, radiation has been treated without any special reference to vision. But the author proceeds to describe the physical functions and adaptations of the eye:—with particular reference to the arrangements for obviating such of the theoretical defects as, while involved in its general plan, *would also tend to diminish its practical usefulness.* The introduction of this obviously natural proviso, one which we do not recollect having seen prominently put forward till now, exhibits in a quite new light the intrinsic value of those objections to the "argument from design" which have been based upon the alleged imperfection of the eye as an optical instrument.

The analogy of fluorescence is once more introduced, but now for the purpose of suggesting a mechanical explanation of the mode in which the sense of vision is produced. This is brought forward after the modern photo-chemical theory of vision has been discussed. The latter is not altogether dismissed as improbable, but some of the more important difficulties which it raises are pointed out. The triplicity of the colour-sense, and the mechanism of single vision with two eyes, are treated at some length. But throughout this part of the work it is frankly confessed that there are many elementary questions, some of fundamental importance, which we are still unable even approximately to answer.

In his final chapter, the lecturer, in conformity with the terms of his appointment, discusses the *argument from design.* The origin of life, and the origin of species, are boldly (though all too briefly) treated:—next comes the question of the adaptation of physical structure, specially of course that of the eye, to the modes of life and the wants of animals.

"There is some very intimate connection between thinking, as we know it in ourselves, and the condition of the brain. So close is the connection that some have supposed that thinking is a mere function of the material organism, conditioned by nothing more than the motions of the molecules of which that organism consists. But surely this is going far beyond a legitimate inference from the observed facts. The body of a living animal is obedient to the laws of motion, the law of gravitation, and similar laws of the kind which belong to dead matter. But that does not prove that life is nothing more than a process depending on such laws. So if thinking be accompanied, as we know it in ourselves to be accompanied, by a state of activity of the material organism of which the body consists, that does not prove that thinking is nothing more than an action of the material organism. We have seen that life can only proceed from the living; may it not be in a similar manner that mind can only proceed from that which has mind? See what the contrary supposition leads us to. Here is man, in a geological sense a creature but of yesterday, utterly incapable of accounting for his own existence by any play of mere natural forces, and yet ignoring the existence of any mind higher than his own mind, though ready enough to admit the existence of unintelligent law, and that without limitations of time or space."

No higher praise need be bestowed on the scientific part of this third volume than is involved in saying that it is a worthy successor to the other two. Together, they form a singularly instructive, and yet (in the best sense) popular, treatise on a fascinating branch of natural philosophy. Were this their only aim, no one could deny that it has been thoroughly attained.

But their aim is of a loftier character. Here and there throughout the work there have been occasional references to the main purpose which has determined the author's mode of arranging his facts and his deductions from them. In the few closing pages this purpose is fully developed, and a brief but exceedingly clear statement shows at once how much in one sense, and yet how little in another, can be gathered as to the personality and the character of the Creator from a close and reverent study of His works.

These closing pages point out distinctly the danger alike of totally neglecting, and of too exclusively studying, the grandeur of nature. The first holder of this new post has set a noble example to his successors. He has supplied, not only to them but also, and we hope especially, to the rapidly-changing quaternion of neo-teleologists who will soon be set at work in the Scottish Universities, a warning which they will do well to lay to heart:—

"If we confine our attention to the study of nature in all its immensity, our conceptions of its Author are in danger of merging in a sort of pantheistic abstraction, in which the idea of personality is lost."

P. G. TAIT.

OUR BOOK SHELF.

Our Bird Allies. By Theodore Wood. (London: Society for Promoting Christian Knowledge, 1887.)

THE author of this little book holds that no British bird is utterly and wholly destructive, but that the misdeeds of even the most mischievous are atoned for in some degree by services rendered to us in other ways. Birds aid us, he points out, in three ways—first, by acting as scavengers, and destroying putrid matter; secondly, by devouring the seeds of the various wild plants which are so troublesome upon cultivated land; thirdly, and most important, by the slaughter of insects. The limitations of space have prevented Mr. Wood from mentioning all the birds he would have liked to describe, but he has found room for an account of most of the British birds which are especially beneficial. He writes simply, clearly, and with adequate knowledge; and there are probably few farmers who would not profit by studying what he has to say on a subject in which they ought to be strongly interested. He expresses his firm conviction that agriculture, as a profitable undertaking, is absolutely dependent upon the preservation of the "feathered race," and in support of this opinion he has brought together much solid evidence. Two of the best chapters in the book are on the sparrow, which he admits to be, during harvest, an unmitigated nuisance. He thinks, however, that even at such times "the farmer best consults his own interests by merely scaring the bird away in place of destroying it, and that sooner or later he will reap his reward for his wise forbearance."

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. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Thought without Words.

THE following correspondence has passed between Prof. Max Müller and Mr. F. Galton with reference to Mr. Galton's letter on "Thought without Words," printed in NATURE on May 12 (p. 28):—

All Souls' College, Oxford, May 15, 1887.

DEAR MR. GALTON,—I have to thank you for sending me the letter which you published in NATURE, and in which you discuss the fundamental principle of my recent book on the "Science of Thought," the identity of language and reason. Yours is the kind of criticism I like—honest, straightforward, to the point. I shall try to answer your criticism in the same spirit.

You say, and you say rightly, that if a single instance could be produced of a man reasoning without words, my whole system of philosophy would collapse, and you go on to say that you yourself are such an instance, that you can reason without words.

So can I, and I have said so in several passages of my book. But what I call reasoning without words is no more than reasoning without pronouncing words. With you it seems to mean, reasoning without possessing words. What I call with Leibniz, symbolic, abbreviated, or hushed language, what savages call "speaking in the stomach," presupposes the former existence of words. What you call thinking without words seems to be intended for the thinking of beings, whether men or animals, that possess as yet no words for what they are thinking.

Now let us try to understand one another; that is to say, let us define the words we are using. We both use thinking in the sense of reasoning. But thinking has been used by Descartes and other philosophers in a much wider sense also, so as to include sensation, passions, and intuitive judgments, which clearly require no words for their realization. It is necessary therefore to define what we mean by thinking, before we try to find out whether we can think without words. In my book on the "Science of Thought" I define thinking as addition and subtraction. That definition may be right or wrong, but every writer has the right, nay the duty, I should say, to explain in what sense he intends to use certain technical terms. Though nowadays this is considered rather pedantic, I performed that duty on the very first page of my book, and it seems somewhat strange that a reviewer in the *Academy* should accuse me of not having defined what I mean by thinking, for most reviewers look at least at the first page of a work which is given them to review.

Now, the cases which you mention of wordless thought are not thought at all in my sense of the word. I grant that animals do a great deal of work by intuition, and that we do the same, nay that we often do that kind of work far more quickly and far more perfectly than by reasoning. You say, for instance, that you take pleasure in mechanical contrivances, and if something does not fit, you examine it, go to your tools, pick out the right one, set to work and repair the defect, often without a single word crossing your mind. No doubt you can do that. So can the beaver and the bee. But neither the beaver nor the bee would say what you say, namely that in doing this "*you inhibit any mental word from presenting itself.*" What does that mean, if not that the mental words are there, the most complicated thought-words, such as *tool, defect, fit*, are there? only you do not pronounce them, as little as you pronounce "two shillings and sixpence," when you pay a cabman half-a-crown.

The same applies to what you say about billiards and fencing. Neither cannoning nor fencing is thinking. The serpent coiling itself and springing forward and shooting out its fangs does neither think nor speak. It sees, it feels, it acts, and as I stated on p. 8 of my book, that kind of instantaneous and thoughtless action is often far more successful than the slow results of reasoning. Well do I remember when I was passing through my drill as a Volunteer, and sometimes had to think what was right and what was left, being told by our sergeant, "Them gentlemen as thinks will never do any good." I am not sure that what we call genius may not often be a manifestation of our purely animal nature—a sudden tiger's spring, rather than *une longue patience.*

It is different, however, with chess. A chess-player may be very silent, but he deals all the time with thought-words or word-thoughts. How could it be otherwise? What would be the use of all his foresight, of all his intuitive combination, if he did not manipulate with king, queen, knights, and castles? and what are all these but names, most artificial names too, real agglomerates of ever so many carefully embedded thoughts?

An animal may build like the beaver, shoot like the serpent, fence like the cat, climb like the goat; but no animal can play chess, and why? Because it has no words, and therefore no thoughts for what we call king, queen, and knights, names and concepts which we combine and separate according to their contents; that is, according to what we ourselves or our ancestors have put into them.

You say, again, that in algebra, the most complicated phase of thought, we do not use words. Nay, you go on to say that in algebra "*the tendency to use mental words should be withstood.*" No doubt it should. The player on the pianoforte should likewise withstand the tendency of saying, now comes C, now comes D, now comes E, before touching the keys. But how could there be a tendency to use words, or, as you say in another place, "*to disembarrass ourselves of words,*" if the words were not there? In algebra we are dealing, not only with words, but with words of words, and it is the highest excellence of language if it can thus abbreviate itself more and more. If we had to pronounce every word we are thinking, our progress would be extremely slow. As it is, we can go through a whole train of thought without uttering a single word, because we have signs, not only for single thoughts, but for whole chains of thoughts. And yet, if we watch ourselves, it is very curious that we can often feel the vocal chords and the muscles of the mouth moving, as if we were speaking; nay, we know that during efforts of intense thought, a word will sometimes break out against our will; it may be, as you say, a nonsense word, yet a word which, for some reason or other, could not be inhibited from presenting itself.

You say you have sometimes great difficulty in finding appropriate words for your thoughts. Who has not? But does that prove that thoughts can exist without words? Quite the contrary. Thoughts for which we cannot find appropriate words are thoughts expressed as yet by inappropriate, very often by very general, words. You see a thing and you do not know what it is, and therefore are at a loss how to call it. There are people who call everything "that thing," in French "*chose,*" because they are lazy thinkers, and therefore clumsy speakers. But even "thing" and "*chose*" are names. The more we distinguish, the better we can name. A good speaker and thinker will not say "that thing," "that person," "that man," "that soldier," "that officer," but he will say at once "that lieutenant-general of Fusiliers." He can name appropriately because he knows correctly, but he knows nothing correctly or vaguely except in a string of names from officer down to thing. Embryonic thought, which never comes to the birth, is not thought at all, but only the material out of which thought may spring. Nor can infant thought, which cannot speak as yet, be called living thought, though the promise of thought is in it. The true life of thought begins when it is named, and has been received by baptism into the congregation of living words.

You say that "after you have made a mental step, the appropriate word frequently follows as an echo; as a rule, it does not accompany it." I know very well what you mean. But only ask yourself what mental step you have made, and you will see you stand on words; more or less perfect and appropriate, true; but nevertheless, always words. You blame me for having ignored your labours, which were intended to show that the minds of everyone are not like one's own. You know that I took a great deal of interest in your researches. They represented to me what I should venture to call the dialectology of thought. But dialects of thought do not affect the fundamental principles of thinking; and the identity of language and reason can hardly be treated as a matter of idiosyncrasy.

You also blame me for not having read a recent book by Monsieur Binet. Dear Mr. Galton, as I grow older I find it the most difficult problem in the world, what new books we may safely leave unread. Think of the number of old books which it is not safe to leave unread; and yet, when I tell my friends that in order to speak the *lingua franca* of philosophy, they ought at least to read Kant, they shrug their shoulders, and say they have no time, or, *horribile dictu*, that Kant is obsolete. I have, however, ordered Binet, and shall hereafter quote him as an authority. But who is an authority in these days of anarchy? I quoted the two greatest authorities in Germany and England in support of my statement that the genealogical descent of man from any other known animal was as yet *unproven*, and I am told by my reviewer in the *Academy* that such statements "deserve to be passed over in respectful silence." If such descent were proved, it would make no difference whatever to the science of thought. Man would remain to me what he always has been, the perfect animal; the animal would remain the stunted man. But why waste our thoughts on things that may be or may not be? One fact remains, animals have no language. If, then, man cannot think, or, better, cannot reason, without language, I think we are right in contending that animals do not reason as man reasons;—though, for all we know, they may be all the better for it.

Yours very truly,
F. MAX MÜLLER.

Francis Galton, Esq., F.R.S.

42 *Rutland Gate, S.W., May* 18, 1887.

DEAR PROFESSOR,—Thank you much for your full letter. I have not yet sent it on to NATURE because it would have been too late for this week's issue, and more especially because I thought you might like to reserve your reply, not only until you had seen my own answer to what you have said in it, but also until others should have written, and possibly also until you had looked at Binet, and some of the writers he quotes. So I send you very briefly my answer, but the letter shall go to NATURE if you send me a post-card to send it.

In my reply, or in any future amplification of what is already written, I should emphasize what was said about fencing, &c., "*with the head*," distinguishing it from intuitive actions (due, as I and others hold, to inherited or personal habit).

The inhibition of words in the cases mentioned was, I should explain, analogous to this:—There are streets improvements in progress hereabouts. I set myself to think, by mental picture only, whether the pulling down of a certain tobacconist's shop (*i.e.* its *subtraction* from the row of houses in which it stands) would afford a good opening for a needed thoroughfare. Now, on first perceiving the image, it was associated with a mental perception of the *smell* of the shop. I inhibited that mental smell because it had nothing to do with what I wanted to think out. So words often arise in my own mind merely through association with what I am thinking about; they are *not* the things that my mind is dealing with; they are superfluous and they are embarrassments, so I inhibit them.

I have not yet inquired, but will do so, whether deaf-mutes who had never learnt words or any symbols for them, had ever been taught dominoes, or possibly even chess. I myself cannot conceive that the names—king, queen, &c.—are of any help in calculating a single move in advance. For the effect of many moves I use them mentally to record the steps gained, but for nothing else. I have reason to believe that not a few first-rate chess-players calculate by their mental eye only.

In speaking of modern mental literature, pray do not think me so conceited as to refer to my own writings only. I value modern above ancient literature on this subject, even if the modern writers are far smaller men than the older ones, because they have two engines of research which the others wanted:—

(1) Inductive inquiry, ethnological and other. The older authorities had no vivid conception of the different qualities of men's minds. They thought that a careful examination of their own minds sufficed for laying down laws that were generally applicable to humanity.

(2) They had no adequate notion of the importance of mental pathology. When by a blow, or by a disease, or, as they now say, by hypnotism, a whole province of mental faculties can be abolished, and the working of what remains can be carefully studied, it is now found that as good a clue to the anatomy of the mind may be obtained as men who study mangled limbs, or who systematically dissect, may obtain of the anatomy of the body.

I add nothing about the advantage to modern inquirers due to their possession of Darwinian facts and theories, because we do not rate them in the same way.

Very truly yours,
FRANCIS GALTON.

Professor Max Müller.

Oxford, May 19, 1887.

MY DEAR MR. GALTON,—If you think my letter worth publishing in NATURE, I have no objection, though it contains no more than what anybody may read in my "Science of Thought."

Nothing proves to my mind the dependence of thought on language so much as the difficulty we have in making others understand our thoughts by means of words. Take the instance you mention of a shop being pulled down in your street, and suggesting to you the desirability of opening a new street. There are races, or, at all events, there have been, who had no name or concept of shop. Still, if they saw your shop, they would call it a *house*, a *building*, a *cave*, a *hole*, or, as you suggest, a chamber of smells and horrors, but at all events a *thing*. Now, all these are names. Even "*thing*" is a name. Take away these names, and all definite thought goes; take away the name *thing*, and thought goes altogether. When I say word, I do not mean *flatus vocis*, I always mean word as inseparable from concept, thought-word or word-thought.

It is quite possible that you may *teach* deaf-and-dumb people dominoes; but deaf-and-dumb people, left to themselves, do not invent dominoes, and that makes a great difference. Even so simple a game as dominoes, would be impossible without names and their underlying concepts. Dominoes are not mere blocks of wood; they signify something. This becomes much clearer in chess. You cannot move king, or queen, or knight as mere dolls. In chess, each one of these figures can be moved according to its name and concept only. Otherwise chess would be a chaotic scramble, not an intelligent game. If you once see what I mean by names, namely that by which a thing becomes *notum* or known, I expect you will say, "Of course we all admit that without a name we cannot really know anything."

I wonder you do not see that in all my writings I have been an evolutionist or Darwinian *pur sang*. What is language but a constant becoming? What is thought but an *Ewiges Werden?*

Everything in language begins by a personal habit, and then becomes inherited; but what we students of language try to discover is the first beginning of each personal habit, the origin of every thought, and the origin of every word. For that purpose ethnological researches are of the highest importance to us, and you will find that Kant, the cleverest dissector of abstract thought, was at the same time the most careful student of ethnology, the most accurate observer of concrete thought in its endless variety. With all my admiration for modern writers, I am in this sense also a Darwinian that I prefer the rudimentary stages of philosophic thought to its later developments, not to say its decadence. I have learnt more from Plato than from Comte. But I have ordered Binet all the same, and when I have read him I shall tell you what I think of him.

Yours very truly,
F. MAX MÜLLER.

A Use of Flowers by Birds.

SOME years ago you allowed me to describe in NATURE the pretty doings of a pair of goldfinches, who, having built their nest on a bough overhanging a garden path, proceeded to make it more like the sky above, and therefore less visible from below, by hanging it round with wreaths of forget-me-nots.

This year, in the same garden, some sparrows have shown equal ingenuity. They began a nest in a *Pyrus japonica* against the white house, whilst the tree was still almost bare of leaves. Not wishing for the noise and dirt so near the windows, I removed it, and they began another; again it was removed, and this time, though apparently little more than a flat beginning, it had eggs upon it. They tried again, and on removing it the third time I found that the birds were overlaying it on all sides with the flowers of some sweet Alyssum that was growing below; the intention being, evidently, to render it more like the background of white wall, and therefore less conspicuous.

Sidmouth. J. M. H.

Earthquakes and the Suspended Magnet.

DURING the afternoon of May 3 at Lyons, N.Y., a peculiar quivering motion of the suspended magnet was noted, especially at about 1 o'clock p.m., and a strong westward deflection continued during the afternoon. Similar phenomena have been noted repeatedly when earthquakes were in progress, in this case the shock being quite severe, and occurring at 3.8 p.m. at El Paso, Texas. M. A. VEEDER.

Lyons, N.Y., May 4

Units of Weight, Mass, and Force.

THE letter of Prof. Greenhill (NATURE, vol. xxxv. p. 486) is both timely and suggestive. Herbert Spencer's chapter on space, time, matter, motion, and force, supplemented by his chapter on the persistence of force, in "First Principles of Philosophy," gives all that can be desired by the student for a complete comprehension of the subject. One who assimilates the basic truths there so clearly given need never be perplexed by any statement found in the mechanical and mathematical text-books. It is simply impossible to use language in regard to these matters without employing expressions that are true only in a certain sense. We say that "the sun rises" and "the sun sets," and that "the heavens revolve." If these words are used to indicate the cause of the progressive shadows on a sun-dial, or the time of day, they serve a practical need as well as if they were true. But a student who should infer the constitution of the solar system from such phrases would go far astray.

When the significance of Spencer's explanation of motion is grasped, a great part of the ambiguity will have vanished. We constantly think of motion as an entity, which is a pure delusion. We also say of force that it is the cause of motion. Nothing can be more untrue. Force is the cause of change of motion only. There is not a conceivable difference between rest and motion otherwise than as the expression of a relation. Whether a body be at rest or in motion depends wholly upon the body to which it is related.

When the student sees that motion is no entity, and is familiar with the process by which the conceptions of matter, force, space, and time, are built up from sensations, he will be in no danger of mistaking the sense in which certain text-book statements are to be taken, much less will he be captured by those in which the errors are unpardonable. I. LANCASTER.

Chicago, Ill., April 28.

WITH regard to Mr. Geoghegan's letter in your issue of April 7 (vol. xxxv. p. 534), my experience in teaching physics long ago led me to the same conclusions. For three years I have used in my classes in this the oldest existing University in Ontario, and with the greatest advantage, the terms *tach*, *gram-tach*, *prem*, and *dyntach* for the units of velocity, momentum, pressure-intensity, and rate of working respectively, in the C.G.S. system of units. These may be found in my "Introduction to Dynamics," which was printed last year for my junior class. *Prem* was chosen after failure to get a euphonious monosyllable from the Greek. A name for the unit of acceleration I have not found to be necessary. *Vel* seems to me to be a good word for the unit of velocity in the F.P.S. system of units, but, for fear of hanging on a sour apple-tree, I would shudder to mention *pound-vel* and *poundal-vel*. The term *squeeze* would be suitable in several respects for a *poundal per square foot*, but in mixed classes, such as we have here, it might lead to disorder.

D. H. MARSHALL.

Queen's University, Kingston, Ontario, Canada, April 27.

Remarkable Phenomenon seen on April 26, 1887.

A PHENOMENON was seen here this evening quite distinct from anything I have before observed. It was an exact copy of streams of aurora borealis rising from a low arch, but instead of being in the northern heavens it was near the south horizon. The sky was cloudless, except a long thundercloud which extended from near south-south-west to almost south-south-east, the upper portion of this cloud being about 12° above the horizon. From this cloud issued from one to three streams of conspicuous white light, the north-easterly stream being the largest and brightest, and this continued visible from 9.40 until 10.5 (the others were only seen for five minutes). The streams were at an angle of about 53°, and moved slowly easterly (the cloud moving in the same direction). The longest stream reached an altitude of 25°, and at 10 o'clock exactly (G.M.T.) the base was immediately over the Avonmouth Lighthouse. The light of the streams was more persistent and less flickering than is usually the case with aurora borealis.

There was also a confused luminosity behind the cloud, which varied considerably in brightness; this made the outline of the cloud at one time distinctly visible, and at another scarcely discernible; this also gave the clouds a black appearance. After 10 p.m. other clouds rose above the cumulo-stratus, and the streams became hid. Three hours afterwards there was a snow-storm, and the ground was white till 7 a.m. Reports from Somerset, Dorset, and Devon would be valuable.

Shirenewton Hall, near Chepstow. E. J. LOWE.

Pear-shaped Hailstones.

ARE pear-shaped hailstones as uncommon as some of your correspondents suppose?

We have had here to-day a succession of heavy showers of rain and hail together, the hailstones being small, but many of them pear-shaped, and the rest of shapes which might easily have been derived from that form by attrition or partial melting.

About half past six this evening we had a storm of hail only, heavier than any that preceded it, in which nearly or quite all the stones were pear-shaped, from a fifth to a third of an inch in diameter. B. WOODD SMITH.

Penmaenmawr, N. Wales, May 20.

P.S.—May 21. At 9.30 this morning we had another shower of hail and rain, in which the stones showed no sign of any pear-shape, but were of irregular rounded forms.

"A Junior Course of Practical Zoology."

IN the review of Messrs. Marshall and Hurst's book referred to in my friend Prof. Bourne's letter, I sought to compare that work with others devoted to the familiar type-system, to which alone the words "all other books current" were meant by me to refer, to the exclusion of general text-books such as those from which he quotes. I admit that I might have made my meaning somewhat plainer than I did, and would beg to be allowed to state that I had it in my mind, at the time of writing, to refer the reader to the impartial statements made on the subject in question by Prof. Rolleston in his "Forms of Animal Life"; the first of the series of what we are now pleased to term "type" or "junior course" books.

With respect to my critic's second objection, I would ask the readers of NATURE to judge for themselves how far the quotations which he so skilfully weaves into his letter do justice to my contention. His view is, like my own, but an expression of opinion, and time alone can show which of the two will come nearest the truth. G. B. H.

South Kensington.

Bishop's Ring.

THE letter by Prof. G. H. Stone in NATURE, vol. xxxv. p. 581, is interesting, as showing the disappearance of "Bishop's ring" in Colorado. It has not wholly disappeared here, being still plainly visible about sunset. In the middle of the day, however, I have rarely seen any trace of its red colour since May last year; but up to that time, although growing much fainter, it was still frequently plain here, and I also saw it in the south of England, both in May and June 1886, but only feebly. Since then, when there has been a slight tinge of red, it has usually appeared of a dirty brown colour, very different from what "Bishop's ring" used to be, and I have thought that often it has not been in the upper atmosphere, but at a lower altitude, and most visible when there has been more or less smoke; so that it seemed not improbable the smoke was the cause of it. Has anyone else noticed such a phenomenon connected with smoke? "Bishop's ring," as still seen at sunset, is evidently not caused by smoke, but doubtless arises from the same circumstances as made it so conspicuous an object at its first appearance in November 1883, and gradually less so since.

The whitish wisps occurring in and near the ring about sunrise and sunset continued visible at intervals, and varying greatly in distinctness, up to the 31st ult. I have not seen them since, but they have been invisible for longer periods before. T. W. BACKHOUSE.

Sunderland.

A REVIEW OF LIGHTHOUSE WORK AND ECONOMY IN THE UNITED KINGDOM DURING THE PAST FIFTY YEARS

I.

IT may be useful to recapitulate very briefly the various steps of progress in this important branch of engineering and optical enterprise since the beginning of the Queen's reign. And a few words may be added on the statistics and economics of the subject.

A lighthouse or lightship is naturally to be considered under four heads: (1) tower or hull and its lantern; (2) optical apparatus and its mechanical accessories; (3) lamps and illuminants; (4) auxiliary sound signals.

In 1837 a high degree of excellence had been attained in the first division, at least as regards stone towers and wooden vessels, but in the others *stare super antiquas vias* was a practice largely submitted to. The number also of established lights was comparatively small, about seventy of all kinds being in England and Wales, less than one-fifth of the present number. France, where there had been from 1824 to 1827 an active movement in the direction of coast illumination, possessed in 1836 about 100 lights. In 1822, and again in 1834 a Parliamentary Committee had inquired into the character and management of our lighthouses, with results to be noticed by and by.

In 1837 the old working Phari of Greece, Carthage, and Rome, from Alexandria to the Pillars of Hercules, had long since disappeared, leaving only a few vestiges, chiefly on the shores of France, Spain, and Britain. Of modern times the most notable towers were, on the Continent, the imposing Cordouan at the mouth of the Garonne (1610), and the tourist-haunted Lanterna of Genoa, the latter still being the tallest lighthouse structure in the world; while at home Smeaton's Eddystone (1759), prototype of British towers, the Bell Rock (1811), the Tuskar (1815), and the Carlingford, on Haulbowline Rock (1823), stood as the most striking examples of such edifices. But in 1838 the great tower of Skerryvore was begun by Alan Stevenson, whose father, Robert, had built the Bell Rock Lighthouse. These accomplished engineers have respectively left a graphic and instructive narrative of their work, which may be fitly classed with Smeaton's memorable account of the third Eddystone.

Skerrymhore or Skerryvore (*ysgar-mawr* = great divided cliff, or rocky islet, as in *scar*, or the hills Skerid Fawr, and Skerid Fach) is a nearly submerged reef adjacent to the Island of Tyree, exposed to the full force of the Atlantic, and surrounded by innumerable rocky points constituting "foul ground" along a line of seven miles. It is thus perhaps the most dangerous of all the *skerries* in British waters, and differs essentially from the Eddystone, which, though formidable in itself, rises from the deep sea, and can be approached more nearly in calm weather. Obviously, then, the 72 feet of elevation of the Eddystone lantern-centre, and even the 93 feet of the Bell Rock, could not afford the necessary range to a light intended to give timely notice to mariners of the outlying perils, and a height of 130 feet was adopted for the Skerryvore edifice, which, permitting one of 150 feet from focal plane to high water, insured a geographical horizon of about fourteen nautical miles, or eighteen miles to a vessel's deck. The mean diameter given to this tower was 29 feet, slightly greater than that of Bell Rock, that of Smeaton's Eddystone being 21 feet. The cubic contents are more than four times those of the Eddystone, and more than double those of Bell Rock. There are ten stories below the lantern, for water, fuel, keepers' rooms, and other purposes. The work was completed early in 1844, after extraordinary difficulties and perils, and it is a splendid monument to the energy and skill of Alan Stevenson. Its cost was £87,000.

Yet perhaps some of the towers of the great nation which charges no dues for its lights, but presents them a noble offering to the world, are fully as remarkable. Minot's Ledge (1859) on the Massachusetts coast, and Spectacle Reef, Lake Huron, are examples. The latter structure was begun in 1871, and, though for an inland water, cost £60,000, the special difficulty having been ice, and the laying, by means of a cofferdam, of the lower courses of masonry on a jagged slope of dolomitic limestone 12 feet under water, and eleven miles from land, like the Eddystone. So in the case of Minot's Ledge Tower, the foundations of which were laid on a rock barely visible at extreme low tide, and in the full swell of the ocean, the distinguished engineer General Alexander was able to secure but thirty hours of work in the first year, and 157 in the second.

The Bishop Lighthouse, on the south-westernmost rock of the Scilly Islands, was completed in 1858 at a cost of £34,560. After a quarter of a century's service it has been found expedient to increase the height, and to erect a more powerful optical apparatus, which will be ready during the present year. Other notable towers of the Trinity House are the Smalls (entrance of Bristol Channel), the Hanois (west end of Guernsey), the Wolf, and the new Longships; all being generally alike in design, and not differing widely in dimensions and cost. The Wolf Tower received its light in January 1870, having been begun in March 1862. It was planned by Mr. James Walker, then Engineer to the Trinity House, but carried out by his successor, Mr., now Sir James, Douglass, and by his brother, Mr. William Douglass. This lighthouse is situated seventeen miles from Penzance, and twenty-three west-north-west of the Lizard. It has a mean diameter of nearly 30 feet, and a total height of 110 feet from high water to lantern-centre, being solid for 39 feet from the base, and containing 44,500 cubic feet of granite, weighing 3300 tons. Each face-stone is dovetailed vertically and also horizontally—the latter was not done in the Eddystone tower—and the courses further secured together by metal bolts. Roman cement was used for the work below water, and Portland cement for that above, the whole mixed with a peculiar granitic sand from a Cornish tin-mine. Very great difficulty, as with all these exposed towers, was experienced in the erection of the Wolf and the new Longships, owing to the terrific seas that assaulted the rocks. The Longships, so conspicuous an object from the Land's End, and so well known from Mr. Brett's luminous pictures, with an original elevation of 79 feet above high water, was so drowned by the waves that the character of the light could hardly be discerned, and a granite column of 110 feet was adopted.

In Scotland the sea-tower of Dubh Artach, or, less correctly, Dhu Heartach (1872), and in the Isle of Man that on the Chicken Rock (1875), may be named and the list of the chief structures of this type may be summed up in the Eddystone of Sir James Douglass, from which a light was first shown in 1882. The rapid disintegration of that part of the reef on which Smeaton's tower stood made it absolutely clear in 1877 that a new tower must be built if a disaster (such as that which befell the Calf Rock Light a few years later) were to be avoided. It had been suggested to destroy the reef by blasting, as it had been persistently suggested since 1844 to remove the Goodwin Sands. But in either case not only would such a thing be impracticable on account of the enormous expenditure of money and time; but also there is a positive advantage for navigation in retaining a lighthouse or a lightship on these sites. The new Eddystone tower replacing that of Smeaton, which had made the name memorable for 123 years, has an elevation from lantern-centre to high water of 133 feet, commanding a horizon of seventeen and a half nautical miles (to a vessel's deck). The corresponding horizon of the old tower was about fourteen miles, with an elevation of 72 feet. The extended range is ample for all maritime needs. The structure contains 63,020 cubic feet, or 4668 tons, of Cornish and Dalbeattie granite. The tower springs from a solid cylinder of granite about 45 feet in diameter and 20 feet high, set indissolubly on the rock. The mean diameter is about 30 feet. It is solid up to 25½ feet above high water, except as regards space for a water-tank which holds 3500 gallons. It has seven chambers for stores and keepers' use, and a room for exhibiting a small light 15° in azimuth to denote a danger called Hand Deeps. These chambers all have a diameter of 14 feet. There are besides two others below them of less size. Two massive fog-bells are fixed under the lantern-gallery. Very little inflammable material is used. The doors, window-frames, and other fittings are of gun-metal, and every modern resource has been employed to make the building weather-proof and enduring, and to insure the comfort of the three men confined in it, and the unfailing exhibition of the powerful light which crowns it. The time occupied in the work was about three years and a half, the cost less than £80,000.

It is unnecessary to refer to the numerous land towers erected by lighthouse authorities during the half century, because these, being reared for the most part on cliffs, and little exposed to stress of sea, present no difficulty of construction or novelty of type.

All the towers hitherto named are of stone, but iron has not been overlooked as in some circumstances a practicable material for a sea structure. The designs of the late Mr. Alexander Gordon, C.E., in cast and wrought iron, for the towers of several West Indian and South African lights are well worthy of attention, as are also those of Messrs. Grissell for Russia, &c.; and, more recently, the tall iron towers designed and made by Messrs. Chance, of Birmingham, for Australasian sites, are not less remarkable. At home, the Fastnet may be taken as a successful instance of the application of iron. The rock so called is four miles south-west by west of Cape Clear, and has been symbolised as the "Tear-drop of Ireland," being the "last of the old country seen by emigrants." This tower was begun in 1848, and completed in 1853. It is composed of a casing of cast-iron plates with a central column and girder floors, forming five chambers 12 feet high. The lowest story is partly filled in with masonry, leaving space for a coal-vault. The other stories are lined with brick. The internal diameter of the tower is 12 feet, the height from base to gallery 64 feet. The focal plane is 148 feet above the sea. The cost of the work was £19,000. The engineer and designer was the late Mr. George Halpin.

The lightships established in British waters are of great interest. There are now about seventy-five, sixty being on the English coast, of which the larger number date from since 1837. Several of these peculiarly English vessels were placed on their stations in the last century, the historical Nore, for instance, in 1732.

Iron had been in use for light-vessels in the Mersey before 1856. In 1843 it had been discussed by the Trinity House as a possible material, but was not then deemed desirable. The first Trinity iron vessel was stationed in 1857 on the Goodwin Sands, the next in Cardigan Bay in 1860. The usual length of a Trinity lightship is 80 feet when constructed of wood, and about 90 feet when of iron, the width is 21 feet, the average tonnage 155 to 160 tons when of wood, and 180 tons when of iron. The focal plane of a light is generally 38 feet above high water. The cost averages £3600 for wood, and £5000 or £5500 for iron. An immense service is rendered by these modest and vigilant sentinels of the deep which surround our coasts in positions impossible for a lighthouse, and for the most part close to the dangers of which they give warning, or to the channel of approach which they indicate. It has long been proposed to connect these vessels, as also rock and pile lighthouses, with the shore, and (in some cases) with one another, by an electric cable; and a Committee is now engaged on the subject. In this way communications may be made as to the safety and requirements of the station, and as to the passing shipping, and to wrecks and other casualties, though it is doubtful whether reports on the last heads are a proper addition to the functions of a light-keeper, or one that is likely to be satisfactory in the result to the persons concerned.

A curious and ingenious plan of combining the lighthouse with the lightship was conceived by Mr. George Herbert in 1853, and much discussed and recommended at the time. On the assumption that the form of a ship is not the best for a stationary floating body, he proposed a circular vessel, moored from its centre of gravity, and supporting a central tower of about 40 feet high, with lantern, gallery, &c., of the usual kind. A candlestick set in a wash-tub may not be too familiar an illustration. A position north of the Stones Rock, on the Cornish coast, was suggested, at an expense of about £10,000. The Trinity House did not adopt this plan, but in 1859 two beacon buoys on the same principle were successively placed off the Stones, and after a few weeks' service were driven from their moorings and destroyed.

The use of screw piles for the foundation of a lighthouse in sand was first demonstrated at Fleetwood in 1840, and Maplin in 1841, and afterward at the Chapman, Gunfleet, and other stations. The method is that of Alexander Mitchell, improved by Mr. George Wells, who has erected many such structures in various shallow seas.

The lantern, that is the framework of glass and metal, which contains the illuminating apparatus, whether in land or floating lights, has been much modified during the past fifty years. At the accession the lantern of a first-class light was from 10 to 12 feet in diameter, with perhaps 8 feet of glazing in polygonal panes. The bars were heavy and intercepted much light, the ventilation defective, the construction more or less weak and unequal. Successive improvements have been effected by the engineers of the Trinity House and Northern Lights Commission, and by Chance, of Birmingham. In its highest type, that of Sir James Douglass, as in the Bishop Rock example, the lantern of to-day for a first order lighthouse is well worthy of the perfected optical instrument which it protects. It has a diameter of 14 feet between the glass surfaces, a height of glass of 15 feet, and a height from base to vane of about 32 feet. It is cylindrical in form, with solid gun-metal bars, helically inclined and of wedge-like section towards the flame, comprising sixty-four openings of diamond and sixty-four of triangular shape. The polished plate-glass is three-eighths of an inch thick, and bent accurately to fit in these openings. Nine-tenths of the incident light from the lamp is transmitted through this glass. Not more than $\frac{3}{100}$ of light is stopped by the lantern framing. Thus the maximum of stability and the minimum obstruction of the rays are obtained. At the same time every expedient to promote perfect ventilation, from the tubes of Faraday to the longitudinal valves and the roof-cylinders of Douglass, has been adopted, this being indispensable for the combustion of the great concentric flames now employed. The dome is of rolled copper, the plinth or base of massive cast-iron lined with iron sheets. The cost of such a lantern is about £1700. The lantern of recent lightships has been treated in the same way, having regard to its lightness, mobility, and smaller dimensions. The diameter has been extended to 8 feet, the height of plate-glass to 4 feet, the cylindrical form substituted for every other.

It does not seem possible to construct lighthouse towers and lanterns of better designs and materials than those which have been described. An important amplification of the dimensions may, however, be resorted to in the future to meet the increasing radii of the lenticular apparatus, and the increasing size and height of the central flames. This is on the assumption that electricity does not displace petroleum and gas as illuminants. It may be counted as an additional claim of the arc to be the light of the future that it requires no apparatus larger than Fresnel's first order of 920 millimetres focal distance, and that therefore no lantern exceeding 14 feet in diameter with 10 feet of glazing, and no tower with a diameter of platform greater than 23 feet, would certainly be needed. The merits and prospects of the rival illuminants will be discussed in a subsequent article.

J. KENWARD

(To be continued.)

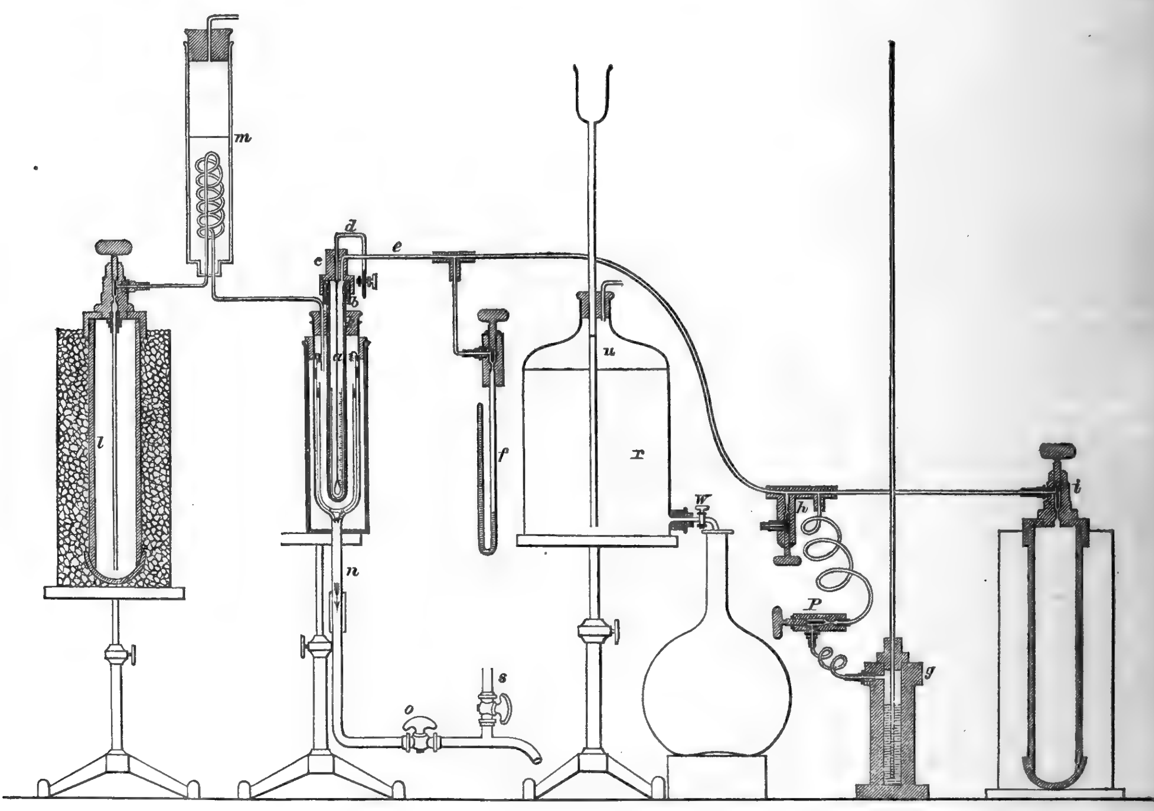
CONDENSATION OF GASES.

AMONG the numerous subjects which have engrossed the attention of the knowledge-seekers of the present century, probably none have surpassed in fascination and in the wealth of results which have followed persistent effort the question of the possibility of liquefying those gases which for ages had been considered permanent. Immediately after that epoch-making period in chemistry and physics, when Faraday, following in the footsteps of Northmore who in 1806 had succeeded in liquefying chlorine, announced to the world the fruitful results of his experiments upon the liquefaction of gaseous sulphurous, carbonic, and hydrochloric acids, nitrous oxide, cyanogen, and ammonia, came a long interval, during which all attempts to induce hydrogen, oxygen, nitric oxide, marsh gas, and carbon monoxide to take up the liquid state yielded little more than negative results, and the subject appeared almost without hope. When one looks back to the end of the year 1877 and remembers the thrill of excitement which ran through the civilized world when the double announcement was made by the French Academicians that oxygen had been independently liquefied by Cailletet and Pictet, and then, in the mind's eye, reverts to the long years of trial and experiment during which these and other workers were slowly but surely building up future success on present failure, one cannot but be cheered by the thought that patient work inevitably brings its own reward. The fundamental principle upon which both based their experiments was, that the gases must be simultaneously exposed to very high pressures and to temperatures lower than their critical points. Pictet, whose apparatus was a triumph of mechanical skill, evolved his gas to be liquefied from a strong wrought-iron cylinder, from whence it passed into a closed copper tube surrounded by a cold bath of rapidly evaporating liquefied carbon dioxide, which reduced the temperature to $-130°$ C. Cailletet arrived at the same end by using a hydraulic press to compress his gas, but instead of using a very cold bath he caused the gas to effect its own reduction of temperature by suddenly releasing the pressure, causing rapid evaporation, and hence such a considerable cooling that the gas condensed in drops of liquid. Pictet, on January 10, 1878, further succeeded in crowning his results by liquefying hydrogen at a pressure of 650 atmospheres and at a temperature of $-140°$, and finally, on releasing the pressure, by actually solidifying the hydrogen, which fell "like so many drops of steel" upon the ground.

But now came the question of the possibility of producing still lower temperatures, so as to effect the same result at correspondingly lower pressures, and so successful have efforts in this direction been that the more permanent gases have at last been liquefied at pressures nearly approaching atmospheric, and retained in the liquid form under even less than atmospheric pressure. This is a great leap in advance, for it not only enables us to determine the boiling-points of the liquefied gases at ordinary pressure, but also to determine their densities in strictly comparable numbers. This happy consummation we mainly owe to the untiring efforts of Dr. K. Olszewski, whose latest results have just been given to the world, and a short description of whose work will probably be of general interest.

The most critical portion of any apparatus for such a purpose is of necessity the glass tube in which the liquefaction is to occur, the capacity of which for withstanding rapid changes of both temperature and pressure is put to the severest test. Olszewski paid particular attention to the preparation of his tube, heating it for some time almost to redness in an iron tube packed with calcined magnesia, and allowing it to cool slowly beneath a thick layer of hot ashes, thereby obtaining a tube in which more than a hundred experiments were performed without a single explosion. The open end of this tube, *a*, was attached to a brass flange, *b*, the upper part of which was furnished with two openings, one for the hydrogen thermometer, whose bulb reached to the bottom of *a*, the other uniting the tube *a* with a branched copper tube *e*, by means of which connexion could be made at pleasure with (1) the manometer *f*, for use with pressures smaller than atmospheric, (2) an air-manometer, *g*, for use with higher pressures, (3) a large air-pump for reducing the pressure upon the liquefied gas, (4) an aspirator, *r*, used as afterwards described in the density determinations, and (5) an iron Natterer cylinder, *i*, in which the gas to be liquefied was stored up under a pressure of 60-80 atmospheres. A caoutchouc stopper, *k*, held the liquefaction tube within a system of glass cylinders designed for the reception of liquid ethylene, which was used to effect the reduction of temperature, and for preserving the same from the warming influence of the surrounding air. The four vessels were held within each other without touching by pieces of cork and felt rings, so that the ethylene was separated from the surrounding air by badly conducting layers of air, and the evaporated ethylene, passing in the direction of the arrows between the walls, still further counteracted the influence of radiation from warmer surroundings. In the outer cylinder were placed a few pieces of chloride of calcium in order to dry the air and prevent the deposition of hoar frost. The liquid ethylene was supplied from a second Natterer cylinder, *l*, fitted with a siphon arrangement and placed in a mixture of ice and salt; on the way to its receptacle the ethylene passed through a spiral copper tube surrounded by a freezing mixture of solid carbon dioxide and ether contained in a double-walled vessel, *m*. On connecting the vessel with the air-pump and reducing the pressure, the temperature of this freezing mixture sank to $-100°$, and 150 c.c. of liquid ethylene were obtained, which remained perfectly quiet for hours under atmospheric pressure. The glass tube *n* was then connected with the air-pump, by means of which the pressure was reduced until the ethylene began to boil; here however a difficulty, for a long time insurmountable, presented itself; for it was found that inequalities of temperature in the ethylene column caused violent disturbances, and the liquid rapidly disappeared out of the vessel. A simple expedient, however, that of forcing a regulated stream of dry air through the ethylene, was eventually hit upon and found to work admirably, keeping the whole column in constant agitation and at a measurable temperature. The pressure over the ethylene was maintained by use of the air-pump at about 10 millimetres of mercury.

By this means such a diminution of temperature was effected that all gases, with the exception of hydrogen, could be liquefied at pressures not exceeding 40 atmospheres. As soon as the manometer of the air-pump indicated 10 mm., the valves *f* and *h* were closed, and *i* of the Natterer cylinder opened, admitting the gas to be liquefied into the tube *a* at 40–60 atmospheres pressure, as indicated by the manometer *g*, when a considerable quantity of the liquefied gas was readily obtained. And now Olszewski elaborated a most ingenious device, by means of which the liquid could for some time be retained as such on releasing the pressure, and even—which is almost incredible, and a striking example of the truth of the adage "fact is stranger than fiction"—*in vacuo*. The addition to the apparatus consisted of the introduction in the liquefaction tube of a second thinner-walled tube, about half the length of the former and of smaller diameter, so that, when in position, the distance of its walls from those of *a* was about 1 millimetre. On performing the experiment as before, the liquid first collected only in this interspace, after a short time also in the inner tube, thus exhibiting two meniscuses; eventually the liquid in the interspace flowed over into the inner tube, and finally the levels equalized at its edge. The liquid was now gradually freed from pressure by shutting off the Natterer cylinder and its manometer and opening the valve *h*, and consequently reduced in temperature still further by the evaporation produced, hence the liquefied ethylene became relatively warmer and caused the liquefied gas contained in the interspace to evaporate



entirely away, leaving a badly conducting layer of gas, whose eminent isolating action was found sufficient to keep the remainder in the inner tube in the liquid state at normal atmospheric pressure. One step further: on closing the stopcock *o*, and connecting *h* with *s* by means of lead and caoutchouc tubing, communication was effected between the liquefaction tube and the air-pump, and, owing to the before-mentioned action of the layer of gas, a notable quantity of the liquefied gas still remained at pressures below 100 millimetres of mercury, as shown by the manometer *f*. The temperature of liquefied oxygen under these circumstances sank to −198° C., that of air to −205°, and that of nitrogen to −213°.

In his latest work Olszewski used two such little isolating tubes, and was enabled to reach in case of oxygen −211°; at −207° and 100 millimetres pressure, carbon monoxide solidified, as did also nitrogen at −214 and 60 millimetres.

By lowering the pressure over the solid nitrogen to 4 mm., Olszewski succeeded in penetrating the dark region approaching absolute zero as far as −225° C. It will be remembered that Pictet found a pressure of 650 atmospheres necessary at −140° to liquefy hydrogen, but by combining the above apparatus with one similar to Cailletet's, so that the gas could be subjected to 190 atmospheres pressure at −213°, Olszewski has effected the same result, which was also independently obtained by use of liquefied nitrogen boiling *in vacuo* (*Compt. rend.* xcviii. 913, 1884).

The chief importance of these experiments lies in the fact that it now becomes possible to determine several of the physical constants of liquefied gases at ordinary pressure, and a short description of how this has been done may not be uninteresting.

In order to determine the boiling-points, about 15 cubic centimetres of the liquid were obtained as above, gently freed from pressure, and communication with the air established by opening the valve *h*. Marsh gas, nitric oxide, and oxygen behaved under these circumstances perfectly quietly, evaporating only from the surface, necessitating shaking of the apparatus to prevent superheating; while in the case of carbon monoxide and nitrogen the evaporation proceeded with gentle ebullition. It required 5 to 15 minutes for the liquid to escape completely out of the apparatus, affording ample time to take the boiling-point with a hydrogen thermometer. A list of the boiling-points obtained is given in the table. It is satisfactory that Wroblewski has completely confirmed the accuracy of Olszewski's temperatures by thermo-electric measurements, and he asserts that a hydrogen thermometer affords correct indications as far as −193°, but the latter gentleman proves that the error must be very small, as all the boiling-points are above −220°, the critical temperature of hydrogen, and he shows that oxygen and nitrogen thermometers are not influenced by an error exceeding 2° even at several degrees below their critical points. From an inspection of the critical points given in the table we can at once see why the earliest attempts to liquefy these gases so utterly failed, for no amount of pressure would liquefy nitrogen for instance, unless its temperature could be at the same time reduced to −146°, a temperature not procurable by the means known to the earlier experimenters.

For the purpose of the density-determinations the inner tube within the liquefaction tube was calibrated, the thermometer removed, and the hole in the stopper closed with glass rod and sealing-wax. About 15 c.c. of the liquefied gas were obtained as before, freed gradually from pressure, and, as soon as all the liquid in the interspace had evaporated, the height of the liquid column left under atmospheric pressure was read off. At the moment of reading off the valve *h* was connected by a caoutchouc tube with the aspirator *r*, and when the gas was completely volatilized, water was run out until the levels in the tube and respirator were again equalized. The volume of water received in the measuring-flask was of course equal to that of the gas formed by evaporation of the known volume of liquid, and, after applying certain corrections dependent upon the nature of the apparatus, was reduced to 0° and 760 mm. As the pressures under which the densities of marsh gas, oxygen, and nitrogen were determined were nearly identical, the numbers obtained are strictly comparable.

	Boiling-point. °C.	Melting-point. °	Critical point. °	Density. mm.
Marsh gas	−164			0·415 at −164° and 736
Oxygen	−181·4		−118·8	1·124 at −181·4 and 743
Nitrogen	−194·4	−214	−146	0·885 at −194·4 and 741
Carbon monoxide	−190	−207	−139·5	
Nitric oxide	−153·6		−93·5	

It is a subject for sincere congratulation that these dangerous experiments should have been so far free from accident, but this immunity was not to last *ad infinitum*, for, just as the last experiment with nitrogen was in progress, the liquefaction tube suddenly flew to pieces and so deranged the apparatus that the densities of carbon monoxide and nitric oxide could not be determined.

These researches, taken in conjunction with those of Victor Meyer on the dissociation of the molecule of iodine, and of Lockyer, Liveing and Dewar, and other workers on the effect of high temperature generally in simplifying the structure of molecules, have assisted, and will in the future assist us still more, in arriving at much more accurate views respecting the ultimate structure of matter itself. On the assumption that the molecule of iodine consists of two atoms, which, according to the view now becoming more and more accepted by thinkers on this subject, may themselves consist of aggregations of a still simpler substance—aggregations which, at temperatures obtainable in the laboratory, we have not been able to break up—the classical experiments of Victor Meyer have shown that at a temperature of about 1500° C. the molecules are dissociated into single atoms, that is to say, the intensity of the heat-vibrations is so great that the attraction between the two atoms in the molecule is overcome, and they are torn asunder. At still higher temperatures there is a possibility that the atom itself could be resolved into something simpler still.

Reasoning on the same lines, there is great probability that even hydrogen, oxygen, and other more permanent gases could, by a sufficiently high temperature, be resolved first into single atoms and then into something simpler still. Now, taking the opposite extreme, on reducing the temperature sufficiently to liquefy and even to solidify these gases, we ought to find that as the atoms in the molecule are allowed to approach more closely, and consequently to attract each other more strongly (according to the law of inverse squares), the difficulty of breaking up the molecule into its constituent atoms is more and more increased. This, in the case of liquefied oxygen, has been directly proved to be the case by a series of very beautiful experiments performed by Prof. Dewar, who has shown that liquefied oxygen at −160° C. has not the slightest chemical action upon, among other substances, the alkali metals and phosphorus, which in ordinary air or oxygen are rapidly converted to oxides. Chemical action, if such there had been, would have shown that the force of the attraction of atoms of phosphorus or potassium for those of oxygen exceeded that of the atoms of oxygen for each other; but the result proved that at this low temperature the force (whatever force may mean) exerted between the atoms of the molecule of oxygen was greater than that between the atoms of potassium and oxygen. What the possibilities are as we approach absolute zero form an interesting subject for the "scientific use of the imagination," but, reasoning from analogous phenomena of polymerization, of which organic chemistry furnishes so many examples, and from the antilogous effect of high temperature, we have some reason to suppose that the condensation will continue until molecules more complex than those consisting of the ordinary two atoms are built up. However this may be, the main result of these important experiments has certainly been to show in the clearest possible light how completely the state of matter depends upon the temperature under which it exists.

A. E. TUTTON.

A RECENT JAPANESE EARTHQUAKE.

PROFESSOR SEKIYA, of the Imperial University, Tokio, has lately sent to this country a remarkably interesting and complete record of earthquake motion obtained by him during a severe shock which occurred at 6.52 p.m. on January 15 of this year. The most important portion of the record is shown in Fig. 1, reduced to a little more than one-third of the original size. The motion is recorded (by means of the writer's horizontal pendulum and vertical motion seismographs) in three rectangular components—two horizontal and one vertical—on a plate of smoked glass which is caused to revolve uniformly by clockwork. The plate is started by an electric seismoscope at the beginning of the disturbance, and for one or two seconds its motion is consequently slower than the uniform rate it afterwards attains. On this occasion the plate made one revolution in 126 seconds, and the horizontal motion continued during several revolutions. To avoid confusion only the first of these is reproduced in the figure: the motions which occurred subsequently were smaller, and, as usual, the disturbance subsided very gradually. The circles in which the three components are recorded have been arranged so that simultaneous motions are on the same radius. Radial straight lines, where they are drawn, mark seconds of time. The disturbance begins at *a*, *b*, and *c* in Fig. 1. In its early portion it is marked very conspicuously by a feature which has been noticed (also at the beginning) in previous records—the presence of short-period oscillations superposed on larger and slower motions. These are particularly well defined in the horizontal motion, where they

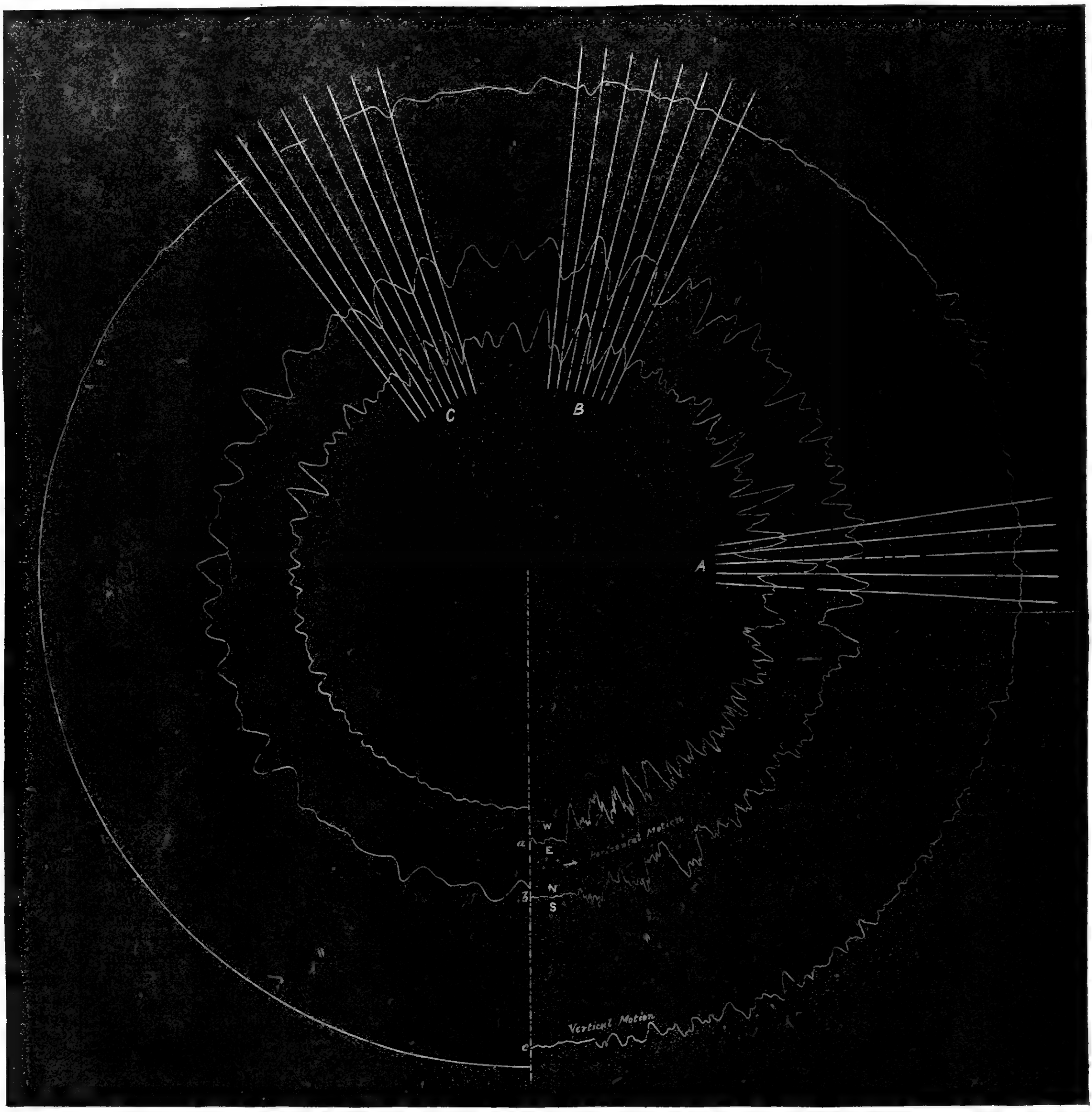


Fig. 1.—Earthquake recorded at the University, Hongo, Tokio, Japan, January 15, 1887, 6.52 p.m., by Prof. Sekiya. The horizontal motion is magnified 1·8 times; the vertical motion is magnified 2·9 times; the radial lines mark seconds of time.

occur, during the first part of the disturbance, with a period of about one-sixth of a second, or with about twelve times the frequency of the principal motions. The greatest amplitude of horizontal motion is found when these small oscillations have nearly died out, at the place marked A. By that time the vertical motion has become comparatively small. A few seconds later two considerable vertical oscillations appear on the record; but the vertical component is, by a long way, the first to vanish. In the original record the horizontal components are each magnified five times, and the vertical component eight times: the same ratio between horizontal and vertical multiplication is of course maintained in the figure given here. At three places, A, B, and C, the

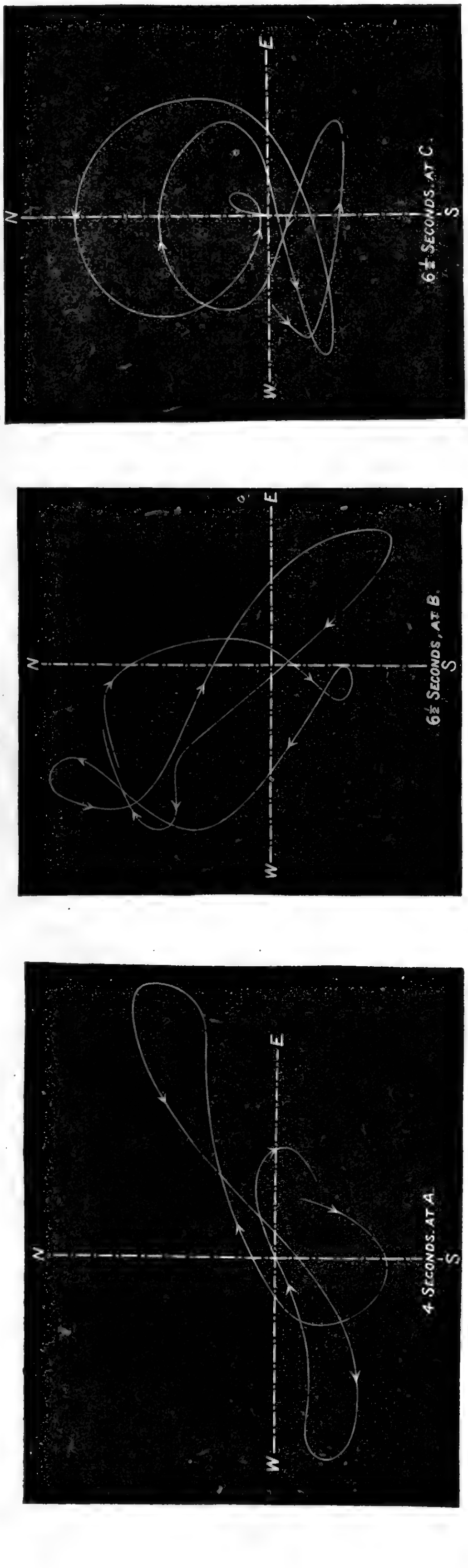


Fig. 2.—Compounded Horizontal Motion.

horizontal motion has been compounded during intervals of 4, 6½, and 6½ seconds respectively: the results are shown to a magnified scale in Fig. 2, and illustrate well the complex character of earthquake motion. The greatest extent of horizontal motion is from one to the other extremity of the figure-of-eight in the first of these diagrams: its actual amount (on the ground) was 7·5 millimetres. The greatest vertical motion was 1·5 millimetres. Other records obtained by Prof. Sekiya lead him to conclude that the greatest vertical motion in Tokio earthquakes is about one-sixth of the greatest horizontal motion. In former examples published by the writer the record was in all cases taken on the soft alluvial soil on which the greater part of the city of Tokio is built. In this instance the record was taken (at the site of the new University buildings, Kaga Yashiki, Hongo) on the much harder ground which here and there rises above the alluvial plain. From a comparison of records taken at the old and the new sites of the Seismological Observatory, Prof. Sekiya concludes that the motion of the alluvial plain is generally greater than that of the higher and stiffer soil in the ratio of two or three to one.

J. A. Ewing.

NOTES.

On Tuesday, Congregation at Oxford declined, by a majority of 106 votes to 60, to sanction the lending of books or manuscripts from the Bodleian Library. This decision is, no doubt, greatly regretted by a number of resident graduates, but it has the cordial approval of most other persons. Had the proposed change been made, it is certain that sooner or later many valuable books and manuscripts would have been lost or injured, and scholars would constantly have found that the works they wanted were "out." It would have been a serious mistake to transform one of the most magnificent collections of books in the world into a lending-library for the benefit of a small class of students.

In celebration of the fiftieth anniversary of Her Majesty's reign, the general meeting of the Zoological Society of London on June 16 will be held, at 4 p.m., in the Society's Gardens on the lawn, which will be reserved for this occasion. After the usual formal business, the silver medal awarded to the Maharajah of Kuch-Behar will be delivered to His Highness. The President will then give a short address on the progress of the Society during the past fifty years. After the conclusion of the general meeting, the President and Council will hold a reception of the Fellows of the Society and other invited guests.

The new University of Upsala was opened with great ceremony on May 17. There were present the King and Crown Prince of Sweden, a number of delegates from foreign Universities, the leading Swedish men of science, and some 1500 students. The building is very handsome, and has cost nearly £250,000.

In the Report of the Royal University of Ireland for 1886, just issued as a Parliamentary Paper, it is stated that last year 2933 persons presented themselves at the various examinations, an increase of 43 on the previous year. The degree of Bachelor of Arts was conferred on 9 women, of whom 4 took honours. One lady was admitted to the degree of Master of Arts, and another, Miss Mary Story, obtained the first place in the first-class honours in modern literature, and won a first-class exhibition. Of the 78 women who presented themselves for matriculation, 71 passed, 27 of them with honours. Speaking of the exhibitions founded by the Drapers' Company and the Irish Society for the promotion of education among women in Londonderry, the Vice-Chancellor says:—"It would be most useful that the example thus set should be followed by others. There are other Companies of the Corporation of London who also hold property in the district of Londonderry. Surely they could not employ the income, which they hold as a public trust, in a more advantageous manner than in facilitating the education of deserving persons, hindered by straitened means from securing for themselves the benefits of higher education."

At the last examination of students of medicine at the Nicholas Hospital, in St. Petersburg, fifty-four ladies out of ninety-two obtained their degree.

The first Danish lady physician, Miss Nielsen, has just begun to practise at Copenhagen. She took her degree with the highest honours.

In a lucid and interesting article in the *Scotsman*, on "Temperature of the Western Lakes and Lochs," Dr. H. R. Mill sums up the results of various recent observations made by himself and by Mr. John Murray, of the *Challenger* Commission. The eastern fringe of the North Atlantic brings between the western islands water at a uniform temperature of 46°. An equal temperature prevails on the surface, exeept in the vicinity of land, where it is higher. In nearly land-locked sea lochs and basins the temperature of the mass of water is determined by the configuration, and varies from 47°·5 to 43°·8, according to certain definite laws. In fresh-water lakes, those that are shallow are at a temperature of about 45°; those that are deep are colder, varying from 43° to 41°, and showing hardly any difference in temperature between surface and bottom.

On May 19, at 22h. 37m. (Greenwich time) a shock of earthquake was felt in the Alpes Vaudoises, at Sion, Bex, Aigle, Vevey, Rougemont, Gessenay, and other places. On May 20, 3 h. 5m., a slighter shock was felt at Rolle (Vaud).

On May 30, about 3 o'clock in the morning, heavy shocks of earthquake were felt at the city of Mexico. The earth tremor was of a violent kind, and had a lifting motion lasting five seconds. This was followed by a low roar and a strong vibration of the earth from east to west, lasting thirty-nine seconds. The houses rocked, and thousands of people left their beds. It was afterwards found that shocks of earthquake had been general in the States of Hidalgo, Mexico, Morelos, Puebla, Tlascala, Vera Cruz, and Oajaca. The force of the earthquake caused bells to ring and walls to crack. One of the aqueducts bringing water into the city of Mexico was damaged. On the same day a severe shock of earthquake occurred at Benton, Arizona, at noon, and at Nogales, Arizona, at 1 o'clock in the afternoon.

According to a telegram from New York, dated May 31, shocks of earthquake have been felt in the islands of St. Lucia, St. Vincent, and Grenada.

A cyclone of unusual severity passed over the northern portion of the Bay of Bengal last week. The Calcutta Correspondent of the *Times* says that at the beginning of the week the Meteorological Department reported that a storm had formed near Diamond Island, and was slowly advancing towards the Madras coast. At first the storm appeared likely to strike land near Vizagapatam, but on Wednesday morning it took a more northerly direction, and during the following night passed between Saugor Island and False Point, and thence inland, *viâ* Midnapore and Chota Nagpore. At Saugor Island the wind's rate was sixty-seven miles an hour, when the anemometer and storm signals were blown away. It is believed that the wind attained greater force later.

The New England Meteorological Society has two special investigations on hand for the coming summer, in addition to its regular work of temperature and rainfall observation. The first special subject (which has been investigated during the last two summers) is thunder-storms in New England; the second is the sea-breeze on the eastern coast of Massachusetts, now undertaken for the first time. The Society would be unable to carry on these inquiries but for the aid received from the U.S. Signal Service, the Bache Fund of the National Academy, and the Harvard College Observatory.

General Greely, the new Chief Signal Officer of the United States, has made a laudable effort to publish the Monthly Weather Review as nearly as possible up to time. The Reviews for January and February last have lately been issued—leaving some six months of arrears to be worked up subsequently. If these Reviews are published regularly, and quickly, the current information contained in them will be of much value, as they give not only complete data for the whole of the United States and Canada, but also details of the storms, ice, and fog in the Atlantic Ocean. The Reviews in question are accompanied by a number of very clear charts, one of which shows the tracks of the areas of low pressure over the ocean in each month, and the appendices contain particulars of miscellaneous phenomena and various notes. Among the latter may be specially mentioned articles on sunspots and meteorological phenomena, and results of anemometer observations at sea, with a description of a graphical method for deducing the true velocity of the wind from the records of the instrument and the motion of the vessel.

The Oficina Meteorológica Argentina has just published vol. v. of its *Anales* (620 pp. 4to, Buenos Aires, 1887), containing the monthly and yearly results of observations made at various stations during the year 1884, together with exhaustive articles on the climate of four places in the Republic, based on observations taken between the years 1855 and 1886. This service, which is undoubtedly the most completely organized of any existing in the South American States, is now under the superintendence of Mr. W. G. Davis, who has succeeded Dr. B. A. Gould, the former Director. A new meteorological observatory is being built at Cordóba, and will be furnished with the best self-recording instruments—the astronomical observatory, with which it has been hitherto connected, being made a separate institution. Several new stations have been recently established in remote places, including an important one on Stat.n Island (54°–55° south latitude). The service is under the Ministry of Public Instruction.

At a recent meeting of the Canadian Institute, Dr. Rosebrugh, of Toronto, read a paper on and presented some specimens of the photography of the interior of the living eye. Two series of photographs were shown. The first simply presented views of the optic nerve and retinal blood-vessels. The second series showed not only the retina of the eye, but also an inverted picture of objects to which the eye was directed, depicted upon the retina.

A photographer at Pesth has succeeded in taking photographs of projectiles, fired from a Werendler gun, whilst having a velocity of 1300 feet per second. The projectiles appeared on the impressions enveloped in a layer of air hyperbolic in form.

In a recent communication to the Russian Geographical Society, M. Krasnoff has described the formation, at the present time, of loess from the glacial gravelly clay in the Tian-Shan. The rains which wash this clay take away its finest dust, which is deposited in layers, but accumulates slowly on account of the small amount of rain. M. Krasnoff supposes with much probability that the yellow loess of China originated in this way. As to the flora of the Tian-Shan, M. Krasnoff points out that it formerly held a place between that of the Altai and that of the Alps, and resembled the present vegetation of the Caucasus. The desiccation of the country caused the retreat of the glaciers, the formation of the debris-covering on the mountain slopes, and the decrease of the lakes. The loess was deposited, and, as desiccation proceeded, the stony and sandy steppes by and by made their appearance. The process of desiccation went on first on the southern slopes of the mountains, where the dry steppe now rises to the limits of perennial snow. With the exception of a few species which accommodated themselves to the new conditions, all plants of an Alpine character and those that grow in moist climates disappeared, as also did the forest vegetation on the dry slopes of the hills. The place of the old flora was taken by immigrants from the drier parts of Asia.

THE death is announced, at the age of seventy-six, of the Swedish botanist, Prof. J. E. Areschong. His best-known works are "Symbola Algarum Floræ Scandinaviæ," "Iconographia Phyctologia," and "Phyceæ Marinæ."

THE Norwegian Storthing has granted £100 to Herr Dahll, to enable him to issue a short popular scientific work on the geology of Northern Norway. The Assembly has, however, refused at present to grant £450 to Prof. W. C. Brögger, who is anxious to complete a work which he has had in hand for several years on the syenite and granite rocks of the mountains around the Christiania fjord.

PERSONS interested in the fisheries of Sweden have often urged that oyster-beds should be formed on the south-west coast of the country, similar to those which have been so successful on the opposite coast of Norway. This is now being done by a Swedish naval captain, Mülenfels, who is founding two oyster-beds on the coast of the province of Bohus. The young oysters to be laid down will be taken from the bed at Öster-Risör, in Norway. The oysters cultivated there are said to equal "natives" in flavour.

ON May 11 the American Oriental Association held its spring meeting in Boston. The number of papers read at the meeting was unusually large. The paper which seems to have attracted most attention was one by Dr. W. Hayes Ward, who offered a new interpretation of a scene presented on a number of Babylonian seals. The late Mr. George Smith thought the design represented the Tower of Babel. Others have held that it is a representation of the underworld opening to receive the dead, and of a porter leading the soul into the presence of a deity. Dr. Ward's theory is that certain prominences invariably found on the seals stand for mountains, as they undoubtedly do in Assyrian art, and that the deity surrounded by rays is the sun-god Shamash. During the night he has been under the earth, and in the morning the porter opens the gate to let him out. In the discussion which followed the reading of the paper, Prof. Lyon, of Harvard College, and Prof. Jastrow, of the University of Pennsylvania, took part; and some evidence was brought forward to show that Dr. Ward's ideas are confirmed by references to sunrise in cuneiform texts.

A FINE series of new colouring-matters has recently been discovered by Dr. J. H. Ziegler (*Berichte der Deut. Chem. Ges.*, No. 8, 1887), by employment of the hydrazine reaction upon amido-derivatives of triphenylmethane. Rosaniline hydrochloride was first converted by nitrous acid into its diazo-derivative, which was then reduced with tin and hydrochloric acid, yielding brilliant green crystals of a hydrazine salt. This new hydrazine, which, in contradistinction to rosaniline, the discoverer terms roshydrazine, is itself a colouring-matter of a somewhat bluer shade than fuchsin, and forms the nucleus of the series. By treatment with aldehyde, acetone, or benzophenone, condensation products are obtained possessing brilliant colours, varying from red to violet; benzaldehyde and aceto-acetic ether, on the other hand, yield beautiful blues, while grape-sugar forms with roshydrazine a dye of a greenish-blue tint. Very numerous shades are further produced by action of many other reagents, and, moreover, the sulpho-derivative of roshydrazine appears to form a second series of coloured substances quite as numerous as those of the nucleus itself. Indeed, the discovery will, in all likelihood, prove a very rich one, and will afford another instance of the immense assistance which pure chemistry so frequently furnishes to the commercial world. The fact of most vital importance about these new colours, which are practically insoluble in water, is that they may be readily prepared *in situ* upon the fibre, it being only necessary to immerse it first in a bath of roshydrazine, and afterwards in a second bath containing the condensing reagent.

MESSRS. LONGMANS are preparing for publication "Modern Theories of Chemistry," by Prof. Lothar Meyer, translated from the fifth edition of the German by Prof. P. Bedson and Prof. W. C. Williams; "Electricity for Public Schools and Colleges," by W. Larden; "A Text-book of Elementary Biology," by Prof. R. I. H. Gibson; "The Testing of Materials of Construction," by W. C. Unwin, F.R.S.; and "Astronomical Work for Amateurs: a Practical Manual of Telescopic Research adapted to Moderate Instruments," edited by I. A. W. Oliver, with the assistance of Messrs. Maunder, Grubb, Gore, Denning, and others.

MANY of the beautiful Alpine flowers, especially the edelweiss and the Alpine rose, are in danger of becoming extinct. The Government of Valais and the Monte Rosa section of the Alpine Club, have caused gardens to be laid out and inclosures to be made for the cultivation and protection of these plants. The station on the Tête de Mouton, near Vissoye, in the Einficht Valley (Valais), situated at the height of 2300 metres, cultivates not only plants belonging to the Alps, but some from the Pyrenees, the Himalayas, and the Caucasus.

IN the Report of the Rugby School Natural History Society for 1886, just issued, the editors are able to congratulate the Society on the number of its members and associates being greater than in any previous year. Among the contributions printed with the Report are papers on the motion of stars in line of sight, the dispersion of seeds and spores, Danes' Blood, and the protective colouring and form of animals.

THE Grand Duke Nicholas of Russia, eldest son of the Grand Duke Michael Nicholaievitch, has, it is said, written a book on the entomology of the Caucasus. His Highness is an ardent student of natural history, and studies every new work on the subject published in England, France, and Germany.

AT a recent meeting of the Natural History Society of Wisconsin, Dr. Peckham, the President, read an interesting paper on wasps, presenting the results of many experiments made in 1886 on the mental habits and peculiarities of these insects. In the section entitled "Emotions," Dr. Peckham discusses the question whether wasps have much sympathy with the suffering of their fellows. "To be sure," he says, "when we caught numbers of them, and painted them within the cage, they at once went to work to clean each other, and this seems to show that they have some desire to aid and comfort their friends. But we have often seen them continue to eat, with entire composure, near the body of one of their number that had just been crushed to death; and they frequently fall upon a dead relative, cut it up, and carry it into the nest to feed their young. An overpowering sense of utility is probably the cause of this cannibal propensity; as was the case in Tierra del Fuego where the natives were frequently forced, through stress of weather and scanty food-supply, to eat their old women."

In the number dated April 19, *Science* publishes an excellent ethnographic map, by Mr. A. S. Gatschet, representing the linguistic families of the Indian dialects in the south-eastern parts of the United States, so far as they can be traced by the study of actual remnants of tribes still lingering in or near their old haunts, and by historic research. Of all the families represented on the map, the Maskóki were at one time most important. It is said that in former times the tribes of this family extended from the Atlantic to the country beyond the Mississippi, and from the Appalachian Range to the Gulf of Mexico. The majority of the Maskóki tribes now live in the eastern parts of the Indian Territory.

Prof. G. Pouchet has recently published a long and interesting paper concerning the life and work of Ch. Robin, the late Professor of Histology in the Paris Medical School. A complete list of Robin's works adds greatly to the value of the paper.

The fourth number of the *Annales de l'Institut Pasteur* contains many interesting papers, among which are one by Duclaux, on the general biological phenomena of micro-organisms, and one by Bardach, Perroncito, and Carita, on the presence of the Bacillus of rabies in milk.

An explosion of natural gas, which had leaked from pipes and mixed with the atmosphere, took place lately at Youngstown, Ohio. The result was a fire, which burned down a church and a large number of new buildings. The cause of ignition was the lantern of a watchman, who narrowly escaped death. The use of natural gas as an illuminant and fuel is attended by considerable danger, because, being inodorous, it may leak without anyone noticing the fact until a disaster occurs.

In a pamphlet issued lately by the U.S. Hydrographic Office, Lieut. Underwood says that mineral oils are not so effective for use at sea as vegetable or animal. A comparatively small amount of the right kind of oil, say two quarts per hour, properly used, is sufficient, he asserts, to prevent much damage, both to vessels and to small boats, in heavy seas. The greatest result from oil is obtained in deep water. In a surf, or where water is breaking on a bar, the effect is not so certain; but, even in this case, oil may be of benefit, and its use is recommended by Lieut. Underwood. He advises that, when an attempt is about to be made to board a wreck, the approaching vessel should use the oil after running as close as possible under the lee of the wreck. The wreck will soon drift into the oil, and then a boat may be sent alongside of her.

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 testatrix, 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 aid is primarily intended for Austrian subjects; but foreigners of every nationality, if resident in Austria, may benefit by the Trust. Austrian subjects residing in England may also make application for a grant. Applications addressed to the Trustees (das Curatorium) must be transmitted to the President's office of the Common Council of the City of Vienna (an das Präsidial-Bureau des Wiener Gemeinderathes Neues Rathhaus) before August 31, 1887, through the Austro-Hungarian Embassy in London, 18 Belgrave Square, S.W., where particulars as to terms and conditions may be obtained.

The Gold and Silver Commissioners have requested Mr. Henry Dunning Macleod to investigate the relation between money and prices.

In Mr. Abercromby's article last week on equatorial wind currents and Krakatão dust, the end of the last paragraph but two (p. 87) should read thus—"and though the highest currents over the Polar limit of both the south-east and north-east trades are from north-west and south-west [not south-east] respectively," &c.

The additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♂) from India, presented by Mrs. C. J. Fisher; a Bonnet Monkey (*Macacus sinicus* ♂) from India, presented by Mrs. Yeates; a Lesser White-nosed Monkey (*Cercopithecus petaurista*) from West Africa, presented by Mr. T. H. Kenyon, R.N.; a Brown Bear (*Ursus arctos*) from Northern Europe, presented by Mr. John Rhind; a Common Squirrel (*Sciurus vulgaris*), British, presented by Miss Muriel Reed; a Blyth's Tragopan (*Ceriornis blythi*) from Upper Assam, presented by Mr. W. Brydon; a King Vulture (*Gypagus papa*) from Tropical America, presented by Mr. W. Allen Sumner; two Little Guans (*Ortalis motmot*) from Guiana, presented by Mr. W. Thomson; six European Tree Frogs (*Hyla arborea*), European, presented by Mr. E. Wroughton; a Larger Hill Mynah (*Gracula intermedia*) from Northern India, four Tuatera Lizards (*Sphenodon punctatus*) from New Zealand, deposited; a Patagonian Conure (*Conurus patagonus*) from La Plata, two Dark-green Snakes (*Zamenis atrovirens*) from Dalmatia, four Axolotls (*Siredon mexicanus*) from Mexico, purchased; a Common Rhea (*Rhea americana*) from South America, received in exchange; a Molucca Deer (*Cervus moluccensis*); a Japanese Deer (*Cervus sika*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

The Paris Observatory.—We have received Admiral Mouchez's Report for the year 1886, which was presented to the Council of the Observatory on February 4, 1887. Admiral Mouchez first refers to M. Lœwy's proposed new methods for determination of the constant of refraction and of the constant of aberration, the principles of which have already been explained in this column. With regard to refraction, it is pointed out that the exact determination of its amount at different altitudes and under varying conditions is of peculiar interest for an Observatory situated as that of Paris is, on the southern borders of a large city, so that the temperature of the strata of air to the north and to the south will probably differ considerably. M. Mouchez hopes that during the current year it will be possible to attack these fundamental problems with an instrument constructed on M. Lœwy's plan. The great meridian instrument and the Gambey circle have been actively employed during the year, a grand total of 16,505 observations having been obtained, 798 of which refer to planets, including 148 of the sun and 128 of the moon. The principal meridian work continues, as in recent years, to be the re-observation of Lalande's stars. The equatorials have been employed in the observations of comets, minor planets, nebulæ, eclipses of Jupiter's satellites, and occultations. It is almost unnecessary to remind our readers of the magnificent work in astronomical photography which has been carried on by the MM. Henry, and which embraces planets and their satellites (Hyperion has been photographed with an exposure of thirty-five minutes), the moon and stars, including clusters and double-stars. M. Mouchez reports that he is considering how the stellar photographs may be most conveniently utilized for the formation of a catalogue, and states that, before final decision, he awaits the results of the then approaching meeting of the Astronomical Congress. The macro-micrometer devised by MM. Henry for measuring the relative positions of stars on the photographic plates is described in detail, and some results of double-star measurements made with this instrument are appended. It appears that these are of considerable accuracy, the mean error of a single measure for the double-star ζ Ursæ Majoris being $0''\cdot077$ in distance and $0^{\circ}\cdot55$ in position-angle.

ASTRONOMICAL PHOTOGRAPHY.—The *New Princeton Review* for May 1887 contains an interesting article, by Prof. C. A. Young, with the above title. The article is, of course, of quite a popular character, but none the less is it deserving of perusal by astronomers—professional as well as amateur. In a rapid survey of the history of astronomical photography, Prof. Young refers briefly to the labours of J. W. Draper, Bond, Rutherfurd, Gould, Henry Draper, and Pickering, in America; of De la Rue, Common, and Roberts, in England; of the Brothers Henry, in France; of Vogel, in Germany; and of Gill, in South Africa. He then goes on to discuss the relative advantages and disavantages attending the employment of the reflector and of the refractor respectively in this particular department of astronomical science; pointing out, in the case of the refractor, the two directions in which, at the present time, efforts are being made to overcome the difficulties arising from the want of perfect achromatism of the object-glass, viz. Prof. Abbe's researches on the production of glass which shall be perfectly achromatic, and Herr Vogel's investigations on a new sensitizing medium which may be as sensitive to the yellow and green rays as the salts of silver are to the violet rays. In the remaining portion of the article Prof. Young distinguishes two classes of astronomical photographs: those in which the end is to produce a *picture* of the object; and those which are made for purposes of *measurement*, and the determination of precise numerical data. He gives various examples of each class, with a brief account of the progress which has been made in solar, lunar, planetary, stellar, and nebular photography, as thus classified, concluding with an account of the very remarkable results which have recently been obtained by Prof. Pickering in the photography of stellar spectra.

COMET 1887 *e* (BARNARD, MAY 12).—Dr. H. Oppenheim (*Astron. Nachr.* No. 278) has computed the following elements and ephemeris of this comet from an observation made at Cambridge, U.S., on May 12, and from two others made at Rome on the 15th and 17th:—

T = 1887 June 24·5559 Berlin M.T.

$$\left.\begin{aligned} \pi - \Omega &= 24^\circ\ 21'\ 30'' \\ \Omega &= 244\ \ 54\ \ 32 \\ \iota &= 17\ \ \ 9\ \ 21 \end{aligned}\right\} \text{Mean Eq. } 1887\cdot0.$$

$\log q = 0\cdot11510$

Ephemeris for Berlin Midnight.

1887.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Brightness.
June 1	15 49 55	16 12·3 S.	9·5323	0·1299	2·0
5	16 0 2	12 19·1	9·5185	0·1253	2·2
9	16 10 46	8 17·1	9·5097	0·1216	2·3
13	16 22 1	4 13·9	9·5062	0·1186	2·4

The brightness on May 12 is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *JUNE* 5–11.

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

At Greenwich on June 5.

Sun rises, 3h. 48m.; souths, 11h. 58m. 10·2s.; sets, 20h. 8m.; decl. on meridian, 22° 33′ N.: Sidereal Time at Sunset, 13h. 4m.

Moon (Full on June 5) rises, 19h. 31m.; souths, 0h. 4m.*; sets, 4h. 32m.*; decl. on meridian, 18° 7′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	4 15	12 44	21 13	25 6 N.
Venus	6 47	15 1	23 15	23 8 N.
Mars	3 14	11 15	19 16	21 13 N.
Jupiter	15 26	20 44	2 2*	8 56 S.
Saturn	6 29	14 35	22 41	21 56 N.

* Indicates that the southing and setting are those of the following morning.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	June 8,	1 16	*m*
δ Libræ	14 54·9	8 4 S.	,, 11,	1 52	*m*
U Coronæ	15 13·6	32 4 N.	,, 7,	23 48	*m*
W Scorpii	16 5·2	19 51 S.	,, 7,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 10,	0 14	*m*

M signifies maximum; *m* minimum.

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. ° °
5	29 Ophiuchi	6	20 52	21 59	60 224
6	B.A.C. 6081	6	20 40	21 39	20 258
10	45 Capricorni	6	23 49	0 53†	42 275
10	44 Capricorni	6	23 58	near approach	156 —

† Occurs on the following morning.

Saturn, June 5.—Outer major axis of outer ring = 38″·1; outer minor axis of outer ring = 15″·2; southern surface visible.

Meteor-Showers.

	R.A. °	Decl. °	
Near Antares	249	20 S.	
β Ophiuchi	261	5 N.	Rather slow.

GEOGRAPHICAL NOTES.

THE Expedition which went out to explore the New Siberian Islands, has returned to St. Petersburg with interesting results. The Expedition was organized by the Academy of Science, St. Petersburg, 26,000 roubles being contributed by the Emperor Alexander. Operations commenced in 1885, and considerable preparations had to be made. A winter retreat was chosen in the district of Kasachje (under 71° N. lat.), 30 kilometres south of Ustjansk at the mouth of the Jana. About 270 kilometres distant from Kasachje, were discovered the remains of a mammoth. At the end of March 1886, Dr. Bunge left for the Swatoinoss Mountains, where the real march with 240 dogs was to begin; 19 sledges drawn by 12 dogs, led the expedition over the frozen sea. In the latter half of April, the Jakutes returned with the sledges, and reported that the journey had been successfully accomplished. Dr. Bunge devoted his attention in particular to the Liachow Island, while Baron Toll attempted not only Kotelni Island, but also the Island of New Siberia. In May both explorers were at the Medweshji foothills, to the south of Kotelni Island. Liachow Island has a very uniform but rough appearance; it is 300 kilometres in circumference, the surface being uneven and hilly. The prevailing winds are east and west. The latter is extraordinarily violent, and works great mischief; it brings first rain, and then frost. Winter retires about the beginning of June, although the summer is never quite free from snow, mist, storms, &c. Enormous masses of perpetual ice inclose the island; only once could Dr. Bunge make a sea passage free from ice. In clear weather, looking northwards from Kotelni Island land is visible, which appears to be only 150 kilometres distant. The possibility of reaching this land is increased by the fact that a warm current flowing in a fixed direction prevents the sea from freezing over. The highest observed temperature in Liachow Island was only 8° (Réaumur). The snow melted in the beginning of June, and about the middle of the same month the first flower was found. Wild reindeer, wolves, Arctic foxes, and mice are found on these islands, as also sea-gulls, snipe, and other birds. With the exception of the mouse, all animals on the island are merely guests; they all winter on the land.

THE Canadian Government sent out at the beginning of May an Expedition for the exploration of the region watered by the great river Yukon in the north-west of the Dominion. The geology and natural history of the Expedition will be under the care of Dr. Dawson of the Canadian Survey; while a careful topographical survey will be made by Mr. W. Ogilvy.

IN the new number (128) of the *Zeitschrift* of the Berlin Geographical Society, Prof. Blumentritt has some critical remarks on the Spanish data with reference to the distribution of the native languages in the Philippines. Colonel Schelling contributes a useful abstract of the Russian Survey work up to 1885, and Dr. Emil Deckert a paper on the country and people of the Southern United States.

THE German Government has appointed Lieut. Kund, who has done such good work in the Congo region, chief of the scientific station which has been established at the Cameroons; for when the Germans undertake the development of any region they at once recognize the necessity for scientific observations in order to accomplish their object. A surgeon and botanist will also be appointed, and the party will remain three years at the Cameroons. The surgeon and botanist will have charge of the meteorological station, while Lieut. Kund will devote himself to the exploration of the interior lying to the east of Cameroons.

THE IRON AND STEEL INSTITUTE.

THE annual meeting of the Iron and Steel Institute was held on Thursday, Friday, and Saturday of last week, in the Theatre of the Institution of Civil Engineers, under the presidency of Mr. Daniel Adamson.

In his inaugural address the President exhaustively treated the question of the selection and adoption of metals for various purposes in the arts. Commencing with the purest iron obtainable, containing only 0·08 per cent. of foreign matter, he explained that it was wonderfully malleable, and welded at a comparatively low temperature; a further exceptional characteristic of such a metal was that it suffered little when worked at a colour-heat, whilst it endured percussive or concussive force without distress much better than the mildest steel. All the alloys of iron, or the steels, were less malleable and ductile than the pure metal, but were on the other hand much stronger, or possessed a much higher carrying power. Pure iron would maintain a maximum load of nineteen tons per square inch, whilst it would set at half that amount. By an addition of 0·13 per cent. of carbon, 0·52 per cent. of manganese, and 0·10 of silicon, sulphur, and phosphorus, a steel might be produced carrying 50 per cent. more than pure iron, whilst by a further addition of these elements, the carrying power might be increased to sixty tons per square inch. In thus increasing the strength, the ductility or reliability was reduced however in nearly the same proportion. It thus becomes evident how important is the selection of material for a given purpose, but besides this the stronger the material the more skill is required in working it, and the more forethought has to be manifested by the constructive engineer.

Referring specially to the subject of steel for guns, the President drew attention to the diversity of opinion, both in England and the United States of America, as regarded the selection of the proper metal and its treatment for ordnance, the artillerists maintaining that a strong and consequently hard steel was desirable, whilst engineers contended that a mild tough metal should be used; this was a question which he thought might be decided by the Iron and Steel Institute, with the result that guns would be made, as they could be made, which would not burst. He referred to what had been done by the late Sir Joseph Whitworth towards the compression and consolidation of steel, and by the late Sir William Siemens, especially as regarded the production and introduction of soft or ductile steel, which possessed great regularity in quality by the uniformity of its composition.

Another most important subject treated of was that of steel rails and weldless solid rolled steel tires. By this application of steel, the saving to railway companies had been estimated at 1 per cent. on the dividend, and this was largely due to the efforts of Sir Henry Bessemer; and he thought it was quite within the province of the Institute to suggest the most suitable material for the construction of railway and river bridges of moderate and large spans, by the application of which further economy would be effected.

After reference to the subjects of case-hardening weldable steel—for which, when manufactured with reliability and economy, there would be an enormous demand—cast-iron, and steel castings, the address concluded by drawing attention to the influence of high railway rates upon trade depression, and to the necessity of employers and employed working in unison, as by their intelligent action alone could we expect to defy the contention and competition of the world. The vote of thanks for the address was proposed by Sir Lowthian Bell, and seconded by Sir James Kits n.

A paper on the Terni Steel Works was read by Sir Bernhard Samuelson, which he prefaced with some remarks on the importance of testing commercial education, which was now under the consideration of the Oxford and Cambridge Joint Board for Local Examinations, and drew attention to the circumstance that Chinese and Japanese were being taught on the Continent in anticipation of trade being opened out with the East.

The next paper was by Mr. George Allan, on "Patent Composite Steel and Iron." After referring to the necessity for a material of this character, and the various attempts that had been made to produce it, the author proceeded to explain the meth of its manufacture. This consisted in embedding fibrous iron mild steel, and subsequently rolling the ingots into bars or plat as desired. "So perfect was the union of the two materia that by an inspection of the samples when the covering of ste was turned down to the strands of iron and the surface polish it was quite impossible to detect any separation between the tw materials, or which was iron and which steel."

The next paper read was by Prof. Chandler Roberts-Auste descriptive of a mode of electro-deposition of iron, a illustrated by a medallion in iron of Her Majesty execut by the process, the secret of success in which appears to be t employment of very feeble currents. The adherence of t deposited iron to the surface of the copper gives rise to conside able difficulty in detaching it; this was obviated by depositi nickel in the first place, allowing it to oxidize slightly, then aga depositing nickel and the iron on its surface. The subject w still under the author's investigation.

The first paper read on Friday was one by Sir Bernha Samuelson on the "Construction and Cost of Blast Furnaces the Cleveland District," supplementary of one read in 187 before the Institution of Civil Engineers.

Mr. James Riley, to whom the Bessemer Medal for this ye has been awarded for his excellent work in developing t manufacture and high quality of mild steel, read a paper of most elaborate character on "Some Investigations as to t Effects of Different Methods of Treatment of Mild Steel in t Manufacture of Plates." The author compared reheating wi soaking, or cooling gradually in pits; hammering with coggin cross-rolling with rolling in one direction only, and the resul due to different amounts of work.

It was found that the soaked ingots were slightly more sat factory than those reheated, the reheating having been perform in a non-radiation furnace, and that the results of cogged a hammered ingots were almost similar. Cross-rolling and ordina rolling were also found to give almost similar results. As regar "working" the ingot, the strength of the steel was found to i crease with the quantity of work put upon it, the ductility bei however diminished. The author looks upon annealing as corrective to damage done, and thinks that as regards t ordinary operations of a well-managed works annealing unnecessary. The paper relates to a very large number exp riments, the bending tests alone being close upon 1300, a gave rise to a very animated discussion.

Other papers on the programme, including one by Dr. H. Sorby, F.R.S., on "The Microscopical Structure of Iron a Steel" were taken as read. With reference to this paper, D Percy, the immediate Past-President, remarked before resigni the chair, "For twenty years, more or less, he has been engag in this kind of research, in which of late much has be done by foreign observers. Having carefully studied wh has been published on this subject, my conviction is tha with regard to originality of contrivance, accuracy, and impo ance, the work of Dr. Sorby is as yet unrivalled. He h successfully explored a comparatively new and most importa field of inquiry, and has thrown much light on some of the mo recondite problems concerning the mechanical and physic properties of iron and steel. My first impression is that t result of such researches will prove to be of the highest practic value."

THE INSTITUTION OF MECHANICAL ENGINEERS.

AT the recent meeting of the Institution of Mechanic Engineers, the President, Mr. E. H. Carbutt, gave a address, in which he reviewed the progress made in the man facture of guns during the last half century. The guns in use the beginning of the present reign, in 1837, were principal the cast-iron smooth-bore 24-pounder and 32-pounder wi spherical shot. Now they are made of steel, and provid with mechanical appliances for every movement; accuracy aim is insured by rifling, and the length of range increased the use of an elongated shot of small cross-section, and increased powder-charges. Breechloading has l d to increase speed of firing, and to the use of guns 35 and 40 feet long o board ship. The loading is self-acting in the smaller field gun whilst on board ship the guns are made to revolve, load, return position, and train to firing-point by hydraulic power. Such gu weigh 110 tons, fire shot 16¼ inches in diameter, weighing 1800 lbs., and costing £190 each. The advance thus shortly chronicled is due to several workers, prominent amongst whom may be mentioned Sir Joseph Whitworth, Sir William Armstrong, and Sir William Anderson. The production of ordnance of such a character has been due to the introduction of steel, and the possibility of producing steel in large masses by means of the open-hearth steel process, with which the name of Sir William Siemens will always be connected. The quick-firing machine guns are known by the names of their inventors, as the Gardner, Nordenfelt, Maxim, Gatling, and Hotchkiss.

The President also drew attention to the circumstance of the inventive talent of the country having been taken advantage of here, and ignored in France until after the Franco-German war; now, however, there as here, many works have found it to their profit to establish gun factories which supplement the Government factories to a large extent.

Two papers were read at the meeting on prime movers, the one by Mr. F. Brown, of Montreal, on "The Construction of Canadian Locomotives," and the other, by Major T. English, R.E., detailing experiments on the distribution of heat in a stationary steam-engine. The former, as its name denotes, refers to details of construction; the latter is illustrated by thirty-five figures, mainly of indicator diagrams, and distribution of heat diagrams showing in one view the applied and wasted heat. The series of trials extended altogether over fifty hours' working of the engine; but out of this trial, various results, representing in the aggregate twenty-eight hours' working, were rejected, on account of doubtful measurements at some point or other. The remaining trials are sixteen in number, in two sets—one condensing and one non-condensing—each with and without the steam-pipe jacketed, and each with a cut-off at approximately one-quarter, one-eighth, and one-sixteenth of the stroke respectively, thus making twelve different combinations. The conclusions drawn by the author are: that, in order to obtain the best results for any given range of temperature, there should be a definite relation between the surface of the steam passages, the diameter of the cylinder, and the length of stroke; and that in the design of a steam-engine the adjustment of these proportions is perhaps the most important point to be considered as regards economy. The calculated results of varying the length of the stroke of the engine which was experimented on—while the diameter of the cylinder, the absolute clearance volume, and the clearance surface exposed, remained unaltered—were tabulated for two different points of cut-off, and show that the same number of expansions may give widely different results as regards the ratio of efficiency and the water consumed per indicated horse-power per hour; and also that with the same length of stroke these results are but slightly affected by doubling the number of expansions.

NOTE ON THE SPECTRUM OF DIDYMIUM.[1]

It is well known that the absorption spectrum usually ascribed to didymium shows six bands in the blue and violet with approximate wave-lengths 482, 476, 469, 462, 444, 428, according to Lecoq de Boisbaudran.

The evidence that we at present possess shows, I think, that these bands belong to at least five different fractions of didymium.

Welsbach (*Monatshefte*, vi. 477) has shown that the band 428 occurs in the absence of all the others mentioned above in the spectrum of the fraction which he names neodymium. On the other hand, Crookes (Proc. Roy. Soc., 1886, 502, Fig. 1) has shown that all the other bands of neodymium can be obtained in the absence of the band 428. This band, therefore, belongs to a distinct fraction, and should be obtainable quite by itself.

Crookes has shown that the band 444 varies in strength independently of all others, and is therefore distinct. The same conclusion is arrived at by a slightly different argument. Welsbach's praseodymium shows the bands 482, 469, and 444, together with a faint band in the orange. Crookes (*ibid.*, Fig. 1) has shown that 482 and 469 can be got in a fraction which does not show 444. It is possible that the faint orange band of praseodymium belongs to the same fraction as 444, since its presence or absence would make little difference in the appearance of the dark orange band of the ordinary didymium spectrum, one part of which it forms.

The band 462 is shown to be distinct by a comparison of Crookes's Figs. 1 and 2, taking into account that 444 and 428 have been shown to be distinct.

The two bands 482 and 469 seem always to accompany each other. They occur together in Welsbach's praseodymium and in all the spectra of didymium fractions published by Crookes. They are distinct from 476, since they occur in praseodymium in the total absence of 476. They may belong to the same fraction as the faint orange band of praseodymium.

The band 476 does not occur in Welsbach's neodymium spectrum.

In fact the two bands 476 and 462 seen in the didymium spectrum are not accounted for by Welsbach at all in the spectra of praseo- and neodymium. Since 462 is distinct, 476 must also be distinct.

I have repeated Welsbach's experiments up to a certain point, and can confirm his results as regards praseodymium in every respect. There is no indication whatever that the three main bands belong to different fractions. I have not been able to satisfy myself quite that the faint orange band of praseodymium really belongs to the same fraction as the others, even supposing that the method of fractionation is not changed. In the didymium spectrum the orange band is much darker than the green, and the difficulty of getting a really concentrated praseodymium solution, which does not show a trace of the green band, is extreme. A small remnant of some other fraction of didymium might therefore cause a faint band in the orange some time after the band in the green had disappeared. Nevertheless, there is no doubt that by Welsbach's method the orange didymium band is split up, for the maximum absorption with didymium is not at the point in the orange where the band of praseodymium occurs.

I have not yet obtained the neodymium fraction free from praseodymium, but I have no reason to doubt that Welsbach's observations are correct. A study of the intermediate fractions brings out a point which Welsbach does not refer to. As we pass from the praseodymium end the bands 482 and 469 become fainter, whilst 476 and 462 first appear and then grow stronger, till they become distinctly stronger and much broader than 482 and 469.

It appears then that the absorption spectrum of didymium is splitting up just as the fluorescent spectrum of yttrium is. I have only discussed a few of the bands, but there is no doubt that the other bands will also in time be separated. Indeed, this separation has already been partially effected by Crookes for some of the bands in the red.

Perhaps the most surprising result arrived at by Crookes is that the splitting up of the fluorescent yttrium spectrum is unaccompanied by any change in the spark spectrum. On the other hand, Welsbach states that the spark spectra of praseo- and neodymium are parts of the didymium spectrum, and that, though similar in general appearance, they are really quite distinct. There does not appear to be any theoretical reason for this difference between yttrium and didymium, and it is to be hoped that the different fractions of didymium will be got pure enough to show whether the spark spectra can be still further split up.

CLAUDE M. THOMPSON.

University College, Cardiff.

[1] Reprinted from the *Chemical News*, May 20, 1887.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The first election to the Harkness Scholarship for Geology and Palæontology will be made in June. All B.A.'s of Cambridge not beyond M.A. standing are eligible. The Rev. Osmond Fisher is appointed an elector to the scholarship.

The report of the Council of the Senate on the teaching of geography is to be voted upon on June 9.

SCIENTIFIC SERIALS.

American Journal of Science, May.—On red and purple chloride, bromide, and iodide of silver; on heliochromy and the latent photographic image, by M. Carey Lee. To this paper we have already called attention. It is the first of a series of important papers, the object of which is to show (1) that chlorine, bromine, and iodine may form compounds with silver of beautiful peach-blossom, rose, purple, and black coloration; (2) that these compounds (except under the influence of light) possess great stability, and may be obtained by purely chemical means in the entire absence of light; (3) that the red chloride shows a tendency to the reproduction of colours, and may probably be the material of the thin films obtained by Becquerel and others in their experiments on heliochromy; (4) that these substances constitute the actual material of the latent or invisible photographic image, a material that may now be obtained in any desired quantity without the aid of light. They also form part of the visible product resulting from the action of light on the silver haloids. This first contribution deals with red silver chloride, and with the relations of photochloride to heliochromy. The author considers that in the reactions here described lies the future of heliochromy, and that this beautiful red chloride may ultimately lead to the reproduction of natural colours.—On the inter-relation of contemporaneous fossil floras and faunas, by Charles A. White. A chief object of this paper is to show that successive orders of fossil floras and faunas do not necessarily correspond so absolutely with given geological epochs as is generally assumed. On the contrary, the rate of progress of biological evolution from epoch to epoch has necessarily been variable, some contemporary species dying out at an early date, while others live on into subsequent epochs, according to the different conditions of their environments. Living species of land mollusks, for instance, are found also associated in the same strata with those of extinct genera and families of Miocene vertebrates. It is also incidentally shown that no European palæontological and geological classifications are entirely applicable to the conditions prevailing in the American continent.—The Eozoonal rock of Manhattan Island, by L. P. Gratacap. An examination of the rock recently exposed in New York when the cisterns were being constructed for the Equitable Gaslight Company, leaves little room to doubt that here a bed of hornblende has undergone a more or less complete conversion into serpentine, the change being in some places accelerated by the elimination of lime carbonate as calcite, and probably elsewhere the double carbonate of lime and magnesia as dolomite.—Terminal moraines in Maine, by George H. Stone. The generally unequal distribution of the glacial drift in Maine is well illustrated by the detailed description here given of its chief terminal moraines.—Note on the enlargement of hornblendes and augites in fragmental and eruptive rocks, by C. R. Van Hise. While recently studying the eruptive rocks of the Penokee-Gogebic iron-bearing series in Michigan and Wisconsin, the author met with cases of new growths occurring upon augite and hornblende, corroborating the observations made by Fr. Becke amongst the eruptive rocks of Lower Austria in 1883. In some instances the augite has been completely, in others partly, changed into hornblende, the rocks where these new growths occur being altered diabases.—The great Acadian Paradoxides, by G. F. Matthew. An almost complete specimen of this gigantic species has recently been found in the Cambrian basin of St. John, differing from any hitherto described, and mostly resembling the *P. bennettii* of Newfoundland and *P. harlani* of Massachusetts.—On the kin of *Paradoxides* (*Olenellus?*) *kjerulfi*, by G. F. Matthew. The object of this paper is to throw some light on the comparative age of the Paradoxides beds in Europe and America, and the probable position of Olenellus in relation thereto, the allies of *P. kjerulfi*, Linrs., being chiefly considered.—On Taconic beds and stratigraphy (continued), by James D. Dana. This second communication, which is accompanied by a large map of the Taconic region in Berkshire, Massachusetts, deals specially with the middle and northern part of that region. The author concludes generally that the limestone must be the underlying rock for the lower and narrower portions of the Taconic range, the schists of which are the same in kind, and essentially continuous. Most of the limestones are referred to the Lower Silurian age, some Cambrian also occurring.

Rendiconti del R. Istituto Lombardo, March 31.—On some methods of testing the purity of drinking-waters, by Prof. L. Maggi. Koch's method by cultivation in gelatine is shown to be greatly inferior in efficacy to that of Fol and Dunant by cultivation in meat extract, the former detecting only 5700 bacterial germs where the latter finds 100,000. The author points out further that Fol and Dunant's is substantially the same as the method already adopted at a much earlier date (1867) by himself and Prof. Giovanni Cantoni.—Meteorological observations made at the Brera Observatory, Milan, for the month of March.

April 14.—Effects of a thunderbolt, by Prof. R. Ferrin[i]. During a recent thunderstorm in Milan some planking place[d] over the mouth of a dry well and covered with cultivated eart[h] was removed by an electric discharge in such a way that th[e] earth was precipitated bodily into the well. A lightning-con[-] ductor from a neighbouring building had its terminus in th[e] well, where it is suggested that the explosion took place with th[e] result described.—On the second derivatives of the potentia[l] functions of space, by G. Morera. A simpler method than tha[t] of Hölder (*Beiträge zur Potentialtheorie*) is here proposed fo[r] determining the existence of the second derivatives of th[e] potential function of a mass distributed in a space of thre[e] dimensions.—The migrations of the tunny, by Prof. Pietr[o] Pavesi. The commonly-accepted view that the true tunn[y] (*Orcynus thynnus*, L.) is an oceanic fish migrating periodicall[y] from the Atlantic through the Strait of Gibraltar round th[e] Mediterranean basin is shown to be erroneous. This fish is, o[n] the contrary, essentially an inhabitant of the Mediterranea[n], where it migrates between the shallows in the spawning-seaso[n] and the deep waters for the rest of the year, but rarely passin[g] in large numbers beyond the Strait of Gibraltar.

Bulletin de l'Académie Royale de Belgique, March.—Memo[ir] on bichlorureted alcohol, by Maurice Delacre. To ethyl alcohol, $CH_3—CH_2(OH)$, correspond the three chlorurete[d] derivatives of alcoholic nature: (1) $ClCH_2—CH_2(OH)$; (2) $Cl_2CH—CH_2(OH)$; (3) $Cl_3C—CH_2(OH)$. The first [of] these having been determined by Würtz, and the third by Ga[-] zarolli-Thurnlack in 1881, the author has now succeeded i[n] obtaining the second, resulting from the action of zinc-ethyl o[n] anhydrous bichlorureted aldehyde, $Cl_2CH—CHO$. His de[-] scription of the process adopted is accompanied by analytica[l] data and experimental determinations leaving no doubt as [to] the nature of this compound.—On some derivatives of propan[e], by C. Winssinger. During his protracted studies of th[is] substance the author has determined, contrary to the observa[-] tions of Pierre and Puchot, the existence of a hydrate of propyl alcohol boiling at 87° C. He has also prepared in a pure sta[te] the sulphuret of orthopropyl with boiling-point 142° instead [of] the hitherto accepted 130° to 135°. He further shows that [a] solution of the organic hydrosulphates in alcohol is continuous[ly] decomposed during ebullition at contact with the alkaline hydr[o-] sulphates, yielding organic sulphur with liberation of hydr[o-] sulphuric acid. Lastly, he has determined some new compound[s], such as the oxysulphide of propyl, which is dissolved at 14° to 15°, and combines with the nitrate of calcium; a mon[o-] orthopropylphosphoric acid, and a tri-orthopropylphosphor[ic] ether. These substances are formed by the action of the penta[-] chloride of phosphorus or orthopropylic alcohol, and have th[e] respective formulas, $C_3H_7PO_4H_2$ and $(C_3H_7)_3PO_4$.—Research[es] on the localization and function of the alkaloids in plants, [by] MM. Errera, Ch. Maistriau, and G. Clautriau. For sever[al] years the authors have been engaged with the study of th[e] alkaloids, especially in *Colchicum autumnale*, *Nicotiana macr[o-] phylla*, *Aconitum Napellus*, and various species of Narcissu[s]. They have so far arrived at the general conclusion that th[e] alkaloids are formed chiefly in the more active tissues where th[e] albuminoids are incessantly decomposed and transformed. Fro[m] these tissues the alkaloids gravitate towards the peripher[y], where they become more easily oxidized, and serve to prote[ct] the plant against attack. Physiologically they are analogous [to] the alkaloids developed in some animals, such as snakes, to [an] extraordinary degree; and must be regarded as the waste [or] refuse of the protoplasmic activity afterwards turned to accou[nt] for protective purposes.

April.—Discovery of instruments of the Stone Age in th[e] Congo State, by Ed. Dupont. Some specimens of rude impl[e-] ments are described, which have recently been discovered [by] Capt. Zboinski on the left bank of the Lower Congo in th[e] region of the cataracts below Stanley Pool. They occurred [in] a district covered with chips of quartzite in the neighbourho[od] of South Manyanga, where this rock crops out, indicating th[e] site of a former quarry or manufactory of such objects, such [as] have frequently been found in other parts of the world, but ve[ry] seldom in Africa. They are unpolished, belonging to the Palæ[o-] lithic epoch, the presence of which along the west coast of Afri[ca] has also been recently confirmed by similar finds, but in sile[x] in the Mossamedes district much further south.—On a case [of] chemical decomposition produced by pressure, by J. H. van['t] Hoff and W. Spring. Under a pressure of 6000 atmospheres [and] a temperature of 40° C. the authors have succeeded in deco[m-] posing cuprico-calcic acetate which had previously been finely pulverized. The salt was slowly liquefied, and on the pressure being removed the surface of the instrument in contact with the salt was found covered with a coating of copper. Other experiments at lower and higher temperatures, but still much under the point of transition, showed that this substance is decomposed under the action of pressure, the process being accelerated according as the pressure and temperature are increased.—On forecasting the weather, by B. G. Jenkins. The author publishes a weather chart for London ranging over 62 years, showing, as he claims, that the moon not merely influences but is the actual cause of the weather, and consequently that it can be forecast by studying accurate barometric and thermometric readings recorded for a sufficiently lengthened period of time. He finds, for instance, that the readings for London for 1887 will be practically the same as those recorded for 1825, those for 1885 and 1886 corresponding in the same way with those for 1823 and 1824, and so on. He adds that in December last he issued a forecast for January 1887 based on the readings for January 1825, with the subjoined results:—Forecast: mean bar., 29·98; mean ther., 35°·5; rain, 1·5. Result: mean bar., 29·99; mean ther., 35°·9; rain, 1·3.

Notes from the Leyden Museum, vol. ix., No. 2, April 1887, contains, as usual, a large number of papers on entomology, and also a paper on a collection of mammals made at Mossamedes, from the pen of Dr. F. A. Jentink, the Director of the Museum. Mr. P. J. van der Kellen was one of the members of an Expedition to the Cunene River, which was commanded by Mr. Veth. On Mr. Veth's death, which took place very shortly on the Expedition reaching Mossamedes, Mr. van der Kellen determined himself to explore the district, and to make a collection of the fauna for the Leyden Museum. The country he is collecting in is, from a zoological point of view, unknown, and although none of the twenty-six species of Mammalia enumerated in this paper by Dr. Jentink are new to science, yet they form a most welcome addition to our knowledge of geographical distribution, and several of the forms are still very rare.

Engler's Botanische Jahrbücher, vol. viii. part 4, contains:—A contribution to the botanical geography of South Africa, by R. Marloth. This is a description of the plants growing in the south-west Kalahari district.—Contributions to the knowledge of the *Aponogetonaceæ*, by A. Engler. The chief conclusions arrived at are that the inflorescence of *Aponogeton* is not axillary in position, but two leaves and an inflorescence together form a collective whole, the inflorescence not being in the axil of either of them, but opposite the margin of one of the leaves: that in *A. distachyus*, which is the commonest cultivated species, the large white bract-like organ, which subtends each flower, is not a bract, but the single developed segment of the perianth: and finally that if the *Aponogetonaceæ* be united with the *Juncagineæ* and *Potamogetonaceæ* in the large family of *Najadaceæ*, the *Alismaceæ* should also be included in that family.—Then follows a condensed translation of the memoir on the vegetative organs of *Phylloglossum Drummondii*, by F. O. Bower, already published in the Trans. Roy. Soc., London: the chief result of this investigation is that as regards the vegetative organs, *Phylloglossum* appears to be a permanently embryonic form of Lycopod.—A list of plants found in West Greenland, together with remarks on their distribution, is contributed by Th. Holm, of Copenhagen, who accompanied the Danish vessel *Fylla* in its expeditions of 1884 and 1886.—The part closes with the continuation of the usual extracts from current literature.

SOCIETIES AND ACADEMIES.

LONDON.

Zoological Society, May 17.—Prof. W. H. Flower, F.R.S., President, in the chair.—The President read some extracts from a letter which he had received from Dr. Emin Pasha, dated Wadelai, November 3, relating to some skulls of the Chimpanzee from Monbottu, to some portions of the skeleton of individuals of the Akka tribe, and to some other objects of natural history which he had forwarded (*viâ* Uganda) to the British Museum of Natural History.—Mr. A. Thomson exhibited some specimens of a rare Papilio (*Papilio porthaon*) from Delagoa Bay, reared in the Society's Gardens.—Prof. Howes exhibited a drawing of a head of *Palinurus penicillatus*, received from M. A. Milne-Edwards, and remarked on the assumption of antenniform characters by the left ophthalmite shown in this specimen.—A paper was read by Mr. W. F. Kirby, Assistant in the Zoological Department, British Museum, entitled "A Revision of the Sub-family *Libellulinæ*, with descriptions of new Genera and Species." The last compendium of this group was published by Dr. Brauer in 1868, in which forty genera were admitted. Mr. Kirby now raised the number to eighty-eight, all fully tabulated and described in his paper, which likewise included descriptions of fifty-two new species. Mr. Kirby gave a short sketch of the characters of the *Libellulinæ*, and more especially of the neuration, which he considered to be of primary importance.—Mr. R. Bowdler Sharpe read the third part of his series of notes on the Hume Collection of Birds, which related to *Syrnium maingayi*, Hume, and to the various specimens of this Owl in the British Museum.—A communication was read from Mr. A. Smith Woodward, on the presence of a canal-system, evidently sensory, in the shields of Pteraspidian fishes. Mr. Woodward described a specimen which seemed to prove that the series of small pits or depressions upon the shields of these ancient fishes, observed by Prof. Ray Lankester, are really the openings of an extensive canal-system traversing the middle layer of the shield.—A second communication from Mr. A. Smith Woodward contained some notes on the "lateral line" of *Squaloraja*, in which it was shown that the "lateral line" of this extinct Liassic Selachian was an open groove supported, as in the Chimæroids, by a series of minute ring-like calcifications.

Anthropological Institute, May 10.—Mr. Francis Galton, F.R.S., President, in the chair.—Prof. Flower read a letter received by him from Emin Pasha, dated Wadelai, November 8, 1886.—Prof. Victor Horsley read a paper on the operation of trephining during the Neolithic period in Europe; and on the probable method and object of its performance. The paper was copiously illustrated by photographs of trephined skulls and of implements that may have been used in the operation. The fact that most of the holes are found in that part of the skull that covers the fissure of Rolando heightens the probability that the operation was performed as a remedy in cases of epilepsy, since the curve of brain-matter around that fissure is specially connected with what is known as cortical or Jacksonian epilepsy. It seems probable that the operation was, in the first instance, performed for depressed fractures of the skull, or for the traumatic form of epilepsy, and afterwards in other cases in which similar symptoms were observed.

Mathematical Society, May 12.—Sir J. Cockle, F.R.S., President, in the chair.—Prof. Anderson, Queen's College, Galway, was elected a member.—The following papers were read:—General theory of Dupin's extension of the focal properties of conic sections, by Dr. J. Larmor.—Sur une propriété de la sphère et son extension aux surfaces quelconques, by M. D'Ocagne.—On the motion of two spheres in a liquid, and allied problems, by Mr. A. B. Basset.—Second note on elliptic transformation annihilators, by Mr. J. Griffiths.

Chemical Society, May 5.—Mr. William Crookes, F.R.S., President, in the chair.—The following papers were read:—A contribution to the study of well water, by Mr. R. Warington, F.R.S.—Crystals in basic-converter slag, by Mr. J. E. Stead and Mr. C. H. Ridsdale.—Note on the influence of temperature on the heat of dissolution of salts in water, by Dr. William A. Tilden, F.R.S.—The distribution of lead in the brains of two factory operatives dying suddenly, by Mr. A. Wynter Blyth. At a certain lead factory in the east of London five cases of more or less sudden death at different dates have been attributed to the effects of lead. In two of the cases the author had an opportunity of making a toxicological investigation. There has hitherto been no reasonable hypothesis to explain the profound nervous effects of the assimilation of minute quantities of lead, but if it is allowed that lead forms definite compounds with essential portions of the nervous system, it may then be assumed that in effect it withdraws such portions from the body; in other words, the symptoms are produced not by poisoning in the ordinary sense of the term, but rather by destruction—a destruction, it may be, of important nerve-centres.—Researches on silicon compounds and their derivatives: a new chlorobromide of silicon, by Dr. J. Emerson Reynolds, F.R.S. In purifying a large quantity of silicon tetrabromide prepared by means of crude bromine, the author has separated a portion boiling at 140°–141°, of the relative density 2·432, which analysis shows to be the chlorobromide of the formula $SiBr_3Cl$.

May 19.—Mr. Wm. Crookes, F.R.S., President, in the chair.—The formation of hyponitrites, by Prof. Dunstan and Mr. T. S. Dymond.—Ozone from pure oxygen, by Mr. W. A. Shenstone and Mr. J. Tudor Cundall.—The volumetric relations of ozone and oxygen: a lecture experiment, by Mr. W. A. Shenstone and Mr. J. Tudor Cundall. Soret and Brodie have shown that if v be the contraction produced on the electrification of a mass of oxygen, then $2v$ will represent the further contraction that will occur on absorbing the ozone formed by means of turpentine. If it be true that ozone completely dissolves in turpentine, this indicates that three measures of oxygen are concerned in the formation of two measures of ozone. The authors describe an apparatus which they have constructed for readily exhibiting Soret's observations to a class. The President said that he had been accustomed to join tubes *in situ* in the manner described by Mr. Shenstone. He added that it was possible to join together two different kinds of glass by means of a little soft white enamel, such as could be obtained from Powell's. Mr. Fairley had also joined tubes in the manner described by the authors; calling attention to Brodie's ozonizing apparatus, he remarked that the tube used by Brodie was probably thinner than was used by the authors. Dr. Armstrong thought that the results of the authors' experiments on the action of mercury on ozone were a valuable contribution to our knowledge of the influence of minute amounts of third bodies on the course of chemical change. He suggested that it was important, if possible, to determine the extent to which oxidation took place in presence of varying minute amounts of moisture, in order to ascertain if this exercised an influence comparable with that exhibited in Prof. H. B. Dixon's experiments on the rate of propagation of the explosive wave in a mixture of carbonic oxide and oxygen. Mr. Shenstone said that experiments such as were suggested by Dr. Armstrong, although very difficult with mercury, might probably be carried out with silver, which effected the decomposition of ozone with extraordinary facility. In reply to the question put by Mr. Page, he was quite unable to account for the peculiar condition assumed by the mercury when submitted to the action of the ozone. He had not been successful in joining tubes with the aid of the enamel spoken of by the President, but on the other hand had found it easy to join even combustion tubing to soft glass by means of an oxyhydrogen jet.—On the thermal phenomena of neutralization and their bearing on the nature of solution and the theory of residual affinity, by Mr. S. U. Pickering.—The action of metallic alkylates on mixtures of ethereal salts and alcohols, by Prof. T. Purdie.

Royal Meteorological Society, April 20.—Mr. W. Ellis, President, in the chair.—The following papers were read:—The storm and low barometer of December 8 and 9, 1886, by Mr. C. Harding. This gale will long be remembered as the one in which twenty-seven lives were lost in the lifeboat disaster off Formby through the capsizing of the Southport and St. Anne's lifeboats. The violence of the storm was felt over the whole of the British Islands as well as over a great part of the Continent of Europe, the force of a gale blowing simultaneously from Norway to Spain. The strongest force of the gale in the United Kingdom was experienced in the west and south-west, and the highest wind force recorded by any anemometer over the country was a velocity of eighty miles in the hour registered at Fleetwood, whilst at Valentia, Scilly, and Holyhead, the velocity reached seventy miles in the hour. The most exceptional feature of the storm was the extraordinary low reading of the barometer and the long time that the mercury remained at a low level. The absolutely lowest authentic reading was 27·38 inches at Belfast, and the barometer fell below 28 inches over a great part of England, Scotland, and Ireland. At Aberdeen the mercury was below 28 inches for eighteen consecutive hours, and below 29 inches for more than sixty hours, whilst in the north of England the barometer readings were equally exceptional.—Report of the Wind Force Committee, drawn up by Mr. G. Chatterton. In this Report, which is a preliminary one, the Committee have dealt mainly with that portion of the investigation relating to Beaufort's scale of wind force and the equivalent velocity in miles per hour.—A new form of velocity anemometer, by Mr. W. H. Dines. In this instrument an attempt has been made to measure the velocity of the wind by the rotation of a small pair of windmill sails, the pitch of the sails being altered automatically, so that the rate may always bear the same rates to that of the wind.—Description of two new maximum pressure registering anemometers, by Mr. G. M. Whipple.

May 18.—Mr. W. Ellis, President, in the chair.—The following papers were read:—Brocken spectres and the bows that often accompany them, by Mr. H. Sharpe. The author has collected all the original descriptions of the Brocken spectre, which is really the shadow of the observer cast by the sun upon clouds. In some cases the shadow is surrounded by a bow, which the author shows is like the rainbow in colour and in the order of colours. The head of a shadow is sometimes surrounded by another sort of phenomenon touching the head, and which the author names the "glory."—Results of thermometrical observations made at 4 feet, 170 feet, and 260 feet above the ground at Boston, Lincolnshire, 1882–86, by Mr. W. Marriott. These observations were made on Boston Church tower which rises quite free from any obstructions, in a very flat country, to the height of 273 feet. A Stevenson screen with a full set of thermometers, was placed 4 feet above the ground in the churchyard, a similar screen and thermometers was fixed above the belfry at 170 feet above the ground, while a Siemens electrical thermometer was placed near the top of the tower, the cable being brought down inside and attached to a galvanometer on the floor of the church, where the indications were read off. The results showed that the mean maximum temperature at 4 feet exceeds that at 170 feet in every month of the year, the difference in the summer months amounting to 3°; while the mean minimum temperature at 4 feet differs but little from that at 170 feet, the tendency, however, being for the former to be slightly higher in the winter and lower in the summer than the latter. As the electrical thermometer was read usually in the day-time, the results naturally showed that the temperature at 4 feet during the day hours was considerably warmer than at 260 feet. The author, however, detailed several sets of readings which had been made during the night as well as the day, the results from which were of a very interesting character.—Snowstorm of March 14 and 15, 1887, at Shirenewton Hall, near Chepstow, by Mr. E. J. Lowe, F.R.S.—During the evening the President made a presentation to Dr. J. W. Tripe of a silver tea and coffee service, which had been subscribed for by the Fellows in acknowledgment of the many services which he had rendered to the Society during a period of over thirty years.

EDINBURGH.

Royal Society, May 16.—Lord Maclaren, Vice-President, in the chair.—Prof. D'Arcy W. Thompson read a paper on the blood of *Myxine*, and also a paper on the larynx and stomach in *Cetacea*.—Mr. W. Peddie read a paper on the increase of electrolytic polarization with time; and another on transition resistance at platinum electrodes, and the action of condensed gaseous films. He showed that such resistance exists; that it gradually increases with the lapse of time after heating the plates to redness; and that it is due to the condensation of gas on the surface of the electrodes. The specific resistance of the condensed gases is probably of the same order as the specific resistance of ordinary dielectrics.—Prof. Crum Brown communicated a paper by Dr. A. B. Griffiths on the problematical organs of the Invertebrata, especially those of the *Cephalopoda, Gastropoda, Lamellibranchiata, Crustacea, Insecta,* and *Oligochæta*.—Mr. J. T. Cunningham gave an account of the nephridia of *Lanice conchilega*, Malmgren.—Prof. Tait informed the meeting that M. Amagat has succeeded in solidifying tetrachloride of carbon by pressure alone.

PARIS.

Academy of Sciences, May 23.—M. Janssen in the chair.—Obituary notices of the late M. Vulpian, by M. Bertrand, in the name of the Academy, by M. Charcot on behalf of the Section for Medicine and Surgery, and by M. Brown-Séquard on behalf of the Biological Society.—A general method of determining the constant of aberration, by M. Lœwy. At the moment of observation, when the two couples of stars are at the same height above the horizon, their common altitude, h, is determined by the formula:

$$\sin h = \cos \frac{\Delta}{2} \cos \frac{\Delta'}{2}.$$

Then, this quantity being known, a complete answer may be given to the questions as to the most rational values to be adopted for Δ and Δ' in order to obtain the greatest effect of aberration.—On the different states of tellurium, by MM. Berthelot and Ch. Fabre. It is shown that in passing from the amorphous to the crystalline state this element absorbs a certain quantity of heat; also that the precipitated tellurium, whether in presence of an alkaline liquid or an excess of hydrotelluric acid, corresponds to the state of the crystallized tellurium, but when precipitated by sulphurous acid it is altogether or mainly amorphous

The same phenomena have been observed with sulphur, showing a parallelism between the states of these two substances under the physical or chemical conditions determining those states.—Method for determining the specific activity of the intra-muscular exchanges, or of the coefficients of the nutritive and respiratory activity of the muscles in repose and at work, by M. A. Chauveau. The author here describes the technical processes adopted in carrying out the experiments, the results of which have already been communicated.—The earthquake of February 23, by M. Albert Offret. A summary description is given of all the seismic apparatus affected by the disturbance. With very few exceptions all those within the whole area of the earthquake yielded some indications, the interpretation of which is reserved for future consideration.—On the history of the Phylloxera of the vine, by M. P. de Lafitte. The existence is denied of the two distinct species determined and described in a recent communication by M. Donnadieu under the names of *P. vastatrix* and *P. pemphigoides*.—On Cremonian quadratic groups, by M. Autonne. Having in a previous paper considered the properties of an isolated quadratic Cremonian, the author here explains how such substitutions combine together to form Cremonian quadratic groups.—On a means of regulating and gauging the discharge of open canals, by M. H. Parenty. A theoretic solution is given of various problems connected with the discharge of open canals, with the view of determining automatically the quantity of water supplied in a given period, the total discharge at a given moment, the proportional discharge from one artery through several diverging rills, and similar questions.—On a general law for the vapour-tensions of dissolvents, by M. F. M. Raoult. By the researches here described the author arrives at the general law that one molecule of a non-saline fixed substance by its solution in 100 molecules of any volatile liquid diminishes the vapour-tension of that liquid by a nearly constant fraction of about 0·0105 of its value. The law is completely analogous to that announced by the author in 1882 regarding the lowering of the freezing-point of dissolvents.—On the compressibility of cyanogen compared with its refraction, by MM. J. Chappuis and Ch. Rivière. In order to complete their studies on the refraction of cyanogen and the comparison of the measured indices with the corresponding specific weights, the authors have undertaken the present researches on the compressibility of this gas, on which only a few imperfect data were incidentally supplied by Regnault.—On the polarization of copper by the extension of its surface in contact with a conducting fluid, by M. Krouchkoll. Lippmann having determined the polarization of mercury by increasing its surface in contact with a conducting fluid, the author has made a series of studies to ascertain whether the same phenomenon applies to the solid metals and to certain organic expansive substances, such as gelatine and coagulated albumen. The present note is confined to the study of copper in contact with distilled water, and with water containing 2 per cent. of ordinary sulphate of soda. The results of experiments with other ductile metals are reserved for a future communication.—Note on a stroke of lightning, communicated by the Minister of Posts and Telegraphs. A series of phenomena are described, which occurred during a thunderstorm at Mortrée (Orne), on April 24. Fragments of incandescent stones fell in large quantities, some about the size of a walnut, of a grayish-white colour, which crumbled between the fingers, emitting a distinct smell of sulphur. The others, which were of smaller size, looked exactly like coke. Some plaster was also detached from the front of a neighbouring house and transferred to the window of a house on the opposite side of the street. During another storm, on May 13, great havoc was done by the electric fluid at Eza (Maritime Alps), where it made a broad and deep fissure 20 metres long in the side of the mountain, detaching a solid mass measuring several hundred cubic metres.

Berlin.

Meteorological Society, May 3.—Professor von Bezold, President, in the chair.—Dr. Schultz spoke on the contrast between the popular names given to meteorological phenomena and their real nature as determined by means of instruments. Thus, for instance, the sirocco wind in Italy is spoken of as "heavy," whereas the barometer indicates a diminished pressure. Summers are spoken of as wet and dry, according as they are accompanied by much or little rain, without taking into account the usually opposed indications of the psychrometers; similarly our sensations of heat and cold are often directly opposed to the indications of the thermometer. The speaker further brought forward meteorological observations which he had made in Rome and the Riviera, and which showed occasionally, among other things, the anomaly that the temperature in the shade was higher than in the sun, especially when the thermometer in the sun was exposed to a strong wind. In the course of the elaborate discussion which followed upon the above communication, the President explained the larger part of the anomalies which had been described, and laid stress upon the difference between physical meteorology and the influence of temperature and moisture on the living organism. Alterations of atmospheric pressure have no effect on healthy human beings, although they must on sickly people, inasmuch as a diminution of pressure must lead, as a consequence, to an increased evolution of gases from the soil, and their accompanying miasmas. The idea of sultriness has not as yet been defined from a physical point of view; probably in connexion with this it should be borne in mind that the air is occasionally supersaturated with aqueous vapour, as shown in the experiments of Robert von Helmholtz, and that in this case a commencing condensation may be accompanied by a real evolution of heat. Prof. Schwalbe explained the conditions as to dampness, which had been brought forward by the speaker. Dr. Assmann explained, in connexion with this communication, an experiment which he had made with a view to determining the real temperature of the air, and which had given good results. The bulb of the thermometer was surrounded by a very perfectly reflecting cylinder of polished silver open below and closed above, but communicating by a lateral tube with an aspirator: by this method the air was drawn past the bulb of the thermometer in a constant current, while at the same time all external heat is prevented from reaching the thermometer by means of the reflecting cylinder. This thermometer indicates exactly similar temperature, both in the sun and in the shade. In conclusion, Dr. Sklarek mentioned experiments on the radiation of heat from the human body, which showed, in opposition to the laws of radiation from non-living bodies, that the human body radiates more heat from exposed parts of its surface, which are usually covered with clothes, when the difference of temperature between the skin and the surroundings is less than when it is greater. This anomalous behaviour may be explained by the supposition that, when the difference of temperature (between the skin and the surroundings) increases, the physical properties of the skin and its radiating powers undergo some change.

Physical Society, May 6.—Prof. Du Bois-Reymond, President, in the chair.—Dr. König spoke on Newton's law of the mixing of colours (see report on the meeting of the Physiological Society of April 29). In connexion with this, Prof. von Bezold communicated the fact that he had observed during his experiments on the mixing of colours, so-called neutral points in the spectrum, not merely when working with dichromatic, but also with normal trichromatic eyes. When, for instance, the intensity of a spectrum is greatly diminished (this may be most simply brought about by inserting a diaphragm with a small opening into the collimator) and a direct-vision spectroscope is used, then only three colours are seen at all—namely, red, green, and violet: between red and green and between green and violet there are neutral points. If the intensity of the light is still further diminished, then the neutral points undergo a change of position; the red extends to beyond the line D, and the neutral point at the line F moves in the opposite direction. This last fact was no longer recollected with any great exactness by the speaker, inasmuch as the experiments had been made many years ago, but the moving of the neutral point near D towards the green he described as existing without doubt. This appearance of the spectrum of light of small intensity was regarded by Prof. von Bezold as a proof of the truth of the Young-Helmholtz theory of colours. A second observation had reference to the mixing of colours with white. According to the Newton-Grassmann theory of the mixing of colours, every spectral colour, when mixed with white, must maintain its "tone" in the sense of the word as used by the French; this observation has, however, shown that not only red, but also violet, if mixed with white, takes on a purplish tone.—Prof. von Bezold made a further communication to the effect that Dr. Sprung had observed a series of notches on the curve of his barograph between six and seven o'clock on the morning of May 3, without any thunderstorm having taken place: the curves of a Bourdon aneroid barometer, and of the barograph at the Landwirthschaftliche Hochschule, showed the same irregularities. This irregularity of the curve of atmospheric pressure repeated itself on the morning of May 4 between 3 and 4 o'clock, but this time it coincided with a thunderstorm. The irregularity of the atmospheric pressure on May 3 acquires an especial significance, on account of the telegraphic news of the serious eruptions which took place in Mexico and California on the same day, although the time of the eruption is not yet definitely fixed. As a matter of fact, the barographic curve of May 3 shows a great resemblance to that observed at the time of the outbreak of Krakatão on August 27, 1883; the speaker produced the latter curve for comparison. It is not altogether impossible that the variation of atmospheric pressure on May 3, and possibly that of May 4, may have been in some way connected with the eruptions in America at the same time.

Physiological Society, May 13.—Prof. Du Bois-Reymond, President, in the chair.—Dr. Joseph communicated the results of his anatomical researches on the physiology of the spinal ganglia. According to Waller's older experiments, section of the nerves between the spinal cord and ganglion produces a degeneration of the central part of the nerve, whereas section of the nerve on the other side of the ganglion leads to a degeneration of all the sensory nerve-fibres up to the section. In 1883, however, a pupil of Gudden raised an objection to these experiments, since he found that, by removal of the connecting portion (between the cord and ganglion), not only the central but also the peripheral part of the nerve degenerated. Dr. Joseph has repeated these experiments on cats, and has arrived at the following results, which agree with those which Krause has recently communicated to the Society (see NATURE, May 12, p. 48). Thus (1) There are a number of nerve-fibres which simply pass through the ganglion without being connected with its cells. (2) The ganglion is the trophic centre for the larger number of sensory nerves. (3) The ganglion-cells are bipolar.—Dr. Lewin has examined a series of specimens of urine which contained blood, and were obtained from widely different cases, and found that most of them contained methæmoglobin, as shown by its characteristic spectrum. When these specimens of urine were reduced by means of sulphide of ammonium, he did not obtain the well-known spectrum of reduced hæmoglobin which is always obtained when blood which contains methæmoglobin is reduced; but in many cases he observed the no less well characterized spectrum of reduced hæmatin. It seems to follow from this that the urine of certain patients may contain hæmatin.—Prof. Zuntz gave an introductory explanation of an experiment which was subsequently carried out by Prof. Wolff, to show, namely, that anyone can diminish his weight by taking a deep inspiration. This experiment is most striking when the subject stands on a decimal balance which is so arranged that it can only give a kick upwards; in this case the pan with the weights in it sinks when a deep inspiration is taken. The speaker explained this phenomenon as being the result of the sudden straightening of the spinal column and elevation of the head which occurs when the deep inspiration is taken; owing to its momentum, the head carries the lower part of the body slightly with it, so that the latter presses less forcibly on its support.

STOCKHOLM.

Royal Academy of Sciences, April 13.—On the Lias of the province of Scania, in the south of Sweden, by Dr. J. C. Moberg.—A theory of unipolar induction, by Prof. E. Edlund.—Report on a visit to the United States and Canada for the purpose of studying the fisheries of those countries, by Dr. F. Trybom.—On the structure of the pericarp in the Boragineæ, by Miss A. Olbers.—On the development of the secondary fibro-vascular bundles in Dracæna and Yucca, by Miss H. Lovén.—A suggestion respecting the theory of the constant electric currents, by Dr. A. Rosén.—A crystallographic study of two new hydro-carbons, by Herr M. Bäckström.—Observations on natural phenomena of corrosion, and new faces of crystals in Adular from Swarzenstein, by Dr. A. Hamberg.—On tetartohedrism in tourmaline, by Dr. W. Ramsay.

May 11.—Contributions to a monograph of the amphipoda Hyperiidea, by Dr. C. Bovallius; part 1, the families Tyronidæ, Lanceolidæ, and Vibilidæ.—On the recent Astro-photographic Congress in Paris, by Prof. Hugo Gyldén.—On a group of differential equations, the solution of which is combined with so-called small divisors, by Dr. C. Bohlin.—On the results of the determinations of the longitude between Stockholm, Gothenburg, and Lund, undertaken during 1885 and 1886, by Prof. Rosén.—On the levellings conducted during 1886, by the same.—On the interior friction of dilute aqueous solutions, by Dr. S. Arrhenius.—Contributions to knowledge of the changes of steel in physical respects when softened, by Herr C. F. Rydberg.—On the diffusion of ra ing heat from spherical surfaces, by Dr. K. Ångström.—On electric resistance against conductibility in crystals, by Her Bäckström.—On acollection of Coleoptera and Lepidoptera the Congo, made by Lieut. Juhlin-Dannfelt, and descr by Prof. C. Aurivillius.—The following papers by Nilsson and Dr. G. Krüss, of Munich, were presented:—the equivalent and atomic weights of thorium.—On the ea and the niobic acid in fergusonite.—On the product of reduction of niobfluorkalium with natrium.—On the Ger fluoride of kalium.—Studies on Taphrina, by Dr. C. J. Johans—On the species of Echinoidea, described by Linnæus in work "Museum Ludovicæ Ulricæ," by Prof. Sven Lovén.—some definite integrals, by Dr. Lindman.—On organic su amido-combinations, by Prof. Cleve.—On naphthydroxam ac by Dr. A. G. Ekstrand.—On the crystals of some combinat of zirconium, by Dr. M. Weibull.—*Lagopus bonasioides* hybrid between *Lagopus subalpina* and *Tetrao bonasia*, by H G. Kolthoff, Conservator of the Zoological Museum of Upsa

BOOKS, PAMPHLETS, and SERIALS RECEIVE

Cartilla de Zoologia Evolucionista: M. R. Mexia (Jacobsen, Bu Aires).—The Health of Nations, 2 vols.: B. W. Richardson (Longmans Proceedings of the Bath Natural History and Antiquarian Field C vol. vi. No. 2 (Bath).—La Matière et l'Energie: E. Ferrière (Alcan, Pa—Life of Charles Darwin: G. T. Bettany (Scott).—Report of the Com sioner of Education for 1884-85 (Washington).—Illustrations of the Bri Flora: Fitch and Smith (Reeve).—Essays and Addresses: Rev. J. Wilson (Macmillan).—Climatic Treatment of Consumption: Dr. J. Lindsay (Macmillan).—Elementary Practical Histology: W. Fear (Macmillan).—Alcyonida: D. C. Danielssen (Grondhal and Son).—A Basis for Chemistry: T. Skerry Hunt (Trübner).—Sketches of Life in Jap Major H. Knollys (Chapman and Hall).—Cosmogonie: C. Braun (Münst Beiträge zur Kenntnis der Nosean-führenden Auswurflinge des Laa Sees (Hölder, Wien).—Annalen der Physik und Chemie, No. 6, (Leipzig).

CONTENTS. PA

THURSDAY, JUNE 9, 1887.

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 Capt. George S. Nares, R.N., F.R.S., and of the late Capt. Frank Tourle Thomson, R.N.* Prepared under the Superintendence of the late Sir C. Wyville Thomson, Knt., F.R.S., &c., and now of John Murray, one of the Naturalists of the Expedition. Zoology—Vol. XVIII. Parts 1 and 2, with a Volume of Plates. (Published by Order of Her Majesty's Government, 1887.)

VOLUME XVIII. of the Zoological Reports of the *Challenger* Voyage well merits to be called enormous, as it contains no less than 1800 pages. It contains but a single memoir, "On the Radiolaria," by Prof. Ernst Haeckel, of Jena, and is accompanied with a volume of 140 plates.

A great work like this demands more than a passing notice, for even in this age of scientific labour one stands amazed at the physical energy, not to refer to the scientific knowledge, that could have accomplished such a result. Ten whole years of the author's life were devoted to this monograph, which will ever be a worthy monument of a most enduring kind.

Some fifty years ago Meyen, and shortly after Ehrenberg, first described some forms of Radiolaria. Meyen has the merit of having observed and noted the first of these curious forms in a living state, but to Prof. Huxley we are indebted for the first accurate observations on some kindred forms met with by him during the voyage of the *Rattlesnake* in the tropical seas. Ehrenberg no doubt was the first to call attention to the exceedingly great numbers of forms that were to be found in the group, but although he was not ignorant of the researches of his colleague, Johannes Müller, whose memoirs were published in the same Academy's Transactions as his own, he never seems to have paid the slightest attention to them, nor does he even allude to the name given to the group by Müller, that of Radiolaria, by which they are now known.

Just twenty-five years ago Haeckel published his well-known "Monograph of the Radiolaria," which with its splendid atlas of plates, was, and is still, an indispensable work for the student. In this all the species known either by figures or descriptions were reviewed, and arranged in 15 families and 113 genera, of which latter 46 were new; the number of forms observed alive amounted to 144, most of which are figured, in a manner that has not, we think, been equalled, certainly not surpassed.

In 1862, Zittel described the first fossil Radiolaria from the chalk; in 1876, John Murray established the family Challengerida; and above all, in 1879, Richard Hertwig showed the essential differences in the formation called the "central capsule," and in accordance therewith divided the Radiolaria into six orders. From this on, with the exception of the various important works on the fossil forms by Emil Stohr, Dante Pantanelli, Butschli, Dunikowski, and D. Rust, the whole record has been filled in by Haeckel, and it has been almost exclusively based on the collections of the *Challenger*.

These Radiolaria, or Capsulate Rhizopoda, form a peculiar class of the Rhizopoda—Haeckel's "Protista." This class is exclusively marine, and, while possessing many of the features of the Rhizopods, differs from them in the possession of a peculiar "membrane" dividing the cell-body into two distinct parts—the "central capsule" or the internal part with the nucleus, and the external part or "extra-capsulum" with the calymma; the protoplasm of both parts communicates through fine pores, which pierce the capsular membrane. The central capsule is composed of three essential parts, viz. the central nucleus, the intra-capsular sarcode, and the capsule membrane. Besides these elements, the central capsule contains very commonly an internal skeleton, fat and pigment granules, crystals, and vacuoli. The outer part of the Radiolarian body is also constantly composed of the calymma, or a thick extra-capsular "jelly-veil." The matrix or maternal tissue of the external protoplasm and the pseudopodia again very commonly contains fat and pigment granules, the skeleton and vacuoli, and, in addition, "xanthellæ" or "zooxanthellæ," peculiar yellow cells which contain starch, and are unicellular yellow Algæ living as "symbiontes" in true symbiosis with a great number of Radiolaria. The skeleton may be either siliceous or acanthinic, and is sometimes wanting. The four sub-classes, as described in this Report, contain 20 orders; and these, 85 families, which include 739 genera, with 4318 species, of which latter 3508 are described as new.

Radiolaria occur in all the seas of the world, in all climatic zones, and at all depths. Probably under normal conditions they always float freely in the water, whether their usual position be at the surface or at a certain depth or near to the very bottom of the sea. Hitherto, no observation has been recorded which justifies the assumption that Radiolaria live anywhere upon the bottom of the sea, either attached or creeping. However able they may be to creep when they fall on a solid basis, they seem normally always to float freely in the water, with pseudopodia radiating in all directions.

As regards their local distribution and its boundaries, the Radiolaria show in general the same relations as other pelagic animals. Since they are only to a very slight extent, if at all, capable of active horizontal locomotion, the dispersion of the different species from their points of development is dependent upon oceanic currents, the play of winds and waves, &c. These passive migrations are here, however, as always, of the greatest significance, and bring about the wide distribution of individual species in a far higher degree than any active wanderings could do. Anyone who has ever followed a stream of pelagic animals for hours, and seen how millions of creatures closely packed together are in a short time carried along for miles by such a current, will be in no danger of under-estimating the enormous importance of marine currents in the passive migration of a marine fauna. The number of cosmopolitan species which live in the Pacific, Atlantic, and Indian Oceans is relatively large. In each of three great ocean basins, too, many species show a wide distribution. On the other hand, there are very many species which are known only from one locality, and probably many small local faunas exist, characterised by the special development of particular groups. From the very richness of the material, Prof. Haeckel has found it impossible to work out completely the local distribution of all the species.

From the tropics the abundance of species seems to diminish regularly towards the Poles, and more rapidly in the northern than in the southern hemisphere; the latter also appears to possess more species than the former: a limit to Radiolarian life towards the Poles has not yet been found. The greater abundance of Radiolaria in the tropical seas is to be accounted for by the more favourable conditions of existence, rather than by any difference in temperature. One station (271) of the *Challenger* Expedition, situated almost on the Equator, in the Mid-Pacific, exceeds all other parts of the world hitherto known in respect of its wealth of these forms; and more than 100 new species are described from it. The fauna of the Pacific Ocean exceeds that of both the Indian and Atlantic, but the fauna of the Indian Ocean is that least known.

In reference to the bathymetrical distribution, it seems certain that numerous species of this class are found at the most various depths of the sea, and that certain species are limited to particular vertical zones, and are adapted to the conditions which obtain there. In this respect three different Radiolarian faunas may be distinguished—the "pelagic," "zonarial," and "abyssal." More than half of all the species known as recent belong to the last fauna.

The chapter on the geological distribution is full of interest. Radiolaria are found fossil in all the more important groups of the sedimentary rocks of the earth's crust. Whilst a few years ago their well-preserved siliceous skeletons were only known in considerable quantity from Tertiary marls, very many are now known to occur in Mesozoic, and a few in Palæozoic, strata. By the aid of improved modern methods of research, it has been shown that many hard siliceous minerals, especially cryptocrystalline quartz, contain numerous well-preserved Radiolaria, and sometimes these are composed almost entirely of closely compacted masses of such siliceous shells. The Jurassic quartzes (Switzerland), as well as the Tertiary marls (Barbados) and clays (Nicobar Islands), may be regarded as "fossil Radiolarian ooze"; and, since specimens have also recently been found both in Silurian and Cambrian strata, it may be inferred that Radiolaria are to be found in all fossiliferous sedimentary deposits, from the oldest to those of the present day. Among the Miocene Radiolaria, numerous species are not to be distinguished from the corresponding still living forms. On the other hand, those genera which are rich both in species and individuals (recent as well as fossil) present continuous series of forms which lead gradually and uninterruptedly from old Tertiary species to others still living, which are specifically indistinguishable from them.

As Chapter XI. of the introductory portion of the Report, Prof. Haeckel gives a very valuable account of the progress of our knowledge of the Radiolaria from 1862 to 1885. In his earlier monograph he had already given a critical discussion of the works which had appeared prior to 1862: we find here a full list of the publications from 1834 to 1884, in which list a little of the author's old trenchant style of criticism breaks out; for he has heaped together in an appendix, to which he gives a somewhat needlessly offensive name, "all the absolutely worthless literature, which contains either only long-known facts or false statements, and which may therefore be entirely neglected with advantage." While we would not, for all the Radiolaria in the sea, give the list of this "foul" literature, we may relieve the reader's mind by at once mentioning that the name of Ehrenberg does not appear in it, and that the value of the laborious works of the great but too self-reliant German in this field meets with all proper appreciation.

The unicellular nature of the Radiolaria was first established by Richard Hertwig in 1879, and was brought by him into conformity with our present histiological knowledge and the new reform of the cell-theory. Huxley, who was the first to examine living Radiolaria with any accuracy, declared, so long ago as 1851, that *Thalassicolla nucleata* was a unicellular Protozoon. Later, both Johannes Müller (1858) and Haeckel (1862), recognising the peculiar "yellow cells" which occur in many Radiolaria, in large numbers, as true nucleated cells, thought it impossible to maintain this view; and it was not until Cienkowski (1871) and Brandt (1881) had shown that these "yellow cells" did not form part of the Radiolarian structures, but are symbiotic unicellular Algæ, that it was possible to revive and demonstrate anew the unicellular nature of the Radiolaria.

From a morphological stand-point the individuality of the unicellular elementary organism is obvious in the solitary Radiolaria (Monobia); and the whole body with all its constituent parts, and not merely the central capsule, is to be looked on as a cell. But this unicellular organisation must be noted as differing from that of all other Protista, inasmuch as an internal membrane (capsule membrane) separates a central from a peripheral portion.

The membrane of the central capsule is invariably present at one period or another of the life of the organism. Karl Brandt, indeed, has recently stated that in some forms it is absent; but Haeckel has recognised its presence in over one thousand species, and even in some of those in which Brandt was unable to find it. It is often very delicate and may easily be overlooked, though the application of the proper reagents will render it always discernible. Those Radiolaria in which for a time it is absent are young of species in which the membrane is only formed immediately before sporification, and persists but for a short time.

All Radiolaria possess a nucleus, but they present two different conditions in respect of its behaviour, since in their young stages they are uninuclear, and in later stages they are multinuclear. Before the formation of swarm-spores the nucleus divides into many nucleoli. Thus the nucleus is pre-eminently the organ of reproduction and inheritance. The division of the originally single nucleus into many small nuclei may take place at very different periods, so that Haeckel divides the Radiolaria into the "precocious" and the "serotinous." Into the subject of the skeleton formation and that of phylogeny the space at our command will not allow us to enter; it will suffice to say that they are treated at great length and with consummate skill.

In such an onerous task as that of describing this mass of varied forms, one always, as the author observes, "runs the risk of either doing too much or too little in the way of creating species"; but he contents himself with the reflection that in the light of the theory of descent this danger is of little consequence.

In the carrying out of the troublesome duty of making the many thousands of required measurements, the author gratefully thanks his friend Dr. Reinhold Teuscher, of Jena, for the patient and careful manner in which he performed this part of the work. The figures of new species, about 1600 in number, which appear in the atlas accompanying the Report, were nearly all drawn with the camera, partly by Mr. A. Giltsch and partly by Prof. Haeckel; but the former has drawn all the figures on the stone in a very masterly way, so that the illustrations present a splendid series of beautiful forms, like the stars in the firmament for number, and surpassing these in the wonderful diversity and complexity of their outlines.

It was indeed a fortunate circumstance that so distinguished a naturalist, with such an intimate knowledge of the Radiolaria, should have been willing to undertake such a task, and exceedingly fortunate for science that he should have been enabled thus to finish it.

A GERMAN TREATISE ON THE VEGETABLE KINGDOM.

Die natürlichen Pflanzenfamilien. Von A. Engler und K. Prantl. (Leipzig: Engelmann, 1887, &c., being issued in numbers at irregular intervals.)

WITH the first three numbers of the above-named work, which are now before us, Profs. Engler and Prantl have embarked on an enterprise of some magnitude. The editors, having recognized the want of a comprehensive treatise, in German, on the Vegetable Kingdom, which should at once be scientifically sound, and yet be written in a style suitable for the use of those who are not professed botanists, have determined to meet that want. With this object they have enlisted the assistance of a number of collaborators: anyone who is conversant with the literature of Botany produced in Germany in recent years will see, on reading the list of names, that Dr. Engler has secured the co operation of a very powerful staff, including many of the most prominent representatives of the science in that country.

With their aid the editors propose to produce a work, extending to some 5000 pages octavo: the whole is to be divided into five parts, one of which will be devoted to the Cryptogams, under the editorship of Prof. Prantl; one will treat of the Gymnosperms and Monocotyledons, and the remaining three of the Dicotyledons, under the editorship of Prof. Engler. The production of the several parts will proceed simultaneously, and they will appear in numbers, at intervals during the next five or six years: thus the distribution of the cost (which in itself is not excessive, considering the quality and extent of the work) over a lengthened period, will bring the book within the reach of a wide constituency.

The first three numbers will give some idea of what will be the scope and character of the work as a whole. One of these is the first instalment of the *Palmæ*, by Dr. Drude. It opens with twenty-six pages of text, illustrated by numerous carefully chosen and excellent woodcuts, on the morphology and anatomy of the vegetative organs, the inflorescence, fruit, and seed of the plants of this order; then follow notes on the distribution, affinities, and uses of the family, and finally its classification. A detailed description of the genera succeeds this general treatment, and it is illustrated by numerous good figures representing the habit of the plants, and dissections of their flowers: this will, in fact, be somewhat like an illustrated and abbreviated "Genera Plantarum," written in German, and in a semi-popular though sound style.

The second number issued contains the *Juncaceæ*, by F. Buchenau, and the *Stemonaceæ* and *Liliaceæ* by Dr. Engler. The subject-matter is treated in the same spirit as the above, and it may be assumed that this method will be pursued throughout the whole work.

But a more special interest attaches to the number which was third in its order of issue; and that on two distinct grounds: first, because from it we gain a more general idea of the plan and scope of the work, and secondly because it is chiefly the work, and probably the last work, of the late Prof. Eichler, a botanist whose loss will be very widely felt (see NATURE, vol. xxxv. p. 493).

The first pages of this number, written by Dr. Engler, give in brief the general plan of the whole work; the main lines of classification being those in common use, though some of the terms used have not as yet been generally accepted. They are as follow:—

I. Mycetozoa.
II. Thallophyta:
  (*a*) Schizophyta.
  (*b*) Algæ.
  (*c*) Fungi.
III. Embryophyta zoidiogama (= Archegoniatæ):
  (*a*) Bryophyta.
  (*b*) Pteridophyta.
IV. Embryophyta siphonogama (= Phanerogamæ):
  (*a*) Gymnospermæ (= Archispermæ).
  (*b*) Angiospermæ (= Metaspermæ).

Then follows the general treatment of the Gymnospermæ, of which four classes are distinguished, the Cycadinæ, Cordaitinæ, Coniferinæ, and Gnetales. It is worthy of note that here the fossil forms are taken into account, and Cordaitinæ, as well as fossil forms of the Cycadaceæ and Coniferæ, are described in their proper places. It will be unnecessary after what has already been said to follow the mode of treatment of the Gymnosperms further; suffice it to say that, while due prominence is given to the external morphology and classification, the results of recent investigation on the development of the sporangia and embryo find a place, *e.g.* those of Treub and Warming on the Cycads, and of Strasburger and others on the Coniferæ. A peculiar interest will attach to the pages on the morphology of the female cone in the Coniferæ, since this will be the last expression of the opinion of Eichler on a subject to which he had devoted special attention.

While extending a welcome to this new enterprise, we may compare it with other undertakings of a somewhat similar nature. Among the comprehensive classificatory works of recent years, the most prominent is the "Genera Plantarum" of Bentham and Hooker; but the most ardent admirers of that solid book could not expect it to appeal to the laity: it is designed for the use of specialists, and they alone will use it. Between this and the illustrated text-books intended for students there has been hardly any intermediate in this country, though Lindley's "Vegetable Kingdom," a book which still holds its place as a classic, served in the past a part not altogether unlike that which Dr. Engler's book may be expected to serve in the future. It is, however, in France that the nearest approach has been made to the idea of Dr. Engler. In the "Traité de Botanique" of Le Maout and Decaisne we have a volume profusely illustrated, and dealing with the vegetable kingdom as a whole: the English translation of this, edited by Sir J. D. Hooker, is familiar to all British botanists. Again, the "Histoire des Plantes" of Baillon, which is still in progress, is a classificatory work of large size, well illustrated as regards external morphology, but somewhat deficient in description of the internal details: his "Dictionnaire de Botanique," which commenced in 1876, is also still in progress, and covers, in dictionary form, much the same ground as his "Histoire." These are, then, the chief illustrated and descriptive works with which Drs. Engler and Prantl will have to compete. If we may judge from the first three numbers, the competition, though keen, will be in favour of the new enterprise, and that chiefly on the ground that the authors of it take a more general view of the subject. They do not confine their task to the description and delineation of external form, classification, and distribution. While giving due prominence to these branches, they also incorporate the results of recent investigations of anatomy and development. F. O. B.

OUR BOOK SHELF.

Nomenclature of Colours for Naturalists, and Compendium of Useful Knowledge for Ornithologists. By Robert Ridgeway, Curator, Department of Birds, United States National Museum. Ten coloured plates, and seven plates of outline illustrations. (Boston: Little, Brown, and Co., 1887.)

THIS will be a very welcome volume to naturalists in general, and ornithologists in particular. We do not know that everybody will agree with the principles laid down by the author, but he has, at all events, brought together a considerable number of colours, and given them very definite names for purposes of comparison, and a mere glance at the coloured plates will show how very important it is that every variety of green shown in Plate 10, for instance, should have its special name and admit of easy reference.

The comparative vocabulary of colours, which occupies a considerable proportion of the first part, is also a very valuable combination, and should be in the hands of naturalists of all civilized countries, as we get the English, Latin, German, French, Spanish, Italian, Norwegian, and Danish equivalents of all the colours shown in the coloured plates, and a great many more.

The *pièce de résistance* in the part of the volume which has been prepared chiefly for the use of ornithologists is a glossary of technical terms. It seems to us to have been very carefully done. A study of the plates illustrating the various feathers of birds, and the various birds' eggs, with the attached nomenclature, is certain to lead to a gradually increasing care in description. There is no doubt that the book will prove of very great value to many naturalists.

English Tobacco Culture, &c. Edited by E. J. Beale, F.L.S. (London: E. Marlborough and Co., 1887.)

THIS little book will serve as an important guide to farmers in conducting experiments in the cultivation of tobacco. It gives a detailed account of the origin of the movement for determining whether tobacco could be relied upon as a farm crop in Great Britain, and, if so, whether it could be cultivated to yield a profit to the grower. These two questions, it is maintained, have been answered in the affirmative by the results of last year's experiments, but this conclusion is founded more upon the appearance of the plants than upon actual results in the production of good commercial tobacco.

Seventeen varieties of tobacco were grown last year in this country, and a full description is given of the plants of each variety, with well-executed illustrations, showing the general appearance and distinctive features of the fully-developed plants. For each description of tobacco grown an "Estimated Balance Sheet" has been prepared, and the anticipated profit, amounting in some cases to as much as £25 and even £27 per acre, is very encouraging for farmers who may think of undertaking experiments in tobacco cultivation.

Perhaps the most useful part of the book is that devoted to directions for conducting the several operations of tobacco culture. These include the preparation of the land; the sowing of the tobacco seed; the transplanting of the young seedlings; the transferring of the plants to the prepared ground; and their subsequent treatment until finally harvested and cured. Altogether the book is prepared with great care, and its publication at the present time is very opportune.

Life of Charles Darwin. By G. T. Bettany. (London: Walter Scott, 1887.)

THIS is one of the series of volumes entitled "Great Writers." It was not to be expected that Mr. Bettany would be able to tell us anything absolutely new about the illustrious man of science concerning whom so much has already been written. He has, however, succeeded in presenting in a bright and attractive style the leading facts of Darwin's career, and he has done good service by taking pains to show that Darwin was not only a great thinker and discoverer, but a man of a singularly pure and noble character. Mr. Bettany's exposition of the results of Darwin's labours is brief, but clear and accurate, and he tries to mark as distinctly as possible the various stages in the process by which the theory of evolution as Darwin conceived it was itself evolved.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Thought without Words.

MAY I demur to the Duke of Argyll's statement that monkeys and dogs have no true reasoning powers? Long and careful attention given to the action of animals consequent on true reasoning power, has led me to an opposite conclusion. I do not trouble you with instances, or could give very many; and I have frequently seen reasoning power exercised after obvious thought over the best course to pursue. Then, *are* animals speechless among themselves? I think not, and believe they speak freely to one another at needed times, in their own language. And I certainly with my own domestic animals can understand in a certain sense their language. I clearly know hat they ask for, or what they wish to call my attention to, om the tone of the voice and its modulations, and this is, I sume, language as regards them. On the main question, I ould hold with Prof. Max Müller from my own personal xperience. H. STUART WORTLEY.

South Kensington Museum, May 21.

I HAVE just noticed in a recent number of NATURE (May 12, . 28) a letter from Mr. Francis Galton, in which he endeavours prove that thought without words is by no means an impossibility. May I advance a small amount of confirmatory vidence which must, I think, have come within the notice of ost people? This evidence is to be found in that peculiar state f mind produced when, as we say, we have a word "on the p of the tongue." In this case the *idea* which the word, when und, will represent is most vividly present to the mind, but it an idea only. No language is needed to make it recognizable ven though, as oftens happens, the idea may be of the most omplicated and abstract kind. HAROLD PICTON.

May 31.

Diatoms in the Thames.

IN NATURE, vol. xxxii. p. 223, you were good enough to ublish a note from me respecting the occurrence in great proasion of small gelatinous bodies in the water surrounding the sle of Sheppey. The same conditions prevailed at about the ame time last year, and in all probability will reappear at the atter end of this month.

I have now to record that since the middle of April the sea ereabout has been what fishermen call "foul" from another ause. While the water has been unusually clear, in it have een floating an enormous quantity of diatoms. The most bundant is *Coscinodiscus concinnus*, the large disks of which an be seen by the naked eye in any sample of sea water dipped t random. Indeed in bright sunlight they can easily be bserved in the sea itself. The other forms are *Rhizosolenia* *etigera*, and *Eucampia zodiacus*.

At low water the sands lying between the Thames and the Iedway have been coloured a rich dark brown by the diatoms eft stranded there.

The effect on marine life seems to have been somewhat varied. Iollusks appear to have thriven on the abundant food; and as hrimps and whitebait have been found in abundance in their sual haunts, it may be presumed that they have not been much nnoyed by the diatoms. On the other hand, the flat fishes have een greatly disturbed, and could not be found on the banks sually frequented. Some fishermen said they had gone right way, and would not return till the water ceased to be "foul." et this could hardly have been the case, as some have since een caught on the Essex flats gorged with young cockles.

During the past fortnight I have examined the water at various oints around Sheppey, and have invariably found the diatoms. n using the tow-net during this period, I have been struck by e scarcity of animal life. Besides the diatoms, a few Noctiucæ, larval Spios, and two Isopods were all that I noticed. hat at least some diatoms are obnoxious to fish was settled by Ir. Pearcey, who, in conducting tow-netting investigations in e Shetland Isles in 1884, found that in regions where large oating banks of the diatom *Rhizosolenia shrubsolii* (Cleve) ccurred animal life was almost entirely absent; and Mr. Isaac . Thompson, of the Liverpool Marine Biological Society, has ecorded a somewhat similar experience in 1885 off the North Vales coast.

It will be interesting to ascertain from which direction these ountless myriads of diatoms have reached the Thames, and ithin what limits they have been found. To this end I invite bservers round the British coast to examine the water in their espective localities, and to publish the result.

In water obtained from the coast of Holland I could not etect a single diatom.

I have reason to believe that an abundant influx of the same haracter has taken place in previous years, but coming at a me of year when the weather is not often favourable for conucting marine observations, the facts have escaped scientific otice. W. H. SHRUBSOLE.

Sheerness-on-Sea.

P.S.—The above was written about a fortnight ago, and now Iay 18) the gelatinous masses are beginning to appear.—V. H. S.

The Structure of the Nostochineæ.

I WAS glad to see in NATURE, vol. xxxv. p. 594, a suitable notice of Prof. Borzi's very interesting paper on the above subject. So far as regards the discovery of the continuity of the protoplasm in this group of plants, I should like to be allowed to state that in my paper "On the Constitution of the Cell-wall and Middle Lamella," read February 10, 1884, and published in the Proceedings of the Cambridge Philosophical Society, vol. v. part ii., I drew attention to the fact that in *Nostoc* I had observed a continuity of the protoplasm between adjacent cells. But I simply stated the bare fact, and my note was therefore even more pronouncedly "una brevissima comunicazione" than that of Wille's on *Stigonema*, to which Prof. Borzi refers.

Clare College, Cambridge. WALTER GARDINER.

Curious Phenomenon in Capillarity.

FOR some years past I have been in the habit, when putting up at obscure hotels and remote "dâk bungalows" during inspection tours, of putting a few drops of the cheap disinfectant known as "Little's soluble phenyle" into my tub before bathing. The bulk of the liquid, when dropped into clear water, diffuses downwards as a milky white emulsion, giving beautiful imitations of inverted cumulus clouds; but a small portion of it, perhaps some oily impurity in the mixture (which is sold under the trade mark C_6H_5, and should therefore presumably be a definite compound), instantly spreads out over the surface as a drop of oil would do, and then, strange to say, after the lapse of about half a second, and usually before the film has extended more than half-way across the tub, it again contracts. The contraction of the film proceeds until it is only two or three inches in diameter, after which its size appears to remain stationary; but about this time the distinct outline of the film usually disappears, owing to the gradual mixing of its substance with the water below—a circumstance which leads me to believe that the film is not caused by an oily impurity, but by a part of the "phenyle" itself, which possesses the property of emulsifying with water. Temperature seems to have no effect on the phenomenon, beyond perhaps modifying the rate at which the film expands and contracts, the effect being apparently exactly the same whether the liquid be added to a cold bath at 60° or to a hot one at 100° F.

Have any of your readers observed this phenomenon, or can anyone give a satisfactory explanation of it? According to the usual theory of the subject, the surface-tension of water in contact with air is greater than the tension of a phenyle-air surface *plus* that of a phenyle-water surface, and hence the film of phenyle spreads like one of oil. But after a time, when the phenyle gets partially emulsified, the sum of the tensions of the two phenyle surfaces must be greater than that of the water surface to make the film contract, and apparently after some further time a condition of equilibrium is established. Is there anything in the process of emulsification, or dividing a liquid up into minute globules suspended in another liquid, that will account for these changes of surface-tension?

Naini Tal, India, May 2. S. A. HILL.

Sense of Taste or Smell in Leeches.

I HAVE recently observed very well marked phenomena, similar to those described by Dr. C. O. Whitman (*Quart. Journ. Micro. Science*, vol. xxvi. new series, p. 409). I picked up with my fingers a stone from the soft muddy bottom of a shallow, torpid stream. Returning to the same spot a few minutes afterwards, I noticed a number of leeches (apparently *Hirudo* sp.) swimming near the spot. On the following day, suspecting that they had "smelt" or "tasted" my hand in the water, I first stirred the surface of the mud with a stick, but no leeches appeared; after the water was clear again I "washed my hands" in the water without disturbing the mud, and very soon a number of leeches came up and swam about. The soft mud in which they live is about a foot deep, and although the disturbance of the surface mud with a stick was not sufficient to bring them out, the "smell" or "taste" of my hands seems to have spread down and extended over an area of more than a yard.

Last year I had an opportunity on these hills of observing the very keen "scent" of the land leeches, who will come towards one's self or one's horse from the banks on either side of even a wide road. A. G. BOURNE.

Ootacamund, Nilgiris, April 11.

Lisping.

A CLERGYMAN, with usually an exceptionally distinct utterance, was observed one Sunday morning at the beginning of the service to speak with a pronounced lisp. After a time it wore off, and his speech became as clear as usual. Has it ever occurred to anyone what a very simple thing may cause a lisp? The case in question was owing to a tiny slice of lozenge sticking to the roof of the mouth just to the left of, and close to, the front tooth. This almost imperceptible impediment was sufficient to render the speech so indistinct as to resemble a marked lisp. Of course as the lozenge dissolved the lisp became no longer observable, and the speech assumed its ordinary clearness.

These being the facts, the question that occurs to every thoughtful mind is, If the cause of lisping be so simple, why cannot the remedy be as simple and yet effectual?

The answer I leave to be supplied by some of your scientific readers.

A NON-LISPER.

ETIOLOGY OF SCARLET FEVER.[1]

AMONG the infectious or zymotic diseases there are two at any rate (namely, scarlet fever and diphtheria) of which it may be said that their spread is to a lesser extent dependent on defective domestic sanitation than is the case with some of the other zymotic diseases, as, for instance, typhoid fever. Indeed, it is maintained by competent authorities that scarlet fever and diphtheria do not invade the houses of the poor with faulty sanitation with greater frequency or with greater severity than those of the well-to-do, however perfect the sanitary arrangements. This view is based on the important experience gained during the past twenty years, viz. that epidemics of scarlet fever and diphtheria have been brought about by milk. I may here state by way of explanation that a fact well established, and needing no further comment, is that scarlet fever and diphtheria are, like small-pox, measles, whooping-cough, and typhus fever, communicable directly from person to person. This mode of infection, doubtless an important one, and coming into operation in single cases wherever the elementary rules of isolation and disinfection are transgressed, altogether sinks into insignificance when compared with the infection produced on a large scale, if a common article of diet like milk should become in some way or another the vehicle of contagium, as has been proved to be the case in a number of epidemic outbreaks. These epidemics, known as milk scarlatina, milk diphtheria, and I may add also milk typhoid, have this in common, that almost simultaneously, or at any rate within a short time, in a number of houses, having no direct communication by person or otherwise with one another, there occur sometimes singly, sometimes in batches, as it were, cases of illness: scarlet fever, diphtheria, or typhoid fever as the case may be. And it was this peculiar character which pointed to a condition which must have been common to all these households. On closer examination it was indeed found that all these households had this, and only this, in common, that they were all supplied with milk coming from the same source—that is to say, from the same dairyman. Other houses supplied with milk from a different source escaped; and further it was shown that, as soon as the consumption of the suspected milk ceased also, the epidemic, as such, came to an end, except of course the cases due to secondary infection from person to person. The Medical Department of the Local Government Board have had for years past their attention fixed on these milk epidemics, and in the Reports of the Medical Officer many of these are described with great detail; amongst these, Dr. Ballard's Report in 1870 on enteric fever in Islington, Dr. Buchanan's in 1875 on an outbreak of scarlet fever in South Kensington, and Mr. Power's on an outbreak of scarlet fever in St. Giles and St. Pancras in 1882, are specially to be referred to. Mr. Ernest Hart has tabulated all the outbreaks of milk epidemics that have been investigated until 1881, in vol. iv. of the Transactions of the International Medical Congress for 1881. Now, analyzing these outbreaks as far as they refer to scarlet fever, there are several of them where the assumption that the milk acquired the power of infection by contamination from a human source cannot be excluded. This infection if proven would stand on the same footing as if due to contagion from person to person, for it is clear whether the contagium is conveyed from one person to another by air, food, drink, or other articles, it always remains contagion from person to person. Now, in some of the epidemics tabulated by Mr. Hart, and recorded by subsequent observers, *i.e.* after 1881, this mode of milk contamination cannot be excluded, as I said before; but comparing the dates when the milk might be supposed to have become so contaminated with the dates when the milk has actually produced infection, it will be found that a certain discrepancy exists, and as will be shown later another mode of infection, viz. from a person affected with scarlatina to the cow, and through the cow to the milk and then to human beings, cannot be excluded either. There are other epidemics recorded in these tables, in which the mode of infection of the milk is not ascertained; and in a third set, the milk acquired infective power in some way or another, but certainly not from a human source. As an illustration of the first group of epidemics, *i.e.* probable contamination from a human source, I will refer to the table given by Mr. Ernest Hart on page 539:—

1881, April.	Keswick.	J. Robertson, M.D., M.O.H.	A dairy closely adjoined a house where scarlet fever had existed for several weeks. The cows were milked, every night and morning, into an open tin can carried across an open yard past the affected house.	The children who first caught scarlet fever in the locality played about the yard whilst in a state of desquamation.	On one particular day a general epidemic of scarlet fever broke out in the town, between thirty and forty families being invaded. All those suffering from the disease received their milk-supply from this particular dairy-farm. Some member of every family supplied had either a scarlatinal sore throat or scarlet fever on this day. Other families supplied from a different source escaped the disease.	A lodger had the milk raw for supper and was attacked. His landlady boiled her milk the same night and escaped.

[1] Lecture delivered by Dr. E. Klein, F.R.S., at the Royal Institution on Friday, May 27, 1887.

Now, mark this, that on one particular day the fever broke out. We will return presently to this point.

As an illustration of the second kind (viz. probably not from a human source), I will refer to the outbreak of scarlet fever in Oxford in the spring of 1882, recorded by Dr. Darbishire in the St. Bartholomew's Hospital Reports, vol. xx.

The substance of Dr. Darbishire's Report is this:—Three cows were kept by those who sold the milk, and nine houses, containing eighty-five persons in all, were supplied morning and evening; the milk was never stored, as there was generally barely enough at each milking for all the customers. In the house to which the cows and paddock belonged, there was a case of diphtheria in a young lady. She was removed to the infirmary on March 1. The cowman had a child ill with scarlet fever in his cottage from February 27 till March 3. On March 3, Dr. Darbishire had this child removed to the hospital and the cowman's cottage thoroughly disinfected; the cowman left his cottage to sleep in lodgings near, the care of the cows having been handed over to another man, engaged for that purpose. Now, if the milk had become infected from either of these two cases (one diphtheria and the other scarlet fever), this must have occurred for the first before March 1, for the other before March 3; and as the period of incubation of scarlet fever is known to be as a rule less than seven days, it follows that March 3, being the last day on which the milk could have received the contagium from a human being, March 10 would be the last day on which scarlet fever could have been produced by that milk, and the majority of cases of scarlet fever must have occurred before that day, as one cannot assume that in all these cases the period of incubation was protracted to such length as seven days. But mark what really did happen. Dr. Darbishire states that no case occurred till March 10, on which day 2 cases of sore throat and 1 case of scarlet fever occurred; on March 11, 1 case of sore throat; March 12, 2 of sore throat and 1 of scarlet fever; March 13, 4 of sore throat and 2 of scarlet fever; March 15, 1 of sore throat and 1 of scarlet fever; March 16, 2 of sore throat and 1 of diphtheria; March 17, 1 of sore throat; March 18, 1 of sore throat.

Now, all these cases were proved by Dr. Darbishire to have been caused by that milk. There occurred subsequently other cases, but these were traced to have been due to secondary infection from person to person.

This is a good illustration of a milk epidemic, in which the milk most probably did not receive the contagium by human agency. And there are other milk epidemics which on analysis of dates lead to the same conclusion. The infection of this milk was probably brought about as I shall show you hereafter in some other way.

As an instance of the third kind, viz. where milk has clearly not been infected from a human source, I will refer to Mr. Power's Report in 1882 on an epidemic outbreak of scarlet fever in St. Giles and St. Pancras. "The disease was distributed with a milk service derived from a Surrey farm. In this case two facts could be affirmed: the one that a cow recently come into milk at this farm had been suffering from some ailment, seemingly from the time of her calving, of which loss of hair in patches was the most conspicuous manifestation; the other that there existed no discoverable means by which the milk which had coincided with scarlatina in its distribution, could have received infective quality from the human subject." (Medical Officer's Report for 1885-86, pages v. and vi.)

The Medical Department of the Local Government Board, have from these facts drawn the conclusion that "distrust must be placed on the universally accepted explanation that milk receives infective properties directly by human agencies," and further that "the question of risk from specific fouling of milk by particular cows, suffering, whether recognized or not, from specific disease, was seen to be arising." This view received striking confirmation and proof by a report of an outbreak of scarlet fever that occurred at the end of 1885, and the beginning of 1886, in the North of London, which was investigated by Mr. Power; his report is published *in extenso* in the Report of the Medical Officer of the Local Government Board for 1886. I will here give you the substance of it. Mr. Wynter Blyth, Medical Officer of Health for Marylebone, "had last December observed a sudden outbreak of scarlatina in his district to be associated with the distribution of milk coming from a farm at Hendon, and had found reason for believing that the disease had prevailed exclusively amongst customers furnished with milk from that source." Mr. Power on a more extended inquiry found that a similar prevalence of scarlatina had occurred about the same time in other parishes in and near the metropolis that were furnished with milk from the same farm. By careful inquiry, Mr. Power could with certainty exclude any contamination of the milk from a human source, or that anything of the kind known as "sanitary" conditions could have had any concern with the infectivity of the milk. Mr. Power showed conclusively that only certain sections of the milk-supplies of this farm, and finally only certain cows from which these sections of milk were derived, had any relation to the observed results. "In the end," says the Medical Officer, "he has demonstrated, beyond reasonable doubt, the dependence of the milk scarlatina of December on a diseased condition of certain milch cows at the farm—a condition first introduced there in the previous month by some animals newly arrived from Derbyshire; and he finds strong circumstantial evidence for believing that the later phenomena of this dependence were brought about through the extension of the diseased condition of one set of animals to another set, after the fashion of an infection."

Now this disease, as it presented itself in some of these Hendon cows, consisted in the presence of sores in different parts of the skin with loss of hair in patches, ulcerations on the udder and teats, and a visceral disease, notably of the lungs, liver, kidney, and spleen, which, although milder in character, very much resembled the visceral lesions occurring in cases of human scarlet fever. By experiment it was shown that the matter of the ulcers of the udder is possessed of infective power, inasmuch as on inoculation into the skin of calves the same ulcers are reproduced; further, it was shown that in the ulcers of the cow there existed in large numbers a species of micrococcus, which, on being planted on artificial nutritive media, such as are used for the study of bacteria, produces in a few days a crop of micrococci, possessed of very distinct characters by which they are distinguishable from other bacteria.

When calves are inoculated from a cultivation of this micrococcus, they become, after an incubation period, affected with a cutaneous and visceral disease the same as the disease of the Hendon cows. From the blood of these animals the same microbe was recovered by cultivation.

To sum up, then, it has been shown that at this Hendon farm there existed certain cows affected with a communicable disease which, in many points of its pathology, bears a great resemblance to human scarlatina; further, that the milk of these cows gave scarlet fever to human beings; and, lastly, that a particular microbe was obtained from these cows, which in calves produced a disease similar to the one from which those cows were suffering. In order to complete the evidence thus far obtained, it was necessary to prove that scarlet fever in man is due to the presence and multiplication in the blood and tissues of the same micrococcus, and that this microbe, if obtained from human scarlet fever, produces in the cow the same disease as is produced by the micrococcus of the Hendon cows. Now, this proof has been satisfactorily given. In the first place, it has been shown that in the blood and tissues of persons affected with scarlet fever there occurs the same micrococcus as was present in the cow, both being identical in microscopical and in cultural characters. In the second place, it was found that the action of this microbe on animals is exactly the same as the micrococcus found in the Hendon cows. Calves and mice, after inoculation or feeding with a trace of the growth of both sets of micrococci, become affected with cutaneous and visceral disease similar to human scarlet fever; in calves, the disease is of the same mild type as in the Hendon cows. I have lately ascertained that milch cows inoculated with the human scarlet fever micrococcus developed readily a disease identical in every respect with the Hendon disease, inclusive of the ulcers on the teats, and the sores and loss of hair in patches in different parts of the skin. Further, it was shown that from the blood and the tissues of these animals infected with one or the other set of cultivations, the same micrococcus was recovered. I will remind you that, in all infectious diseases which have been proved definitely to be associated with a particular species of microbes, this microbe introduced into a susceptible body thrives and multiplies, and thus sets up the diseased condition, differing of course with the different species of microbes. I think I may after this say that this microbe, *Micrococcus scarlatinæ*, is the cause of human scarlet fever; further, that it produces in bovine animals a disease identical with the Hendon disease and human scarlet fever, and that consequently, while the cow is susceptible to infection with human scarlet fever, it can in its turn be the source of contagium for the human species, as was no doubt the case in that milk epidemic from the Hendon farm.

I shall now give a striking piece of evidence well in harmony with what I have mentioned hitherto. In October 1886, Prof. Corfield forwarded to me certain tins of condensed milk, sold under the name of "Rose brand." This milk was under suspicion of having produced scarlet fever in a number of persons that had partaken of it. From one out of three tins of this condensed milk, I have obtained by cultivation a microbe which in every respect, morphologically and in cultures, is the same as the microbe obtained from the Hendon cows and from human scarlet fever. The action of the microbe of the condensed milk was also tested on animals, calves and mice, and it was found that it produced the identical disease that was produced by the microbe of human scarlet fever, and of the Hendon cows. I may add that this Rose brand of condensed milk is, like all condensed milk, obtained from cows' milk. The Rose brand is a cheap article, and meant for the poorer classes; probably it has not been sufficiently heated in the tins before sealing the latter; that this is so can be inferred from the fact that every tin of this brand which I opened contained some organisms. Thus, for instance, I find that one tin contained the scarlet fever microbe and another species of micrococcus; another tin contained a harmless species of micrococcus only; and a third tin opened contained a micrococcus and a species of bacillus.[1]

Another piece of interesting evidence concerning the *Micrococcus scarlatinæ* is this: there occurred during the beginning of this year a severe epidemic of scarlet fever in Wimbledon. This epidemic was also traced to milk coming from a particular farm. In one of the houses supplied with this milk there occurred cases of scarlet fever amongst human beings, and at the same time a pet monkey, who also consumed a good deal of the milk, became ill; it died after five days. I had the opportunity to make a *post-mortem* examination of this animal, and there could be no doubt about its having died of scarlet fever. From the blood of this monkey I obtained by cultivation the same micrococcus as was obtained fro human scarlet fever, from the Hendon cows, and fro the condensed milk. Experiments made on animals wit this micrococcus of the Wimbledon monkey showed tha the same disease is produced both by inoculation and b feeding.

It having been proved, then, that the cow is susceptibl to infection with scarlet fever from man, the next impo tant question is this, How does the milk of such infecte cows assume infective power? Clearly in one of tw ways: first, either the milk becomes infected by th milker during the process of milking, particles of con tagium being rubbed off the ulcers of the udder or teat or, the milk *per se* is possessed of infective power—that i it being a secretion of a constitutionally diseased animal From previous and from more recent observations, I an inclined to think that both views hold good.

I now come to the question, How is the spread o scarlet fever by milk to be controlled and checked This question resolves itself into three parts. First prevention of infection of the cow by man, directly o indirectly; second, prevention of infection of the cow b the cow; and third, destruction of the contagium of th milk of such cows.

As regards the first, all those rules which have bee laid down to prevent infection of one human being from another, of milk or any dairy utensil by contact or other wise with a person suffering from scarlet fever or comin from an infected house, apply also here; and this part o the subject comes under the general aspect of the prope sanitary management of dairies, which is acted upon i all well-managed dairies.

As regards the second, viz. prevention of infection o the cow from the cow, this is obviously more important an more difficult to be carried out. I say obviously, becaus one cow affected with the disease is capable of com municating it to others in the same farm, and whe moved to another farm also to the cows there.

The disease in the cow being of a mild character i easily overlooked. The disease in the skin of the co may be present and slight, or may be absent in its more conspicuous manifestation, whereas the visceral disease is of so mild a character that it requires an expert t diagnose it. When a cow shows the disease of the skin and on the udder well pronounced, such an animal will have to be carefully examined for visceral disease. I need hardly say that amongst the many cutaneous disorders of the cow, known and unknown, there may be one or the other which bears a resemblance to the cutaneous disorder occurring in scarlatina; such cutaneous disease must be carefully excluded before an animal is condemned; but, if visceral disease should be diagnosed as well, the animal should be carefully isolated and its milk should not be used. And it must be clear from this that every dairy should be permanently under the supervision of an expert, and in this the veterinary profession should be as eager for the work as the medical sanitary officers are, and for some time past have been. But judging from the attitude assumed by the veterinary authorities I am afraid the veterinary profession has not yet grasped the full responsibility that rests on them, both towards the general public and the dairy farmers. Instances are on record, when, on the milk from a particular farm having been proved or even suspected to bear any relation to a scarlet fever epidemic, the business of such farm became temporarily or even permanently suspended, and the pecuniary loss of the owner of such farm irrevocable. That the disease in the cow which I have described to you as scarlet fever is as yet unknown to the veterinary profession does not do away with the existence of such disease, and I venture to say that the fact of its being as yet unknown to and unrecognized by them should stimulate them to try to recognize it.

Now the third question, as to the destruction of the

[1] It is well known that no species of micrococci hitherto known are capable of surviving a temperature of 212° F., *i.e.* of boiling water; many of them are killed by an exposure to 180°–190° F.

contagium in the milk. This, I am glad to say, is very easily carried out. Heating milk up to 85° C. or 185° F., that is, considerably under the boiling-point, is perfectly sufficient to completely destroy the vitality of the microbe of scarlet fever. In harmony with these experiments on the influence of heat on the microbe of scarlet fever, I can quote, besides the observation given above by Dr. Robertson, also the following observations recorded by Dr. Jacob, Medical Officer of Health of High Ashurst and Headley, and reported in 1878, to this effect. Between June 1 and 7, there were fifteen cases of scarlet fever in three distant houses, the inmates of which had had no communication with infected persons, but had all been supplied with milk from a farm where a certain cowman worked. This cowman had in his family several children ill with scarlet fever. The cowman continued milking the cows during the illness of his children, though he did not himself have the fever, and the milk was not taken into his cottage; but the point which I wish to bring out is this, that other houses besides those in which scarlet fever had broken out had been supplied with the same milk, but no scarlet fever occurred in them, and why? because all these had consumed only the scalded milk.

I should therefore strongly urge that all milk should be boiled, or at any rate heated to at least 85° C. (that is 185° F.) before being consumed. Judging by the large number of cases of scarlet fever recorded in these milk epidemics, one is justified in saying that a considerable percentage of the total number of cases of scarlet fever would have been avoided thereby. Not all, because unfortunately the rules of isolation of patients suffering from scarlet fever are not always rigorously carried out, and therefore infection from person to person will occur. Nor would prevention of scarlatina by milk exclude scarlatina by cream,—cream cannot be easily subjected to heat; and in the epidemic of scarlet fever that occurred in South Kensington in 1875, and that was investigated by Dr. Buchanan, cream was the vehicle of the contagium. But considering the prominent position that milk occupies in every household with children, the possibility of infection with scarlet fever by raw milk deserves careful attention.

THE SECOHMMETER.

A CIRCUIT containing self-induction acts as if it had a larger resistance than its true one when a current is started in it, and a smaller resistance when the current is stopped. Hence, if balance be obtained with a Wheatstone's bridge in the ordinary way, the fact of any of the arms possessing self-induction, or of any one of the arms having a condenser attached to it, will produce no effect on the balance if the battery circuit be rapidly made and broken, provided that the rapidity of make and break be not too great for the currents in the arms of the bridge to reach their steady values each time that the battery circuit is made, and to die away each time that it is broken. If the currents have not time to reach their steady value when the battery circuit is closed, and to die away when it is broken, then self-induction in any one of the arms will produce a disturbance in the balance; but such a method of measuring a coefficient of self-induction would lead to very complicated formulæ, and is not worth developing with the view of obtaining a simple method of measuring self-induction.

It therefore occurred to us to consider whether, without employing such rapid makes and breaks as would prevent the currents reaching their steady values, the self-induction of a circuit might not be made to act as an apparent steady definite increase of the resistance of that circuit which could be measured in the ordinary way with a Wheatstone's bridge or differential galvanometer; and by this means the measurement of a coefficient of self-induction would simply resolve itself into the measurement of a resistance. And this problem we solved in the following way, in the spring of 1886:—

The coil, the coefficient of self-induction of which it is desired to measure, is placed in one of the arms of a Wheatstone's bridge, the three other arms consisting of ordinary doubly-wound resistance coils possessing no appreciable self-induction, and not only is the battery circuit rapidly made and broken, but, in addition, after each closing of the battery circuit the galvanometer circuit of the bridge is either short-circuited or broken, so as to cut out the galvanometer, and after each breaking of the battery circuit the galvanometer circuit is either unshort-circuited or closed again, so that the galvanometer is now operative again. In this way all the successive impulses of the galvanometer needle that are produced on starting the current in the coil with self-induction produce their *cumulative* effects, but the successive impulses of the needle that, under ordinary circumstances, would be produced on the needle in the opposite direction are cut out. Hence the self-induction possessed by one of the arms causes that arm to apparently increase in resistance by a definite amount depending on the coefficient of self-

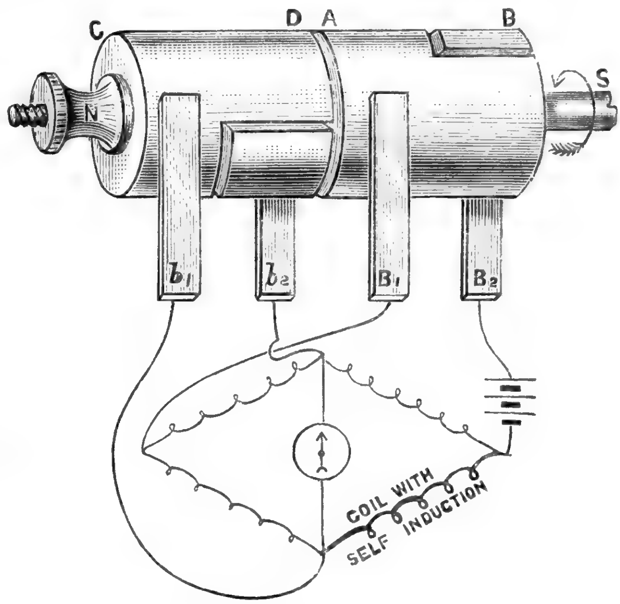


FIG. 1.—Preliminary Apparatus.

induction and the number of operations performed per minute. This apparent increase of resistance produces a deflection of the galvanometer which can be noted, and its value ascertained by comparing it with the deflection produced with steady currents when one or more of the arms of the bridge is altered by a known amount, as in making the Rayleigh test. But since the necessity of having to read the deflection limits the speed of performing the double make and break operation, in order that the spot of light may not be sent off the scale, we soon replaced this comparative deflection *cumulative* method by a much more sensitive *zero cumulative* method; and instead of reading the galvanometer deflection we re-establish the balance, and bring the needle back to zero, by altering one or more of the arms of the bridge, as in making an ordinary resistance test with a Wheatstone's bridge.

The first apparatus for enabling measurements of self-induction to be made in this way was constructed in the spring of 1886, under the superintendence of one of our assistants, Mr. Mather. It consisted of a double commutator, shown in Fig. 1, the spindle, S, to which the commutators were locked by the nut, N, being rotated at any speed by a small electromotor, not shown in the figure, to which was attached a Young's speed indicator, which registered the speed of rotation at any moment. The brushes, B_1, B_2, b_1, b_2, were fixed to the baseboard and joined to the bridge, as indicated in the figure. When the double commutator was rotated by the motor (of which the speed was correctly adjusted by means of a Varley's flexible carbon-resistance), the portion A B caused the battery circuits to be periodically made and broken, while the other portion, C D, periodically short-circuited and unshort-circuited the galvanometer, so that the following cycle of operation, called for simplicity *one operation*, was performed any desired number of times per minute :—

Battery circuit.	Galvanometer short circuit.
Make.	While broken.
While made.	Make.
Break.	While made.
While broken.	Break.
Make.	While broken.

If we call

$$\frac{\text{angle between the slits in the two commutators}}{360^\circ}$$

the lead or l, so that l will be equal, for example, to $\frac{1}{4}$ when each of the cycle of operations given in the table lasts for one-quarter of a revolution, then we have shown that

$$L = \frac{l}{n}\sigma \text{ second-ohms,}$$

where n is the number of revolutions of the commutator per second, and σ the apparent increase of resistance of the coil with self-induction, or

$$L = \frac{60l\sigma}{N} \text{ second-ohms}$$

where N is the number of revolutions per minute.

A number of experiments were made in the summer and autumn of last year, and they showed that this new method was very accurate and furnished an extremely sensitive test for the absolute measurement of a small coefficient of self-induction.

By the simple addition, therefore, of such a commutating arrangement as we have described to an ordinary Wheatstone's bridge, it becomes possible, whenever the resistance of a coil electro-maget, &c., is being measured, to measure also the coefficient of self-induction in absolute measure, by a zero method which is as sensitive for the measurement of self-induction as the ordinary Wheatstone's bridge method is for the measurement of resistance.

The instrument previously described requires an electromotor to drive it, and a speed-indicator to register its speed, hence it would be too cumbersome for every-day work. It therefore became necessary to devise commercial apparatus, and this was done as follows :—

Attached to the commutator of our self-induction apparatus is a box, B (Fig. 2), fitted with weighted elastic sides made of corrugated steel, which fly out more and more, under the action of centrifugal force, as the box is rotated faster and faster. A stout glass tube, G G′, of comparatively small bore, open at both ends, is cemented into a collar in the axis of the box, and rotates with the box. The box is completely filled with mercury, and the tube partially, hence when the volume of the box expands as its sides fly out the length of the column of mercury in the tube diminishes, and the length of the column at any moment is a measure of the speed of rotation of the box. In the neck of the collar, C, in which the tube is cemented, there is a steel tap attached to an axial spindle passing through a tube inside the box, and projected out of this tube at the other end of the box. If this spindle be turned relatively to the box, the tap is opened or closed. At the commencement of the experiment the tap is opened, and the handle, H, is turned with the right hand, faster and faster, until, on depressing the key, K, with the left hand from time to time, the galvanometer needle is seen to be approaching zero, or the spot of light the zero position on the scale. The key may now be kept depressed, and on turning the handle a little faster a speed is at length reached producing exact balance—if the handle be turned faster, the needle or spot of light deflects to one side of the zero, if more slowly to the other—at this moment the trigger, T, is lightly touched with the left hand, and a spring is liberated. This has the effect of producing a resistance to the rotation of the tap-spindle, which previously was rotating freely with the rotating box, and the tap is thus turned off, cutting off the connexion between the mercury in the glass tube and that in the box. Consequently the mercury in the tube remains, even after the instrument is stopped, of exactly the same length that it had when the trigger was touched. The position of the end of thread of mercury in the tube is now read off on the scale attached, and the apparent increase of resistance of the coil, electro-magnet, or whatever it may be, divided by the number on the scale, gives the required coefficient of self-induction in second-ohms without any further calculation.

The instrument is, therefore, direct-reading.

At first, rotating commutators similar to those shown in Fig. 1 were employed with the apparatus shown in Fig. 2; next the brushes were made of a variety of different forms, so as to press *radially* on the rotating commutators to prevent the wear altering the lead, and thus changing the sensibility of the instrument; but this form of commutator has at length been entirely superseded by the two oscillating arms, or brushes, A, A, worked by a cam. Each arm is composed of several pieces of hard copper, contact being made through the ends, as in many of the switches now used for electric-light work. The end of each brush alternately rubs on a flat piece of phosphor bronze, P, P, when it makes contact, and on a *flat* piece of glass or agate, g, when it does not. This form of commutator we found superior for our purpose to the double cylindrical one, since, with the two oscillating arms, the lead can be more easily varied for adjustment; and this slight adjustment of the lead, we may here mention, forms the fine adjustment in the construction of this direct-reading instrument. Further, the slow wearing of this form of brush does not alter the lead; consequently the value of the graduations of the scale remains constant.

The temperature adjustment of the instrument is effected by moving the scale until the zero is opposite the end of the thread of mercury when the instrument is at rest.

Following the precedent of naming an instrument after the name of the unit employed—for example, "ammeter," "voltmeter," "ohmmeter," "wattmeter,"—it seems desirable to call this instrument after the name of the commercial unit of self and mutual induction. The absolute electro-magnetic unit of self and mutual induction is 1 centimetre, a name used by all scientific nations. But the commercial unit of self and mutual induction is $99{,}777 \times 10^4$ centimetres, or the second-ohm, which is about 2·3 in a thousand less than 10^9 centimetres, or one earth's quadrant. Now, in spite of the difference between these two numbers, which, although small, it is a pity to lose sight of, the English word "quadrant" is not used in French, therefore it would not be well to suggest this word as the international name for the unit. Yet it is most important that some name should be universally adopted, since the use of simple familiar names has much to do with making people familiar with the laws of the effect measured by the unit. The unit of electro-static capacity, the farad, has been called after the greatest experimental worker in electricity; it would therefore seem appropriate that the unit of electro-magnetic capacity should be called after Maxwell, the greatest mathematical worker in electricity. We do not, however, like to propose this, as we feel there might be difficulty in obtaining the general acceptance of the name of an Englishman, however great, unless it were sanctioned by an International Electrical Congress, or unless the man's name was intimately associated in men's minds with self and mutual induction. And Maxwell's large contribution to the subject of electro-magnetic induction is surrounded by his equally large contributions to all other branches of

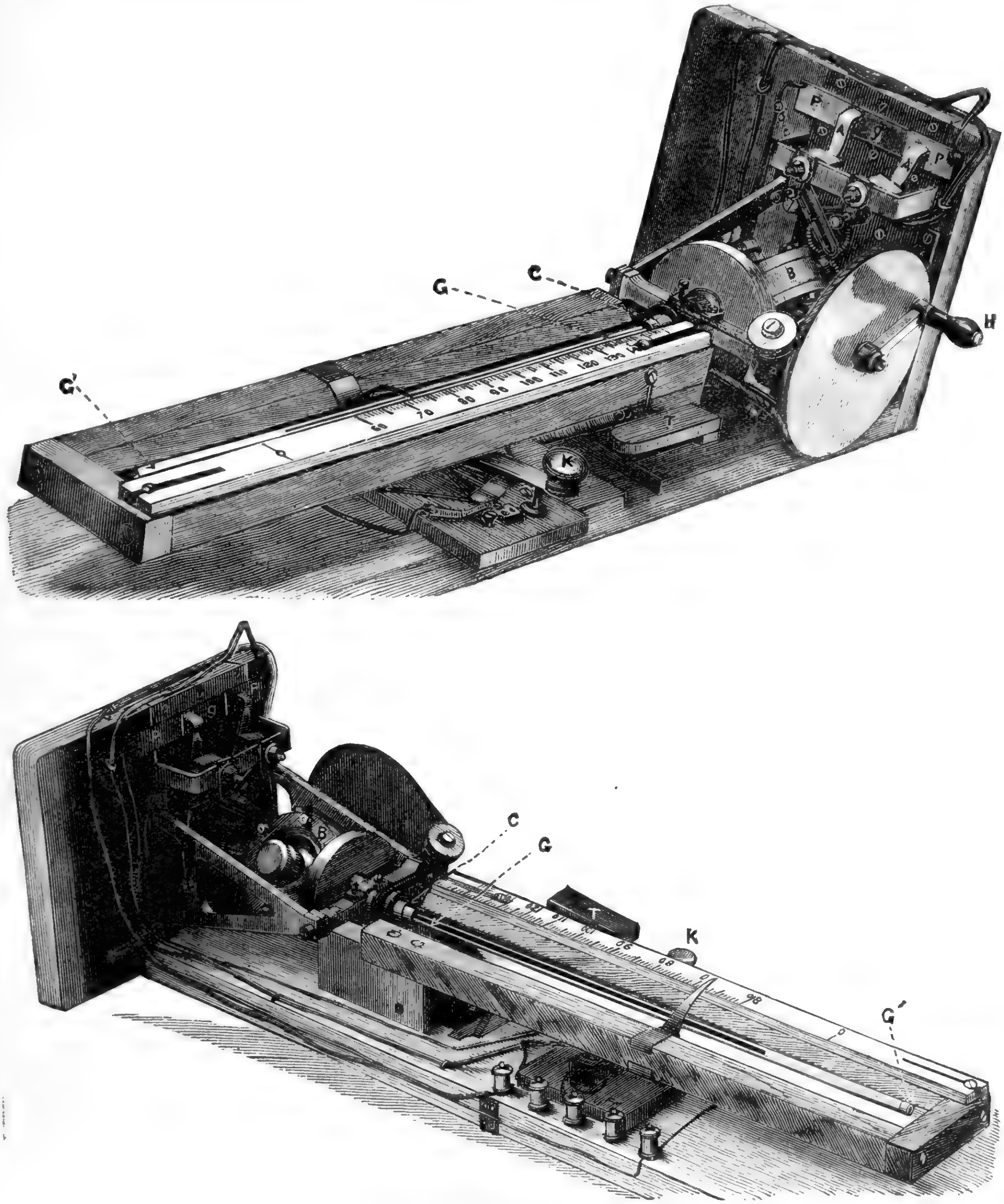


Fig. 2.—Experimental Secohmmeter.

electricity and magnetism—a giant surrounded by giants is not prominent. Coming to the last two years, we are glad that the leader and all those who have followed him in taking part in the widening of our ideas on self-induction are still with us. Hence we are driven to suggesting a temporary name for the unit, and as the first three letters in "second" are common to the name in English, French, German, Italian, &c., and ohm is also common, we venture to suggest "secohm" as a provisional name, and our instrument we will therefore call a "secohmmeter."

Unless the glass tube in the secohmmeter just described be rather long, either the sensibility or the range of the instrument must be limited; but a very long straight tube would make the instrument inconveniently large, and a rapidly rotating *spiral* tube would probably break, from centrifugal force acting on those parts of the tube that were not on the axis of rotation. Hence, in the latest form of secohmmeter, Fig. 3, we have been led to employ a *stationary* spiral glass tube, G, with its end cemented into a stationary hollow steel conical plug fitting mercury-tight in the collar of the rotating metal box, B, with its weighted elastic sides. This arrangement simplifies the tap mechanism, as the tap now is not rotating, also many small improvements have been introduced into this last form: for example, at all the joints there is mercury under pressure, so that there is no tendency for air to be drawn into the apparatus at the joints, a fault which sometimes occurred with the earlier form of the apparatus, and led to irregularities in the readings from a bubble of air in the box acting on an air spring, or from air in the glass tube altering the length of the thread of mercury. The temperature adjustment in this last form of the secohmmeter is made by screwing a screw in or out, which slightly alters the volume of the stationary portion of the mercury vessel. The fly-wheel F has been made to have

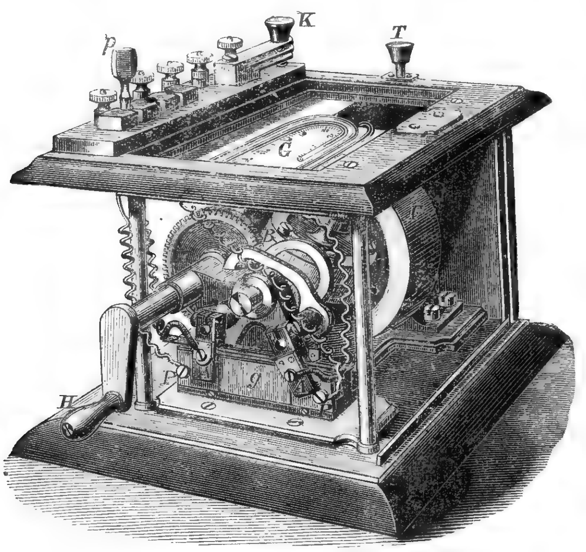


FIG. 3.—Improved Secohmmeter.

a much larger moment of inertia, and the box B is placed inside it, so as to be screened from damage.

Mutual Induction.

If we wish not merely to determine the coefficient of self-induction, L, of a coil, S, but also the coefficient of mutual induction, M, between it and any other coil, we first exclude the other coil from the battery circuit and determine L in the manner already described. We next include the other coil in the battery circuit, and repeat the experiment with the secohmmeter; then, as shown by one of our students, Mr. Sumpner (to whom our thanks are due for the most able assistance that he has rendered in this investigation)—

$$M = \frac{p}{p+r}\left(\frac{60\,l}{N}\,\sigma - L\right),$$

or if N_1 and σ_1 are the speeds and apparent increase of resistance in a first experiment, and N_2 and σ_2 in a second, we have

$$L = 60\,l\,\frac{\sigma_1}{N_1}$$

$$M = \frac{p}{p+r}\,60\,l\left(\frac{\sigma_2}{N_2} - \frac{\sigma_1}{N_1}\right),$$

where p is the resistance of the arm of the bridge opposite the coil S, and r the resistance of the arm joining p and S.

Capacity.

We have also shown that, if, instead of placing a coil with self-induction in one of the arms of the bridge, the arm be shunted with a condenser, there will be an apparent diminution of the resistance of that arm, since such a shunted condenser acts as if it had a negative self-induction. This apparent diminution divided by the product of the square of the actual resistance of the arm into the reading of the scale of the instrument corresponding with the speed at which balance is obtained gives the capacity absolutely in farads. The formula is, therefore, far simpler than that given by Clerk Maxwell for the absolute measurement of the capacity of a condenser, by placing it on one of the arms of the bridge and rapidly reversing the connexions of the condenser with the bridge.

Secohmmeter without Speed Indicator.

Lastly, all known zero galvanometric methods of comparing the coefficients of self or mutual induction with one another, or with the capacity of a condenser, can be increased enormously in sensibility by the use of the secohmmeter, and, since in such cases the speed of rotation need not be known, a very simple form of secohmmeter without speed indicator can be employed. The comparison of the coefficients of self or mutual induction with one another, or with the capacity of a condenser, is usually effected by tests that are completed during the growth or the dying away of a current, since it is only during the variation of a current that self or mutual induction, or the electro-static capacity of a condenser, evidence themselves. The effect of an error in the balance only lasts for a very short time, and therefore is very small if the error be small, that is, the tests are not sensitive. But by the use of the secohmmeter it is now possible not merely to measure the coefficients of self and mutual induction and the capacities of condensers absolutely, but, in addition, to secure the same high degree of sensibility with comparison tests that have hitherto had to be completed during the growth or dying away of a current that it is customary to obtain in the use of the Wheatstone bridge for measuring resistances with steady currents.

W. E. AYRTON.
JOHN PERRY.

THE FOSSIL FISHES OF MOUNT LEBANON.

THE last published part of the Transactions of the Royal Dublin Society (May 1887) contains a memoir on the fossil fishes of the chalk of Mount Lebanon, in Syria, by James W. Davis, which is an important contribution to a very interesting subject.

The existence of fossil fishes in the chalk of Mount Lebanon has been known from remote antiquity; Herodotus refers to them, and various statements about them are recorded in writings scattered over the period between the fourteenth and eighteenth centuries. In our own century, Louis Agassiz, Pictet, Haeckel, Costa, Botta, Fraas, and others, have added greatly to our knowledge of the various species met with, and now this memoir of Mr. Davis, illustrated as it is by twenty-four excellent plates, several of which are folding plates, brings up our knowledge of these remains to the most recent date.

For the chief material on which this memoir is based the author is indebted to the zeal and energy of the Rev. Prof. Lewis, who, during his residence in the American College at Beyrout, availed himself of every opportunity of collecting specimens of these fossils, and succeeded in accumulating a very large series of new forms. Many of these have been acquired for the British Museum Natural

History Department; the rest are in the possession of Mr. Robert Damon, of Weymouth. In addition to Prof. Lewis's collection, Mr. Davis has availed himself of the material already existing in the British Museum, chiefly from the fine collections of the late Sir Philip Egerton and the Earl of Enniskillen, the latter of whom, we notice, communicated this memoir to the Royal Dublin Society, of which he was a very old member.

The two principal localities in which fish remains are found in the Lebanon are at Hakel and at Sahel Alma. In order to reach Hakel, it is necessary to go to Djebail, the ancient Byblos, a small village situated on the coast, about seventeen miles north of Beyrout. Hakel is about six miles and a quarter from Djebail, in a north-easterly direction. M. Botta describes the locality as being in a deep valley, situated at a great height above the sea-level. The beds containing the fish remains are upon the slope of the hill on the right, in ascending towards the village of Hakel. The beds are considerably displaced, and vary much in their direction and inclination; the sides of the mountain are covered with debris, and it is in this debris that the fishes are found. The debris is in the form of thin foliated slabs, exhaling when struck a strong odour of sulphureted hydrogen; these contain irregular beds of flint, or siliceous limestone, which inclose the fossils. The Sahel Alma locality is situated below the convent of this name, which is about eleven miles from Beyrout. The convent is built on ground sloping rapidly towards the sea, the surface soil is covered with mulberry-trees, and beneath this is the marly chalk containing the fish remains. It is an argilo-calcareous stone, sometimes laminated, soft, and without appreciable odour. There are parts of a deep gray colour, almost resembling a plastic clay. The fish impressions occur in considerable numbers, both of species and individuals, mixed with some species of Crustacea. The species of fish found in the two localities are very seldom the same. The opinions of authors vary as to the geological age of these fish beds. Agassiz hesitated as to whether they should be considered as pertaining to the Jurassic epoch or to that of the chalk; whilst Haeckel was doubtful whether to place them between the chalk and the Tertiary formations. Pictet considered that the large number of extinct forms, and the great differences between the fauna of the fish beds and that existing in the sea at the present time, made it impossible to attribute the remains to a Tertiary period; on the other hand, the entire absence of ganoid fishes appeared to indicate that they are of a period anterior to the Jurassic, and that they must consequently have belonged to that of the Cretaceous period. Dr. Oscar Fraas places the beds as the upper ones of the Turonian group, corresponding to the chalk marl, and below the white chalk and the Maestricht beds.

No less than sixty-three new species are described by Mr. Davis, and a number of species of other authors are re-described. Extremely beautiful drawings of most of these, from the original specimens, by Miss E. C. Woodward, accompany the memoir, which will be received with interest by all palæontologists.

The printing and paper of this volume well deserve our praise, and are fully up to the style of the recent memoirs published by the Royal Dublin Society.

COMPLIMENTARY DINNER TO PROFESSOR TYNDALL.

WE are glad to be able to announce that a complimentary dinner is to be given to Prof. Tyndall on the occasion of his retirement from the Chair of Physics in the Royal Institution. Prof. Tyndall has still before him, we hope, many a long year of fruitful research, but it would have been strange if the present opportunity had been allowed to pass without an adequate expression of the gratitude which is felt by large classes of his countrymen for the services he has already rendered to science. His great reputation he has won by severe and long-continued labour, the value of which is most highly estimated by those who are most capable of forming a judgment on its worth. Prof. Tyndall has not only made additions to the sum of human knowledge; he has done much to aid the process by which the English public are acquiring a new conception of the place that properly belongs to science in modern life, and of the need for applying scientific method to departments of thought and work from which it has hitherto been too often rigidly excluded. Moreover, by his popular expositions of the results of inquiry in various branches of physics, he has shown that science, so far from being in any sense hostile to literature, can receive full justice only when it is handled by writers who are masters of literary expression. The books in which Prof. Tyndall has appealed to the general public have marked an era in the intellectual development of many of his readers, and his works will always serve to remind men of science of the possibility of presenting profound and accurate thought in luminous and attractive forms.

We print the letter which the Honorary Secretaries are now sending to the members of scientific Societies and to various representative men.

Science Schools, South Kensington,
June 6, 1887.

DEAR SIR,—The retirement of Prof. Tyndall from the Chair of Natural Philosophy in the Royal Institution affords a fitting occasion for a formal recognition of the great services which he has rendered to the cause of scientific progress.

Prof. Tyndall has therefore been invited to a complimentary dinner which will take place at Willis's Rooms on Wednesday, June 29, at 7 o'clock.

The chair will be taken by the President of the Royal Society, who, it is hoped, will be supported by a large and representative body both of scientific men and of others who appreciate the importance to the nation of scientific instruction and of the promotion of natural knowledge.

The Committee hope that you will be able to attend, and in this case we shall be glad if you will kindly fill up the accompanying form and return it to us at your earliest convenience.

Tickets will be 30*s*. each, and the Committee request us to state that it will be necessary to hold gentlemen who receive tickets responsible for that sum, even if they should unfortunately be prevented from attending the dinner.

The early return of the accompanying form is desirable, as it will be impossible to find room for more than 280 persons. Should more than that number apply, the Committee will, as far as possible, distribute the tickets in the order of application. In any case, a further communication will be addressed to you.

We are, dear Sir,

Faithfully yours,

J. NORMAN LOCKYER } *Hon. Secs.*
ARTHUR W. RÜCKER }

The following is a list of those who have up to the present consented to serve on the Committee:—

Chairman, Prof. G. G. STOKES, President of the Royal Society.

The MARQUIS OF SALISBURY, K.G., F.R.S., Chancellor of the University of Oxford.

The DUKE OF DEVONSHIRE, K.G., F.R.S., Chancellor of the University of Cambridge, and of the Victoria University.

The DUKE OF ARGYLL, K.G., F.R.S., Chancellor of the University of St. Andrews.

The Right Hon. JOHN INGLIS, D.C.L., LL.D., Chancellor of the University of Edinburgh.

The EARL OF ROSSE, F.R.S., Chancellor of the University of Dublin.

The EARL GRANVILLE, K.G., F.R.S., Chancellor of the University of London.

Sir F. ABEL, C.B., F.R.S., ex-President of the Chemical Society.

Prof. J. C. ADAMS, F.R.S., ex-President of the Astronomical Society.
Prof. W. G. ADAMS, F.R.S., ex-President of the Physical Society.
Sir GEORGE B. AIRY, K.C.B., F.R.S., ex-Astronomer-Royal, and ex-President of the Royal Society.
Sir W. BOWMAN, Bart., F.R.S., formerly Secretary to the Royal Institution.
Sir F. BRAMWELL, F.R.S., Secretary to the Royal Institution, and ex-President of the Institution of Civil Engineers.
Prof. CAYLEY, F.R.S., ex-President of the British Association.
Prof. CLIFTON, F.R.S., ex-President of the Physical Society.
W. CROOKES, Esq., F.R.S., President of the Chemical Society.
W. H. M. CHRISTIE, Esq., F.R.S., Astronomer-Royal.
WARREN DE LA RUE, Esq., F.R.S., ex-President of the Royal Astronomical and Chemical Societies.
Prof. DEWAR, F.R.S., Professor of Chemistry in the Royal Institution.
Colonel DONNELLY, C.B., Secretary to the Science and Art Department.
Prof. P. M. DUNCAN, F.R.S., ex-President of the Geological Society.
W. T. THISELTON DYER, Esq., F.R.S., Director of the Royal Gardens, Kew.
Dr. EVANS, Treasurer of the Royal Society, and President of the Society of Antiquaries.
Prof. FLOWER, F.R.S., Director of the Natural History Department, British Museum.
Prof. G. CAREY FOSTER, F.R.S., ex-President of the Physical Society.
Prof. M. FOSTER, Secretary of the Royal Society.
F. GALTON, Esq., F.R.S., President of the Anthropological Society.
Prof. GAMGEE, F.R.S., Professor of Physiology in the Royal Institution.
A. GEIKIE, Esq., F.R.S., Director-General of the Geological Survey.
Sir W. GROVE, F.R.S., ex-President of the British Association.
Dr. HIRST, F.R.S., ex-President of the Mathematical Society.
Sir J. HOOKER, F.R.S., ex-President of the Royal Society.
Prof. HUXLEY, F.R.S., ex-President of the Royal Society.
Prof. JUDD, F.R.S., President of the Geological Society.
Sir JOHN LUBBOCK, F.R.S., ex-President of the British Association.
HUGO MÜLLER, Esq., F.R.S., ex-President of the Chemical Society.
Prof. ODLING, F.R.S., ex-President of the Chemical Society.
Sir LYON PLAYFAIR, K.C.B., F.R.S., ex-President of the British Association.
Lord RAYLEIGH, Secretary of the Royal Society.
Admiral Sir G. H. RICHARDS, K.C.B., F.R.S., ex-Hydrographer to the Navy.
Sir H. E. ROSCOE, F.R.S., ex-President of the Chemical Society, and President-Elect of the British Association.
Prof. BALFOUR STEWART, F.R.S., President of the Physical Society.
General R. STRACHEY, F.R.S., President of the Royal Geographical Society.
Sir W. THOMSON, F.R.S., President of the Royal Society of Edinburgh.
Captain WHARTON, R.N., F.R.S., Hydrographer to the Navy.
Professor A. W. WILLIAMSON, Foreign Secretary of the Royal Society.

M. BOUSSINGAULT.

STUDENTS of agricultural chemistry have received with much regret the tidings of the death of M. Boussingault, one of the earliest and most eminent investigators in this branch of science. He was born at Paris on February 2, 1802, and obtained his scientific education at the School of Mines of St. Étienne. When little more than twenty years of age, he went as a mining engineer to Columbia, South America, where he remained ten years. During his residence in South America he made the acquaintance of Alexander von Humboldt, who warmly praised his work in chemistry, meteorology, geography, and astronomy. On his return to France, M. Boussingault was appointed Professor of Chemistry at Lyons. He married the sister of M. Lebel, who had been his fellow student at St. Étienne, and by his marriage he became, with his brother-in-law, joint proprietor of the estate of Bechelbronn, in Alsace. Here he set up the first laboratory that had ever been established on a farm, and carried on a long series of important researches.

From the time of his marriage, Boussingault generally spent about half the year in Paris, and the other half in Alsace. In 1836, he published a paper on the quantity of nitrogen in different foods, and on the equivalents of the foods, founded on the amounts of nitrogen they contained. This was his first important contribution to agricultural chemistry. It was soon followed by others, which secured for him, in 1839, the honour of being elected a member of the Institute. Among his publications in 1837 and 1838, were papers on the amount of gluten in different kinds of wheat, on the influence of the clearing of forests on the diminution of the flow of rivers, on the meteorological influences affecting the culture of the vine, and on the principles underlying the value of a rotation of crops. In connexion with this last subject he brought out many new facts, which seem to have been of essential service to Liebig. In 1843, much attention was attracted by a work entitled "Économie Rurale," in which M. Boussingault embodied the results of many of his original investigations. A translation, under the title of "Rural Economy in its Relations with Chemistry, Physics, and Meteorology," was published in this country, and made the author's name widely known among English agriculturists. In a review of this translation in 1845 the *Agricultural Gazette* described the work as "the most important and valuable book for farmers which the chemists of the present century had produced—not so attractive as the clever paragraphs of Prof. Liebig, but much more than compensating for want of brilliancy by solid worth."

In an excellent biographical sketch of Boussingault, printed in the *Agricultural Gazette*, January 6, 1879, it is pointed out that, although his attention was by no means limited to subjects bearing on agriculture, by far the greater number of his researches had relation to the problems it suggests. "Thus," says the writer, "the amount and condition of the combined nitrogen in the atmosphere, in the aqueous depositions from it, in rivers and springs, and in the soil, have been investigated. The amounts of nitrogen, phosphoric acid, &c., in different manuring substances have been determined, and their comparative values estimated accordingly. The question of whether or not plants assimilate the free nitrogen of the air has again and again been taken up, the weight of the evidence always serving to confirm the conclusion that they do not. Very recently, too, he has made experiments in regard to some functions of the leaves of plants. Lastly, in the sphere of animal chemistry, he has from time to time devoted himself to the elucidation of important points, such as the sources in the food of the fat of the fattening animal, the assimilation of mineral constituents, the question whether any of the nitrogen of the food or of the animal is exhaled, and so on." Most of the results of his investigations relating to agricultural chemistry are given in his work "Agronomie, Chimie agricole, et Physiologie," published in seven volumes, the first of which appeared in 1860, the last in 1884.

M. Boussingault received many honours from foreign Governments and from scientific Societies both at home and abroad. In 1878, the Council of the Royal Society awarded the Copley Medal for his numerous and varied contributions to science, especially for those connected with agriculture.

In 1848, Boussingault was elected a member of the National Assembly, where he sat as a Moderate Republican, and for a short time he was a member of the Conseil d'État. In 1851 he was dismissed, on account of

his political opinions, from his position of Professor at the Conservatoire des Arts et Métiers; but this step caused so much discontent among scientific men, and was so vigorously resented by his colleagues, who threatened to resign in a body, that the Government had to reinstate him.

He died on May 11, 1887, in his eighty-sixth year.

NOTES.

THE "Ladies' *Soirée*" at the Royal Society held last night was carefully prepared and largely attended. We shall refer at length next week to some of the objects exhibited.

THOSE who have made the arrangements for the great national ceremony at Westminster Abbey in connexion with the Queen's Jubilee cannot, it would appear, be congratulated on the manner in which they have discharged one of the most important of their functions. On so striking an occasion all the highest interests of the nation ought to be adequately represented; yet some of the most vital of these interests have been practically ignored. "A Student" has alluded to the matter in a letter to the *Times*, and his remarks seem to be worthy of attention. Having referred to the eminent fitness of Westminster Abbey for a ceremony of this kind, he says:—"I imagined gathered together there all the men who by their deeds, their discoveries, their inventions, their writings, or their noble lives and ideas, have helped during the Queen's reign to make England what she is at the present moment, and I imagined, too, that the list of the names of those present might be a roll fit to be handed down to the remotest posterity as an authoritative statement of England's most illustrious citizens in the present year. Proud that the English Government had resolved to act upon such a noble idea, I have been endeavouring to express my enthusiasm and gratitude to many that I have met, with the result that I have found either that my view of the Government's intention was perfectly wrong or that it is being carried out in such a manner that the thing promises to be an expensive and unworthy farce. I have been informed by some who should know *that among those who have already been invited hardly the name of any representative of literature, science, or art has been included.*"

THE Queen has intimated her intention of accepting the Albert Medal, which has been awarded to her by the Council of the Society of Arts. The Albert Medal is annually given for "distinguished merit in promoting arts, manufactures, or commerce."

ALL who remember the important aid rendered by Governor Sendall to the Government Eclipse Expedition to the West Indies last year will be glad to see by a recent *Gazette* that a C.M.G. has been conferred upon him. The same *Gazette* also included the name of Dr. Hector for the step of K.C.M.G. This is also another unexceptionable appointment. We are glad that the authorities at the Colonial Office are making such wise selections; the order of St. Michael and St. George bids fair to eclipse that of the Bath, the civilian distinctions connected with which seem more and more rarely to come in a scientific direction, and to be more and more limited to the spending rather than the thinking departments.

SOME important appointments have just been made at University College, London. Dr. William Ramsay, Principal of, and Professor of Chemistry in, University College, Bristol, has been appointed to fill the Chair of Chemistry, vacant by the resignation of Dr. Williamson; Dr. Sydney Ringer, F.R.S., has been made Holme Professor of Clinical Medicine, in succession to the late Dr. Wilson Fox; and Mr. Victor Horsley, F.R.S., succeeds Dr. Bastian (resigned) as Professor of Pathology.

WE referred lately (p. 87) to a Bill introduced into the House of Commons by Sir Henry Roscoe, empowering any School Board, local authority, or managers of a public elementary school, to provide day technical and commercial schools and classes. Mr. James Stuart has introduced a corresponding measure for the establishment of evening schools and classes which shall give instruction in continuation of that obtained in public elementary schools. The subjects to be taught include the elements of such portions of science as may be likely to be useful to artisans and other persons engaged in industrial and agricultural occupations; also elementary mechanics, mechanical drawing, the elements of art and design, the use of ordinary tools, commercial arithmetic, and commercial geography. For providing these evening continuation schools the powers of School Boards or other local authorities are to be in all respects the same as for providing ordinary public elementary schools. Further, there is to be the power of providing or contributing to the maintenance of laboratories or workshops in endowed schools for the purpose of carrying on classes under the Bill. The schools and classes thus provided are to be subject to the inspection of the officers of the Committee of Council on Education or of the Science and Art Department, and no scholar is to be admitted to a school or class who has not passed an examination in the sixth standard. It is also proposed that School Boards or other local authorities shall have power to provide evening schools and classes, either in connexion with "evening continuation schools" or not, for the purpose of giving instruction in a particular group of subjects, among which are arithmetic, geography, elementary science, drawing, wood-carving, and modelling. The conditions as to these schools and classes do not differ from those as to the continuation schools, except that the standard to be passed previously to admission is the fourth, not the sixth. For any of the subjects taught in evening schools or classes under the Bill the Committee of Council on Education are empowered to give grants on such conditions as they may lay down.

WE learn that the Bentham Trustees have purchased for presentation to the Library of the Royal Gardens, Kew, the unique collection of portraits of *Bromeliaceæ* which were accumulated during a life-long study of the order by the late Dr. Morren, Professor of Botany in the University of Liége. Some of the drawings, which are in all cases of life size, were exhibited at the recent reception of the Royal Society.

IN an article printed in NATURE on January 13 (vol. xxxv. p. 248), Mr. D. Morris dealt with the important question of botanical federation in the West Indies. He again discusses this subject in the sixth Bulletin of Miscellaneous Information, just issued from the Royal Gardens, Kew. For the last hundred years the cultivation of the sugar-cane has been the only important industry in the West Indies, and the fall in the price of cane sugar has seriously affected the general condition of the population. It is estimated that one-half of the surface of these islands, with the exception of Antigua and Barbados, is better fitted for other cultivation than that of the sugar-cane. Fresh industries might therefore be safely started, and Mr. Morris is careful to point out that "by too close an adhesion to purely sugar-growing habits and methods the people act injuriously to their best interests and neglect the numerous resources at their command." It is, however, absolutely necessary that any new enterprises which may be undertaken shall be carried on by persons equipped with adequate knowledge; and no real progress can be made unless the people of the various islands provide themselves with small but good botanical establishments in connexion with the Botanical Department in Jamaica. Something has already been done in this direction. At Grenada a

Botanic Garden is in course of being established under the charge of Mr. W. R. Elliott; and, with the sanction of the Legislature, £100 has been granted for the formation of a botanical station at Dodd's Reformatory, Barbados. A botanical station, for which £300 has been voted, is being made near Castries, St. Lucia. Mr. Morris is of opinion that the prospects of the scheme for botanical federation in the West Indies are, upon the whole, very good. The recent appointment of Mr. William Fawcett to the post of Director of the Botanical Department at Jamaica appears, he thinks, to offer every hope of success to the scheme. "It is also anticipated," Mr. Morris says, "that, while granting valuable aid to the smaller islands, Jamaica, as a centre, will herself derive, both directly and indirectly, considerable benefit from such vigorous and systematic working as would naturally arise in her own area as well as from a larger interchange of plants and seeds with the neighbouring islands."

IN connexion with the sixtieth meeting of German Naturalists and Physicians, which is to be held at Wiesbaden from September 15 to 24, there will be an important scientific exhibition. It is intended that the exhibition shall include the latest and best instruments and apparatus used in the study and in the teaching of science and medicine. The following are among the groups to be represented:—Surgery, physical diagnosis and therapeutics; ophthalmology, gynæcology; laryngology, rhinology, and otiatry; orthopædia, dentistry, chemistry, instruments of precision, with subdivision for microscopy; instruments and apparatus aiding instruction in natural history, geography, equipment for scientific travel, photography, anthropology, biology and physiology, hygiene, electro-therapeutics and neurology, and pharmacology. Applications are to be addressed to the Exhibition Committee, 44 Frankfurterstrasse, Wiesbaden.

ON Friday, the 3rd inst., the work of constructing the canal which is to connect the German Ocean with the Baltic Sea was formally begun by the German Emperor. The ceremony, which took place at Holtenau, on the Bay of Kiel, consisted of the laying of the foundation-stone of a lock near the Baltic end of the canal. It is estimated that the total cost of the undertaking will be 156,000,000 marks (about £7,800,000). This sum has already been voted by the Reichstag and the Prussian Parliament. The canal is being constructed mainly for naval and military purposes, but in times of peace it will be open to the merchant ships of all nations. The German authorities calculate that it will be used annually by about 18,000 vessels, with a collective tonnage of 5,500,000, and yielding a revenue of 4,125,000 marks (about £206,250).

ON Wednesday, the 15th inst., at 3 p.m., Sir H. W. Acland will distribute prizes to students at the Medical School of St. Thomas's Hospital. The ceremony will take place in the Governors' Hall.

LORD CADOGAN has offered to present a site for the Free Public Library which is to be erected in Chelsea. He also promises to give £300 worth of books, to which Lady Cadogan adds a gift of £50.

ON Tuesday evening a meeting, held in the lecture-hall of the Polytechnic Institution, Regent Street, decided that an effort should be made to secure the establishment of a Free Public Library in Marylebone. Prof. Huxley, who presided, said it was proposed that £20,000 should be raised to cover the cost of the site and building, and he was able to announce that £10,500 had already been promised. If they succeeded in their object, as he was sure they would, they could go to the authorities and the ratepayers and say, "We have done our part of a public duty, now perform yours."

THE Corporation of London have voted a donation of one hundred guineas towards the thousand pounds required by the Bethnal Green Free Library Committee for the further development of the Institution.

ON the 3rd inst. several shocks of earthquake occurred in Northern California and Western Nevada. They were distinctly felt in the Yosemite Valley.

DURING the earthquakes in the Sierra Madre, five persons were killed and nineteen injured at Bapipe (Sonora province), and five persons were killed at Oputa. Both towns were completely destroyed. The inhabitants, as well as those of the towns of Barceraca and Quasa, are living in the open fields, shocks being still felt continually. Some places which were quite dry formerly are now submerged.

THE Oficina Central Meteorolójica de Chile is endeavouring to keep up and improve the meteorological service of the Republic. Vol. xviii. of its *Anuario*, containing the observations for the year 1886, has been recently published. The first observations published by the Office were for 1869, but for several years past the publication has fallen into arrears for want of sufficient funds. Since the re-organization of the Office in 1885, the *Anuario* has appeared in two-monthly parts, and the management of the service is now intrusted to a Committee composed of members of the Faculty of Sciences at the University of Santiago. There are now twenty-eight stations at which observations are being taken or in course of establishment; the most northerly station is Iquique (lat. 20° 12′ S.), and the most southerly Punto Arenas (lat. 53° 10′ S.), but there are none between this and Ancud (lat. 41° 52′ S.). Many of the stations are provided with the best instruments, ordered from Europe, and the Central Office has a complete outfit of self-recording instruments. The Astronomical Observatory at Santiago has also published meteorological observations, independently, from 1873–84, together with curves of the automatic records.

THE *Annalen der Hydrographie und maritimen Meteorologie* for May contains a notice of a fall of volcanic ashes, at Finsch Harbour, on the eastern coast of New Guinea, which lasted from 7 a.m. till about 11 a.m. on February 5 last, and covered the country round with a thin layer of light-gray ashes. On February 2 the whole sky appeared gray, and at noon the sun was of a blood-red colour, while lunar halos and rings occurred for several nights. Captain von Schleinitz states that the north-west monsoon, which prevails at this season, had ceased for four days, and was replaced by fresh southerly winds, but made its appearance again at noon on the 4th. From this he concludes that the volcanic eruption which caused the fall of ashes might have occurred either to the north or south, as the fall did not take place with either the northerly or southerly wind, but during a calm, the ashes having remained suspended for some time. He thinks that a northerly origin is most probable, although they might have been carried thither from an easterly or westerly direction by an upper air-current, or that the volcanoes in Vulcan Island and Lesson Island may have shown unusual activity during the period in question.

THE Monthly Weather Review, published by the Chief Signal Officer of the United States, and referred to in our issue of last week (p. 110), has now been received for the six months ending December 1886, making the series complete up to February 1887.

The recent cyclone in the Bay of Bengal, to which we referred last week (p. 110), did much injury both on sea and land. Commenting on the fact that the Viceroy has telegraphed to the Sheriff of Calcutta expressing the regret and sympathy felt by himself and the members of the Government, the Calcutta Correspondent of the *Times* says:—"It is to be hoped that this sympathy will take the practical form of the authorizing an extension of the telegraph to the Andamans and Diamond Island. The latter locality is now recognized as the birth-place of cyclones, and the importance of getting early intimation of their approach cannot be over-estimated."

WE have to record the death of Dr. Karl Friedländer, Professor at the Berlin University, an eminent pathologist and anatomist; also of Dr. Alexander Ecker, Professor at the Freiburg University, a well-known anatomist and anthropologist, and founder of the Ethnographical Museum at Freiburg. Dr. Ecker died at the age of seventy.

THE General Meetings of the Geographical Society and the Botanical Club of Thuringia took place at Saalfeld on June 5.

THE largest Apicultural Meeting and Exhibition ever held in Germany will take place at Hanover from September 22 to 25.

THE Council of the Royal Meteorological Society are anxious to obtain photographs of flashes of lightning, as they believe that a great deal of research on this subject can be pursued only by means of the camera. In a letter which has been sent to persons likely to be interested in the subject, the Council express a hope that now the thunderstorm season is approaching, many photographers may be found willing to take up this branch of their art. It is pointed out that the photography of lightning does not present any particular difficulties. If a rapid plate and an ordinary rapid doublet with full aperture be left uncovered at night during a thunderstorm for a short time, 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 NEW chlorobromide of silicon has been isolated by Prof. Emerson Reynolds from a large quantity of crude silicon tetrabromide. A current of dry hydrogen was first driven through the crude bromide to remove free bromine, and the residue, after shaking with mercury, was afterwards subjected to fractional distillation. An early fraction, that passing over between 140° and 144°, was separately collected, and, by repeated refractioning, was found to consist largely of a portion boiling at 140°–141°, which proved to be pure silicon chlorotribromide, $SiClBr_3$. This liquid fumes in the air, and, on addition of water, is decomposed into a mixture of silicic, hydrobromic, and hydrochloric acids. It is of considerable theoretical interest, inasmuch as it completes our knowledge of the following series of compounds, in which chlorine and bromine mutually replace each other, and the end members of which are formed by the tetrachloride and the tetrabromide of silicon respectively: $SiCl_4$, $SiCl_3Br$, $SiCl_2Br_2$, $SiClBr_3$, and $SiBr_4$. This series is now perfectly analogous to the one formed by the compounds of chlorine and bromine with carbon.

A NEW quantitative reaction of very wide application, by means of which any desired substitution of chlorine may be readily effected in a large number of hydrocarbons, is described by MM. Colson and Gautier in the last number of the *Annales de Chimie et de Physique*. It simply consists in heating in a sealed tube the calculated quantities of hydrocarbon and phosphorus pentachloride on the supposition, shown by analysis to be well founded, that the pentachloride is dissociated into the trichloride and free chlorine, which latter acts precisely like free chlorine. The great value of this means of substitution is at once seen to consist in the fact that, instead of the uncertain results obtained by the graduated use of free chlorine, it now becomes possible to obtain a quantitative yield, in an easily separable form, of the particular chlorine derivative desired. Thus, in order to obtain benzal chloride, the starting-point in the artificial preparation of indigo, it is only necessary to heat together to 195° for two hours in a closed vessel the calculated quantities of toluene and phosphorus pentachloride.

WE understand that the Rev. J. B. Lock intends to write a "Statics for Beginners" as a complement to the "Dynamics for Beginners" which was recently noticed in these columns. A Key is now being prepared, under Mr. Lock's superintendence, to his well-known "Arithmetic for Schools."

IN their report for the session 1886–87 the Council of the Institute of Actuaries say that the prosperity of the Institute has been fully maintained during the past year. Referring to the new offices at Staple Inn Hall, the Council trust that they have at last secured a permanent and suitable home for the Institute, corresponding to its higher dignity and its ever-increasing duties.

AN International Exhibition is to be held at Glasgow during the summer of 1888. The guarantee fund already exceeds £240,000, and is being increased. The objects of the Exhibition, as stated in the prospectus, are "to promote and foster industry, science, and art, by inciting the inventive genius of our people to still further development in arts and manufactures; and to stimulate commercial enterprise by inviting all nations to exhibit their products, both in the raw and finished state." Examples of the manufactures of Glasgow and the surrounding districts—chemical, iron, and other mineral products, engineering, shipbuilding, electrical and scientific appliances, and textile fabrics—will be shown; and similar and more varied exhibits may be expected from other parts of Great Britain and from the Continent. Promises of support have also been received from America, India, the Canadian, Australian, Cape, and other colonies. The site, which has been granted by the Glasgow Corporation, extends to sixty acres, and the buildings will cover about ten acres.

IN an article in the current number of the *Entomologist*, Mr. J. T. Carrington speaks of a phenomenon which has frequently puzzled him when hunting for insects on salt-marshes. He refers especially to the marshes of the River Medway. The tide often completely overflows the marshes, and for an hour or two turns the collecting-ground into an arm of the sea, with multitudes of rippling wavelets. During this period there is not a sign of an insect flying over the water. "As the tide recedes, and little islands of the taller plants appear through the water," says Mr. Carrington, "we notice the first indication of moths appearing. When the water has left the marsh we examine the wet and sloppy ground, and find multitudes of delicate Tortrices and plumes in perfect condition, flitting about as though nothing had happened to disturb their comfort. Now, where were these moths when the tide covered the marsh some two or three feet deep? One can hardly imagine they were under the water all the time, though there was not a sign of them over it. Many times have I watched this rising and falling of the tide, but never solved the problem."

IN the June number of the *Zoologist*, Mr. Murray A. Mathew describes what he calls "a strange capture of a hare." A neighbour of his in Pembrokeshire was crossing one of his fields late in the evening when he heard a hare crying. He went in the direction, expecting to find a hare in a trap, but was astonished to come across one attacked by a hedgehog, which was holding on to one of its hind legs. The hare (fully-grown) seemed paralyzed by fear, and allowed itself to be lifted up. Directly the hedgehog was shaken off the hare died, although the injury it had received from the bite of its assailant was but slight.

A NEW weekly newspaper, devoted more especially to the commercial side of the chemical and allied industries, is being issued by Messrs. Palmer and Howe, of Manchester. It is called the *Chemical Trade Journal*, and is edited by Mr. George E. Davis. Two numbers have already appeared.

THE additions to the Zoological Society's Gardens during the past week include two Egyptian Jerboas (*Dipus ægyptius*) from Egypt, a Moorish Toad (*Bufo mauritanica*) from Tunis, presented by the Hon. Terence Bourke; a White-crowned Pigeon (*Columba leucocephala*) from the West Indies, presented by Lieut.-Colonel W. G. Dawkins; a Common Trumpeter (*Psophia crepitans*) from Demerara, presented by Mr. G. H. Hawtayne; a Crowned Horned Lizard (*Phrynosoma coronatum*) from Texas, presented by Mr. Claude A. Millard; two Egyptian Jerboas (*Dipus ægyptius*) from Egypt, deposited; two Cape Sparrows (*Passer arcuatus*), four Alario Finches (*Alario alario*) from South Africa, purchased; a Wapiti Deer (*Cervus canadensis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

COMET 1887 *e* (BARNARD, MAY 12).—Dr. H. Oppenheim supplies the following improved elements for this comet in Dun Echt Circular No. 147:—

T = 1887 June 17·2209 Berlin M.T.

$$\left.\begin{aligned} \pi - \Omega &= 15^\circ\ 40'\ 19'' \\ \Omega &= 245\ \ 13\ \ \ 1 \\ \iota &= 17\ \ 31\ \ 52 \end{aligned}\right\}\text{Mean Eq. 1887·0.}$$

$$\log q = 0·14288$$

Ephemeris for Berlin Midnight.

1887.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Brightness.
June 13	16 13 43	7 50·3 S.	9·6006	0·1432	1·5
15	16 18 17	6 20·5			
17	16 22 51	4 53·9	9·6077	0·1429	1·5
19	16 27 26	3 30·8			
21	16 32 1	2 11·4 S.	9·6182	0·1433	1·4

The brightness on May 14 has been taken as unity.

MINOR PLANET NO. 266.—This object has received the name of Aline.

THE PARALLAX OF α TAURI.—Prof. Asaph Hall has published in the *Astronomical Journal*, No. 156, a determination of the parallax of this star deduced from measures of the position-angle and distance of the eleventh magnitude companion made with the Washington 26-inch refractor between October 2, 1886, and March 15, 1887. The resulting values of the relative parallax are: from measures of angle, $\pi = +0''·163 \pm 0·0409$, and from measures of distance, $\pi = +0''·035 \pm 0·0431$. The mean value of the parallax of α Tauri from these observations is therefore $\pi = +0''·102 \pm 0''·0296$. It will be remembered that M. O. Struve recently published a determination of the parallax of this star, referred to the same comparison-star, and found $\pi = +0''·516 \pm 0''·057$.

MADRAS MERIDIAN OBSERVATIONS.—A volume of Madras astronomical observations at last! In 1887 Mr. Pogson publishes the results of the meridian circle work during 1862, 1863, and 1864. A prefatory epistle addressed to Sir M. E. Grant-Duff, late Governor of Madras, speaks of "the removal of certain arbitrary and suppressive restrictions which have prevented me and my predecessors from attempting anything of the kind for considerably more than thirty years past," but gives the reader no more definite information as to the reason of this unparalleled delay in publication, nor why the Madras Observatory should have thus fallen from the high position which it formerly held. The instrument with which the observations now published were made is a transit-circle constructed by Messrs. Troughton and Simms, in consultation with the late Mr. Carrington. The object-glass is of 5½-inches aperture, and the circle of 42-inches diameter. It was brought into use in May 1862, and was devoted to the observation of stars down to the fifth magnitude, the moon and accompanying stars, Mars and comparison stars at successive oppositions, minor planets, and as many stars of more than 120° N.P.D. as could be found, not less than the eighth magnitude. The present volume contains star places only. The ledgers and catalogues of mean places for each year are given separately and take up much more space in printing than is necessary for mere annual results. Altogether 227 stars were observed in 1862, 782 in 1863, and 1000 in 1864. A comparison between the Madras places of time-stars and those of the Nautical Almanac (on the R.A.'s of which those of Madras depend) shows a good agreement in R.A., but in N P.D. a mean excess of the former of $+0''·7$, which "renders it certain that the Polar Distances will require some further small correction before being formed into a final general Catalogue."

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 JUNE 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 June 12.

Sun rises, 3h. 45m.; souths, 11h. 59m. 29·5s.; sets, 20h. 14m.; decl. on meridian, 23° 9′ N.: Sidereal Time at Sunset, 13h. 37m.

Moon (at Last Quarter on June 13) rises, 0h. 5m.; souths, 5h. 13m.; sets, 10h. 30m.; decl. on meridian, 10° 32′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury ...	4 46 ...	13 16 ...	21 46 ...	25 12 N.
Venus	7 4 ...	15 6 ...	23 8 ...	21 21 N.
Mars	3 2 ...	11 9 ...	19 16 ...	22 7 N.
Jupiter... ...	14 57 ...	20 16 ...	1 35* ...	8 51 S.
Saturn... ...	6 6 ...	14 11 ...	22 16 ...	21 49 N.

* Indicates that the setting is that of the following morning.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·3 ...	81 16 N. ...	June 13,	0 55 *m*
			,, 18,	0 35 *m*
R Crateris	10 55·0 ...	17 43 S. ...	,, 15,	*m*
U Virginis	12 45·4 ...	6 10 N. ...	,, 13,	*M*
R Hydræ	13 23·6 ...	22 42 S. ...	,, 12,	*m*
R Boötis	14 32·2 ...	27 14 N. ...	,, 18,	*m*
δ Libræ	14 54·9 ..	8 4 S. ...	,, 18,	1 26 *m*
U Coronæ	15 13·6 ...	32 4 N. ...	,, 14,	21 31 *m*
U Ophiuchi... ...	17 10·8 ...	1 20 N. ...	,, 15,	1 0 *m*
W Sagittarii ...	17 57·8 ...	29 35 S. ...	,, 13,	1 0 *M*
R Scuti	18 41·5 ...	5 50 S. ...	,, 18,	*m*
β Lyræ...	18 45·9 ...	33 14 N. ...	,, 17,	2 0 *M*
R Delphini	20 9·5 ...	8 45 N. ...	,, 18,	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near β Lyræ	282	32 N.	
ζ Cygni	320	32 N.	
β Piscium	345	0	Very swift.

GEOGRAPHICAL NOTES.

IT may interest our readers to know that a full account of Baron Nordenskjöld's narrative of his very interesting journey across Greenland has been published in German by Brockhaus, of Leipzig, with numerous maps and illustrations. Doubtless, like the same explorer's previous narratives, it will soon appear in an English dress. We are assured that Nordenskjöld will not undertake any Antarctic expedition before 1888 or 1889, if, indeed, he undertakes it at all, which is highly doubtful. He has much to do still before the publications connected with the *Vega* Expedition are complete, and he has a variety of other work in hand which must be finished before he enters on any new undertaking.

THE paper read at Monday's meeting of the Royal Geographical Society was one of unusual novelty and interest. It described the exploration which Mr. H. E. M. James, of the Bombay Civil Service, in company with two friends, made last spring and summer in Manchuria. The region explored extends from the Yellow Sea to beyond 45° N., and between 122° and 130° E. long. A considerable section of the journey was over virtually new ground, and as Mr. James is a careful observer, and, we believe, a botanist, and an accurate describer, his paper is of some scientific value. He has at least been able to add some precise features to our maps of the region. The paper contains a useful general account of Manchuria and its history. Mr. James calls it the Manitoba of Asia. What with the depletion of the country for military service and the influx of immigrants from China, there is little of the old Manchu population left. Nearly all special Manchu customs have disappeared, and the language itself is now only spoken in a few remote valleys. Mr. James's party started from Newchang and went north to Mukden. Thence they went due east up the beautiful and well-wooded valley of the Hun. This is a particularly rich region, and is being rapidly colonized. The first day Mr. James began to collect he found no less than five kinds of lilies of the valley, and it was common to see whole hill-sides covered with masses of that flower. On account of the flooded state of the rivers, it took them a month to reach Mau-erh-shan, the furthest Chinese outpost on the Yalu, at the south foot of the Lao-ling Mountains. Thence they struck northwards across the mountains to the junction of the Sungari and Tang-ho, four days march. Here they looked in vain for the snowy peaks of 10,000 to 12,000 feet high, reported by previous writers on Manchuria; they were assured no such peaks existed in all the region. An official guided them back south-east to the Pei-shan Mountains, a sort of knot in which the Yalu, the Tumen, and the Sungari take their rise. For a long distance the route was over a succession of ranges covered with dense forests, with only at long intervals a hut of a ginseng cultivator, sometimes in the crater of an old volcano. Bogs also were frequent, and gave much trouble. It was the ninth day before they actually began to ascend the mountain itself. The lower slopes are covered with birch and pine, leading to a delightful grassy plateau dotted with trees, and rich open meadows bright with flowers of every imaginable colour. As they approached the needle-like peaks of Old White Mountain, the noise of underground streams was frequently heard. The steep sides of the two-peaked upper ridge shines white with disintegrated pumice-stone. On reaching the saddle and looking over the edge, the party found themselves looking down into a crater, at the bottom of which, about 350 feet below, was a beautiful lake, of the deepest and most pellucid blue. The lake was about 6 miles in circumference. The height of the mountain is not more than 8000 feet. The party then proceeded north to Kirin and Tsitsikar, through forests and swamps, and, lastly, across Mongolian steppes. Then, proceeding eastwards and southwards, the country to the east of the previous route was explored, Mr. James learning much by the way of the country and the people. Altogether the journey has been a fruitful one, and shows how much can be done for science by our Indian officials when they have the inclination and are properly trained.

We have already referred to the remarkable journey of Mr. Carey in Central Asia. Information has now been received from him showing how the second year of his journey was passed. In May 1886 he started from Châklik, with the object of exploring some of the northern regions of Tibet. He passed south, across the Altyn and Chinan Mountains, and reached the foot of a high chain, which is probably the true Kuen-lun. Here he had to travel a considerable distance eastwards, through barren and difficult country, until an opening was found leading to the valley of the Ma-chu, the head source of the Yang-tse-kiang. After falling down the river some distance, Mr. Carey had to turn northwards again, and recross the Kuen-lun. He now found himself in the Tsaidam region, and made an interesting round journey from a place called Golmo and back to the same point, during which he saw a good deal of the nomadic Kalmucks and Mongols who inhabit the comparatively low valleys of Tsaidam. In the autumn, Mr. Carey made a second journey across the Kuen-lun, and then, again turning northward, struck straight across the Tsaidam country and the Gobi, to Sáchan and Hami, whence he travelled to Urumtsi in the Tian-shan. Thence the party left for Yarkand, whence a start was made on March 7 for Ladak. A great part of the ground traversed by Mr. Carey is new, and he and his assistant, Mr. Dalgleish, are the only Englishmen who have ever travelled through the entire length of Chinese or Eastern Turkestan.

M. Constantin Nosiloff writes to the Royal Geographical Society of his intention to undertake this year a summer expedition to Nova Zembla. His object will be (1) to prepare a detailed map of the coasts and interior of the island; (2) to study the hydrography of the coast, and make observations regarding the movements of the ice in the Kara Sea, and in the straits leading to it; (3) to make meteorological observations, and to collect zoological and botanical specimens; (4) to study the ethnography of the Samoïedes.

THE ANNUAL VISITATION OF THE ROYAL OBSERVATORY.

The Report of the Astronomer-Royal to the Board of Visitors, read at the annual visitation of the Royal Observatory on Saturday last, refers to the period of twelve months from 1886 May 21, to 1887 May 20, and exhibits the state of the Observatory on the last-named day.

The following are among the points of most general interest:—

I. Buildings and Grounds.

Above the extended portion of the upper computing-room, a dome 18 feet in diameter is to be erected, in which it is proposed to mount a Cooke 6-inch equatorial, a photo heliograph tube being attached to the same mounting. The combined instrument will command a complete view of the sun throughout the day—an important consideration, as the work of the present photoheliograph is seriously interfered with by trees and the Lassell dome. The new instrument will be available for occultations, phenomena of Jupiter's satellites, and other occasional observations.

II. Astronomical Observations.

Transit-Circle.—The regular subjects of observation with the transit-circle are the sun, moon, planets, and fundamental stars, with other stars from a working Catalogue. On the conclusion of the observations for the Ten-Year Catalogue at the end of 1886, a new list of some 3000 stars was prepared, to include all the stars in Groombridge's Catalogue and in the Harvard Photometry, which had not been observed at Greenwich since 1867. The Annual Catalogue of stars observed in 1886 contains about 1665 stars.

The observations for the Ten-Year Catalogue, epoch 1880, were concluded at the end of 1886, special efforts being made in the latter part of the year to make the Catalogue as far as possible complete to the sixth magnitude inclusive. It is estimated that the Catalogue will contain about 4000 stars, all of which, with very few accidental exceptions, have been observed at least three times in R.A. and N.P.D., the total number of observations being about 40,000 in each element.

The following statement shows the number of observations made with the transit-circle in the twelve months ending 1887 May 20:—

Transits, the separate limbs being counted as separate observations ...	6366
Determinations of collimation error ...	304
Determinations of level error ...	410
Circle-observations ...	5983
Determinations of nadir point (included in the number of circle-observations ...	385
Reflexion-observations of stars (similarly included) ...	602

About 400 transits (included in the above number) have been observed with the reversion-prism, to determine personality depending on the direction of motion.

The value found for the colatitude from the observations of 1886 is 38° 31′ 22″·03, differing by 0″·13 from the assumed value; the correction to the tabular obliquity of the ecliptic is +0″·65, and the discordance between the results from the summer and winter solstices is −0″·25, indicating that the mean of the observed distances from the Pole to the ecliptic is too great by +0″·12.

The mean error of the moon's tabular place (computed from Hansen's lunar tables, with Prof. Newcomb's corrections) is +0·029s. in R.A. and +0″·34 in longitude as deduced from ninety-seven meridian-observations in 1886. The mean error in tabular N.P.D. is −0″·66, which would appear to agree with the observations of the sun in indicating that the mean of the observed N.P.D.'s is too great.

As regards the computations for the Ten-Year Catalogue, a large amount of preparatory work has been done in the application of corrections to the observations as printed to reduce them to a homogeneous system, and some progress has been made in the formation of the Catalogue results. The proper motions actually used have been thoroughly revised for every observation in the period 1877-86, and corrections applied where, as occasionally happened, different proper motions had been used in the same year. A comparison has been made of the R.A.'s of clock-stars as observed in the last ten years and as computed from the Nine-Year Catalogue, epoch 1872, with Auwers' recently published proper motions, the result of which is to show that the Greenwich observations are better represented by these than by the proper motions in use hitherto, and it has therefore been decided to adopt Auwers' proper motions throughout. Preparations have accordingly been made for reducing the observations in the Ten-Year Catalogue to the epoch 1880, with Auwers' proper motions wherever available.

It has appeared doubtful whether the reading of the exterior thermometer placed near the north wall of the transit-circle room represents the true temperature of the external air as affecting the refraction for the sun and other southern objects in the daytime. A discussion of simultaneous readings of the exterior, front court, and meteorological standard thermometers, which is being made by Mr. Thackeray, shows systematic differences between the first and last at the time of observation of the sun, the mean monthly excess of the meteorological standard over the exterior thermometer for the ten years 1877–86 ranging from $+0^{\circ}\cdot7$ in December to $+2^{\circ}\cdot1$ in May and August and $+2^{\circ}\cdot6$ in September. The reading of the front court thermometer (which is at a distance from any building) appears to agree closely with that of the meteorological standard, and it has been adopted, from the beginning of this year, in computing refractions for the sun, moon, planets, and stars south of the zenith observed in the daytime, the exterior thermometer being still used for northern stars as probably representing better the temperature of the air on the north side of the transit-circle. The systematic differences in thermometer readings have a sensible effect on the position of the ecliptic as deduced from observations of the sun, the discordance in the results between the summer and winter solstices found when the reading of the exterior thermometer are used being rendered insensible when corrections are applied to reduce to the reading of the meteorological standard thermometer.

Altazimuth.—The total number of observations of various kinds made in the twelve months ending 1887 May 20 is as follows, the observations of the moon having been as usual restricted to the first and last quarters in each lunation :—

Azimuths of the moon and stars	356
Azimuths of the azimuth mark	208
Azimuths of the collimating mark	242
Zenith distances of the moon	181
Zenith distances of the collimating mark ...	240

The altazimuth observations are completely reduced to March 31, so as to exhibit errors of moon's tabular R.A., N.P.D., longitude, and ecliptic N.P.D., and the manuscript for the printer has been prepared to the same date.

Equatorials.—Various additions have been made to the Lassell equatorial with a view to making it available for astronomical photography and for general use. A delicate slow motion in R.A. (with differential wheels) and a firm N.P.D. clamping arm with fine motion in N.P.D. have been applied, the steadiness and general usefulness of the telescope being greatly increased by these additions. The Corbett 6½-inch refractor has been mounted below the tube of the reflector and parallel to it to serve as a directing telescope in taking photographs and also for observation of occasional phenomena. A camera to take circular plates 8¼ inches in diameter (giving a field 1° 58′ in diameter) has been mounted at the principal focus of the Lassell mirror, and some trial photographs of the moon, Procyon, Regulus, γ Leonis, and Præsepe, have been taken.

The construction of the new 28-inch refractor has been delayed by difficulty in obtaining the disks of glass. Messrs. Chance are engaged in removing a bunch of fine veins from the flint glass disk, and have every hope of being able very shortly to report the disk practically perfect ; and M. Feil's successor has successfully moulded a crown disk from which he believes that he has removed all defects.

The south-east and Sheepshanks equatorials are in good order. Some trouble has been experienced with the water-supply for the driving clock of the former instrument, and an alteration in the arrangements for maintaining the pressure has been made at the Kent Waterworks, since which the working has been found quite satisfactory.

The Cooke 6-inch equatorial is being mounted in the south ground for trial as to the practicability of using curved plates for stellar photography and other questions which have been raised at the Paris Conference on Astronomical Photography.

III. Spectroscopic and Photographic Observations.

For determination of the motions of stars in the line of sight, 206 measures have been made of the displacement of the F line in the spectra of 26 stars, and 20 measures of the *b* lines in 8 stars, besides comparisons with lines in the spectrum of the moon made in the course of the night's observations of star motions, or of the sky on the following morning, as a check on the general accuracy of the results. The observations of Sirius since the date of the last Report indicate that the apparent displacement of the F line (which was originally towards the red and subsequently towards the blue) is now insensible. The displacement of the F line in the spectrum of Algol has been measured as frequently as possible during the winter months, in order to ascertain if any evidence could be obtained of rapid orbital motion such as would result from the hypothesis of the variability of Algol being caused by the transits of a large satellite. A sufficient number of observations has not yet been obtained to allow a definite conclusion to be formed, but as far as the observations go there are indications of a variation of the motion in the line of sight corresponding to orbital motion, having the same period as that of the star's variability.

A photographic corrector, consisting of a concave crown and convex flint lens (in contact), placed about 30 inches within the focus, has been applied to the telescope of the south-east equatorial to correct the chromatic aberration of the object-glass for the photographic rays without alteration of the focal length. A Dallmeyer doublet (formerly used in the photoheliograph) has been employed to enlarge the primary image about 7½ times, so as to give on the photographic plate an image on a scale of about 0·45 inch to one minute of arc, or 15 inches to the sun's diameter. A number of trial photographs of Castor, γ Virginis, Venus, Jupiter, and Saturn have been obtained. The photographs of the double stars appear to be susceptible of very accurate measurement, and several of the photographs of Jupiter show the four satellites, the belts, and the red spot. A photograph of γ Virginis, showing the components widely separated, has also been taken at the primary focus, the Dallmeyer enlarging doublet having been removed. It is intended also to use the photographic corrector with the Dallmeyer doublet to obtain photographs on a large scale of sunspots, craters on the moon, and other objects of small angular dimensions. The field of view with the photographic corrector is necessarily very restricted.

For the year 1886, Greenwich photographs are available on 199 days, and photographs from India and Mauritius filling up the gaps in the series on 164 days, making a total of 363 days out of the 365 on which photographs have been measured, the record being thus practically complete for 1886.

As regards the photographic reductions :—

The Greenwich photographs have been measured in duplicate as far as 1887 April 28, and the measures have been completely reduced so as to exhibit heliographic longitudes and latitudes of spots and areas of spots and faculæ.

The photographs from India and Mauritius have been received from the Solar Physics Committee as far as March 10 and February 20 respectively, and these have all been measured, and the measures completely reduced.

IV. Magnetical Observations.

The observations have been continued on the same lines as in former years, changes in the magnetic declination, horizontal force, and vertical force being continuously recorded by photography and the absolute values of magnetic declination, horizontal force, and dip being determined from time to time by eye-observation. Earth currents in two directions nearly at right angles to each other are also photographically registered. For these last the ordinates have hitherto been measured on an arbitrary scale, and it appeared desirable to obtain the data for expressing this in terms of the accepted electrical units. The authorities of the Post Office Telegraphs have courteously given every assistance in regard to the requisite electrical measurements, and an electrical balance for measuring resistance, a standard cell, and a galvanometer of the Post Office pattern have been procured under their auspices. In October last, Mr. H. R. Kempe, of the Post Office Telegraphs, made some measures of the resistances of the earth current wires, but the conditions were not then favourable for insulation. Subsequently the wires were damaged in the snowstorm of December 26–27 last, and were temporarily repaired on January 25. It is believed that they are now restored to their normal condition, and arrangements are being made to obtain the value of the difference of electric potential between the two earth-plates on each line corresponding to a given length of ordinate on the photographic register. An experimental set of measures of resistance has been taken recently.

The following are the principal results for magnetic elements for 1886:—

Approximate mean declination	17° 55′ West
Mean horizontal force... ...	3·9379 (in British units)
	1·8157 (in Metric units)
Mean dip	67° 26′ 38″ (by 9-inch needles)
	67° 26′ 45″ (by 6-inch needles)
	67° 27′ 40″ (by 3-inch needles)

The declination and horizontal force magnets were thrown into vibration by the earthquake shock of February 23, the extent of vibration being 20′ in declination and 0·004 of the whole horizontal force in that element. The motion commenced at 5h. 37·6m. Greenwich civil time, and a second double disturbance of much smaller amplitude (possibly accidental) was registered from 7h. 39m. to about 7h. 57m. At the request of M. Mascart, a copy of the photograph has been sent to him for discussion with other records of the earthquake which he is collecting. In view of the importance of the study of earthquakes, it appears desirable that a suitable seismograph should be procured for the Observatory.

V. Meteorological Observations.

The mean temperature of the year 1886 was 48°·7, being 0°·6 below the average of the preceding forty-five years. The highest air temperature in the shade was 89°·8 on July 6, and the lowest, 16°·5, on January 7. The mean monthly temperature in 1886 was below the average in January, February (6°), March, June, and December, and above the average in September, October, and November. In the period of 156 days from 1886 December 16 to 1887 May 20 the mean temperature was 3°·1 below the average of twenty years, the daily temperature being below the average on 115 days.

The mean daily motion of the air in 1886 was 291 miles, being 7 miles above the average of the preceding nineteen years. The greatest daily motion was 857 miles on December 8, and the least, 56 miles, on October 8. The recorded pressures in 1886, exceeding 20 lbs. on the square foot, were 27·6 lbs. on March 31, and 23·5 lbs. on December 9.

During the year 1886, 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 by Campbell's sunshine instrument during 1886 was 1228, which is about twenty hours above the average of the preceding nine 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 0·276, constant sunshine being represented by 1.

The rainfall in 1886 was 24·2 inches, being 0·5 inch below the average of the preceding forty-five years.

VII. Chronometers, Time Signals, and Longitude Operations.

The number of chronometers now being tested at the Observatory is 225.

The first seven chronometers in the competitive trial of 1886 were exceptionally good, the first chronometer being superior to any we have previously had on trial, except the first in 1882.

For the annual trial of deck-watches, which commenced last November, fifteen watches were entered, and of these nine were purchased for the Navy, the first three being classed "A," or equal, in performance, to an average box-chronometer.

A supplementary trial took place in February and March, for which nine deck-watches were entered, and of these seven were purchased for the Navy, the first two being classed "A."

The watches in each trial were rated for a period of nine weeks, viz. two weeks (dial up) in the room at a temperature of 50° to 55°, four weeks in four different positions in the oven (dial up, pendant up, pendant right, pendant left, arranged symmetrically) at a temperature of about 80°, and three weeks (dial up) in the room. When the period of rating in any position was less than a week, weekly rates were inferred from the rate for the period by simple proportion.

In order to compare the performance of the several watches, "trial numbers," representing deviations in weekly rates, have been formed on the same general principles as for the chronometer trials. The trials in different positions introduce, however, a new element, and an arbitrary weight must be assigned to them in combining them with the trials "dial up." It has been considered that when the watch is worn in the pocket the pendant will generally be "up," and that not more than one-third of the deviation "pendant right" or "pendant left" is likely to have practical effect.

Putting a = Difference between greatest and least weekly rates "dial up,"
b = Greatest difference between one week and the next "dial up,"
c = Difference between weekly rates "pendant up" and "dial up,"
d = Difference between weekly rates "pendant right" and "dial up,"
e = Difference between weekly rates "pendant left" and "dial up,"

the quantity $c + \frac{d}{3} + \frac{e}{3}$ may be taken as the measure of the deviation in weekly rate due to positions in ordinary wear. Half weight has been given to this quantity in combining it with the trial number "dial up" $(a + 2b)$, on the assumption that the deck-watch would be usually lying "dial up" and that it would not be carried in the pocket more than eight hours a day on the average. Thus the quantity $a + 2b + \frac{1}{2}(c + \frac{d}{3} + \frac{e}{3})$, has been adopted as the trial number for deck-watches. It has been arranged that for the future all pocket chronometers and deck-watches rated at the Observatory after repair shall be tested in positions.

The automatic drop of the Greenwich time-ball failed on one day only during the past twelve months. On three days the ball was not raised on account of the violence of the wind, and on five days on account of accumulation of snow on the mast.

As regards the Deal time-ball, there have been twelve cases of failure owing to interruption of the telegraph connexions, and on three days the violence of the wind prevented the raising of the ball. For fourteen days after the snowstorm of December 26–27, no signals were sent to or received from the Deal time-ball tower, telegraphic communication being interrupted. There have been four cases of failure of the 1 p.m. signal to the Post Office Telegraphs.

The arrangements for hourly time-signals at Devonport to be given by a local clock, corrected daily by the help of a time-signal at Greenwich at 10 a.m., have been carried out under Captain Wharton's directions, and a return signal from Devonport (serving as a test of the accuracy with which the local clock was corrected) has been regularly received at Greenwich (at 13h. 0m. 39s. G.C.T.) since November 22, with the exception of 36 days following the snowstorm of December 26–27, when there was interruption of the telegraphic communication with the West of England, and of 23 days when no return signal was received. The failures occurred for the most part on Sundays. The plan appears to answer well, and it seems desirous that apparatus should be provided by the Government to enable the Committee of Lloyd's to establish hourly signals at the Lizard on the same system.

The new contact apparatus of the Westminster clock was brought into action on 1886 May 22, and the automatic signals from the clock have been received regularly from that date, except on three days following the snowstorm of December 26–27. The error of the clock was insensible on 25 per cent. of the days of observation, 1s. on 40 per cent., 2s. on 22 per cent., 3s. on 11 per cent., and 4s. on 2 per cent. On one day the signal was 15s. late, and on another day 10s. late.

A suggestion has been made that in view of the importance of the connexion of the British and Continental Surveys, the telegraphic difference of longitude between Greenwich and Paris, which was originally determined with great care in 1854, should be confirmed in order to complete the network of telegraphic longitudes which have been determined of late years by Continental astronomers. It seems desirable that Greenwich Observatory, which, under Sir G. B. Airy's direction, took such an active part in utilizing the telegraph for the determination of longitude, should now assist in completing the cycle. The necessary exchange of observers and signals could conveniently be carried out in the summer of next year, when the French geodetists will, I understand, be prepared for their share of the work.

The Report concludes with the following general remarks:—

"As the result of an International Congress on Astronomical Photography held at Paris in April on the invitation of the

French Academy of Sciences, at which fifty-six representative astronomers from all parts of the world were present, a scheme has been approved for the formation of a photographic map of the heavens by the concerted action of a number of Observatories in both hemispheres. This scheme provides for two series of photographs, the one intended to contain all stars down to the fourteenth magnitude inclusive, and the other, taken with short exposure, specially designed to give accurate positions of brighter stars down to the eleventh magnitude, so that it may be possible to form an extensive Catalogue of reference-stars for the first series, and thus to give the means of accurately determining the position of any star on the photographic map down to the fourteenth magnitude. The instruments with which this work is to be jointly carried out are to be photographic refractors of 0·33m. (13 inches) aperture and 3·43m. (11 feet 3 inches) focal length, and the Directors of the following ten Observatories have already announced that they are prepared to take part in the enterprise: Algiers, Bordeaux, Paris, Toulouse, and Vienna in the northern hemisphere; La Plata, Melbourne, Rio de Janeiro, Santiago da Chile, and Sydney in the southern hemisphere. It seems fitting that Greenwich should take its share in a scheme which will in a few years so greatly extend our knowledge of the places of the fixed stars, and thus serve to carry out one of the principal objects for which the Astronomer-Royal was appointed.

"On a review of the work of the past twelve months, it will be found that the activity of the Observatory has increased in various directions. The number of meridian observations is much larger than usual; additions have been made to the work of the magnetical and meteorological branch; there have been continuous trials of chronometers and deck-watches (requiring special arrangements in each case), which have made large demands on my own time, as well as on that of Mr. Turner and of Mr. Lewis. Extraneous work in connexion with the Navy has also absorbed a good deal of time that would otherwise have been free for scientific investigations. Questions connected with gun-directors, mirrors for electric search-lights, and binoculars for the Navy, have continued to engage our attention, and since the date of the last Report 510 telescopes and 35 binoculars for the Navy have been sent to the Observatory for examination, whilst it is to be presumed that a further supply of 500 binoculars, now on order, will be forwarded here to be tested in due course.

"Whilst it seems desirable that such directly utilitarian work should be undertaken at the Observatory, as being the only existing Government establishment where it can be done efficiently, I feel it necessary to point out that the existing staff is inadequate for these extraneous duties in addition to the well-defined work for which the Observatory is primarily maintained. By great efforts, which can hardly be sustained for an indefinite period, the current reductions have been kept up, notwithstanding the large number of observations obtained in the last twelve months, but the ulterior discussions which are required to maintain the character of the Observatory as a scientific institution are falling further and further behind. Amongst other matters which I should wish to take up, if leisure could be found, I may mention the determination of proper motions of stars from the observations made at Greenwich since Sir G. B. Airy's appointment in 1835, an investigation which appears to come within the terms of the Royal Warrant directing the Astronomer-Royal 'to rectify the tables of the motions of the heavens and the places of the fixed stars.'

"The appointment of a clerk, which has presumably received the sanction of the Admiralty, will, when it is made, provide for the increase of office-work which has taken place of late years in regard to chronometers, accounts, stores, stationery, printing, &c., and if the maintenance of the telegraph-wires, batteries, &c., for communication of time-signals were undertaken by the Post Office Telegraphs as part of the distribution of time to the public, there would be some further relief. But to enable me to give time to extraneous questions referred to the Astronomer-Royal by the Government, it appears necessary that the Chief Assistant and I should be relieved of certain mechanical work which might be intrusted to computers, and that further responsibility should be delegated to the Assistants. Proceeding on the lines which have been laid down by my predecessor, I believe that the maximum of efficiency at the minimum of cost would be attained if an increase of work were met by an increase in the staff of computers, with due recognition of the position of two or three senior computers, and of the increased responsibility of the Assistants."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—In Convocation on Tuesday, a grant of £4800, applied for by Prof. Clifton, for the extension of the Clarendon Laboratory by the erection of buildings for an Electrical Department, was refused by a large majority.

Twenty-seven men have entered for the final schools in Natural Science this year, of whom sixteen offer chemistry, four physiology, three animal morphology and physics, and one botany.

A course of medical teaching, including clinical demonstrations and elementary surgery, is to be given at the Radcliffe Infirmary during the first half of the Long Vacation.

Besides the lectures which we announced at the beginning of term, Mr. Arthur Evans, the Keeper of the Ashmolean Museum, is giving a course of lectures on "The Early Iron Age."

CAMBRIDGE.—The twenty-first Annual Report of the Museums and Lecture Rooms Syndicate states that during the year 1886 considerable progress has been made in arranging the various collections, but additional accommodation in the form of cases and cabinets is required in various departments, especially for botany and ornithology. Additional accommodation is urgently demanded for the teaching of physiology, pathology, and botany. It is also desirable that permanent arrangements for human anatomy and medicine should be taken into consideration without further delay, and that the work should be commenced as soon as possible after the present chemical laboratory is vacated.

Mr. J. W. L. Glaisher, F.R.S., and Mr. J. S. Nicholson, Professor of Political Economy in the University of Edinburgh, have been approved for the degree of Doctor in Science.

The University having been applied to by the Association for the Improvement of Geometrical Teaching to take some steps to give improved methods of teaching geometry fair play in their examinations, and the Association having sent a deputation to Cambridge to confer with the Board of Mathematical Studies, the latter Board have recommended that other proofs than Euclid's be accepted in the Previous Examination, no proof of any proposition occurring in Euclid being admitted in which use is made of any proposition which in Euclid's order occurs subsequently. They do not at present propose modifications in the syllabus of geometry for the Mathematical Tripos, because they are about to revise the schedule of Part I. as a whole.

The recent report on the local lectures scheme shows that a fair share of attention has been devoted to natural science—namely, thirty-five out of one hundred courses of lectures. The courses on "Work and Energy" by Mr. A. Berry, delivered at five centres in the Northumberland mining district, were very successful. There is distinct progress in the systematization of work, and the development of local centres; but there are many difficulties owing to lack of endowments. Attempts are being made to connect practical courses of instruction with the scientific lectures, but here again the lack of apparatus and laboratories is a serious disadvantage. An endowment fund of £1136 has been contributed, of which more than half is given by the Local Lectures and Examinations Syndicate. The chief purpose contemplated is the retention of the services of practised lecturers.

The class list of the Natural Sciences Tripos, Part I., just issued, contains the following names in Class I.: Anderson, Cai.; Barber, Chr.; Colbeck, Cai.; D'Albuquerque, Joh.; Dufton, S. F., Trin.; Dufton, A., non-collegiate; Elliott, Chr.; Francis, King's; Fry, King's; Grabham, Joh.; Groom, Joh.; Richardson, King's; Tennant, Cai.; Turner, F. M., Trin.; Waggett, Pemb.; Wagstaff, Sid.; Williams, Cai.

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, May 28.—Prof. W. E. Ayrton, Vice-President, in the chair.—Dr. S. P. Thompson read a note on transformers for electric distribution. In the simple algebraic treatment of the dynamo several assumptions approximately true for well-made machines are made use of. The author finds that a similar set of assumptions for transformers greatly simplifies the algebraic theory:—(1) The iron, copper, and insulation are assumed good. (2) The reaction of the secondary on the primary (other than that desired) is small; thus, if the primary be supposed to be supplied with constant mean current or constant mean potential difference, this is not to be altered by the current in the secondary. (3) No magnetic leakage; so that the coefficient of mutual induction is the geometric mean between their coefficients of self-induction. (4) The quantities of copper in the primary and secondary are to be equal. These assumptions are shown to be legitimate, and the ratios of the resistances, E.M.F.'s, currents, and coefficients of self-induction are expressed in terms of the ratio of the numbers of convolutions, which ratio is represented by $p = \frac{S_1}{S_2}$. From analogy with the dynamo it is shown that $E_2 = \frac{\omega M}{\sqrt{R_1^2 + \omega L_1^2}} E_1$, where $\omega = 2\pi n$, E_1 and E_2 the E.M.F.'s of the primary and secondary respectively, and R_1 and L_1 the resistance and self-induction of the primary coil. If R_1 be negligible, the above reduces to $E_2 = \frac{\omega M}{\sqrt{\omega^2 L_1^2}} = \frac{E_1}{p}$, since $\frac{L_1}{L_2} = p^2$, and $M = \sqrt{L_1 L_2}$. The latter part of the paper contains a general investigation of two neighbouring circuits both having self-induction, and it is shown that the effective resistance of the primary is increased, and the self-induction decreased by closing the secondary circuit. Mr. Kapp said the investigations assumed the coefficients of induction to be constants, and that the phases of current in primary and secondary were opposite. The former being by no means true, he asked, What values were to be taken? and he believes the phases of current are not opposite in ordinary transformers. Mr. Swinburne protested against the use of formulæ to calculate the inductions when the required data could be obtained much more accurately from Dr. Hopkinson's curves on magnetization of iron. He also thought the curve of sines did not nearly represent the current curve for ordinary machines. Mr. Bosanquet thought the effective magnetization of a transformer would be different from that of a dynamo, for, in the former, permanent magnetism was not utilized. In reply to Mr. Kapp and Mr. Swinburne the author pointed out that as the coefficients of induction enter in both numerator and denominator, it would not matter which set of values were taken if the resistance was small compared with ωL; and that self-induction tends to smooth out irregularities in the current curve. Prof. Ayrton described a method of regulating a series transformer devised by himself and colleague some two years ago, based on analogy with a compound dynamo. Referring to the variation of L with current, he sketched a curve connecting them, obtained by Mr. Sumpner at the Central Institution, and mentioned that the E.M.F. curve of a Ferranti dynamo is an exact sine curve. He believes problems involving alternating currents would be greatly simplified by using a new set of measurable quantities, such as will render the equations as simple as possible. At Prof. Thompson's request, Prof. Ayrton exhibited a lecture experiment illustrating the action of transformers. The secondaries of two ordinary induction coils were joined in series through long fine wires, and an incandescent lamp placed in the primary circuit of one, lighted up on completing the primary of the other coil in which a battery was placed.—On magnetic torsion of iron wires, by Shelford Bidwell. This is an account of experiments made on the twisting produced by sending a current along magnetized iron wires, and the author shows that Wiedemann's explanation of these phenomena (by assuming a difference in molecular friction at the polar and lateral surfaces of magnetized molecules), is unsatisfactory. The wires were magnetized longitudinally by means of a solenoid in the axis of which the wires were suspended. To obtain consistent results it was found necessary to demagnetize the wire between the observations. This is done by reversed currents of gradually decreasing strength, and a simple arrangement of rheostat and commutator devised for this purpose was exhibited. Two sets of experiments were made, in one of which the current in wire or solenoid was kept constant whilst that in the other was varied. The amount of twisting does not increase continuously when the currents are increased, but attains a maximum when the inclination of the helix, representing the direction of magnetization, is inclined at about 33° to the axis of the wire. When the current in the solenoid was kept constant and that in the wire increased, permanent deflections remained on stopping the current. For small currents in the wire this deflection was diminished on starting the current, whilst stronger currents increased the deflection. For some intermediate value of the current, no change took place, and this value was dependent on the current in the solenoid. Experiments were shown illustrating these phenomena.

Anthropological Institute, May 24.—Mr. Francis Galton, F.R.S., President, in the chair.—Dr. George Harley, F.R.S., read a paper on the relative recuperative powers of man living in a rude, and man living in a highly civilized state; in which he brought forward a number of hitherto unpublished, though mostly well-known facts, demonstrating that the refining influence of civilization had not been altogether the unalloyed boon we so fondly imagine it to have been. For the cases cited went far to demonstrate the fact that while man's physique, as well as his mental power, had increased during his evolution from a barbaric state into a condition of *bienséance*, his recuperative capacity, on the other hand, has materially deteriorated. In fact, it appeared from the examples cited that every appliance adding to man's bodily comfort, as well as every contrivance either stimulating or developing his mental faculties, while increasing his personal enjoyments materially diminishes his animal vitality; rendering him less able to resist the effects of lethal bodily injuries, or recover from them as well and as quickly as his barbaric ancestors, or his less favoured brethren.—Mr. G. L. Gomme read a paper on the evidence for Mr. McLennan's theory of the primitive human horde: and a communication by Mr. Samuel Gason on the Dieyerie Tribe of South Australia was also received.

Mineralogical Society, May 10.—Mr. L. Fletcher, President, in the chair.—It was reported that Mr. F. Pearce, of Maritzburg, Natal, and P. of. Albert Chester, of Clinton, N.Y., had been elected members in April.—The following papers were read:—Microscopical studies on some eruptive rocks from the Caucasus and Armenia, by Dr. Hjalmar Gylling, of Helsingfors.—Note on some specimens of glaucophane rock from the Ile de Groix, by Rev. Prof. Bonney, F.R.S.—On the crystalline form of kreatine, by Mr. L. Fletcher.—Note on francolite, by Mr. F. H. Butler.—On the meteoric iron seen to fall in the district of Nejed, in Central Arabia, in the spring of 1865, by Mr. L. Fletcher.—On a granite containing andalusite from the Cheesewring, Cornwall, by Mr. J. J. H. Teall.—Prof. J. W. Judd, F.R.S., exhibited some specimens and sections of tabasheer and other forms of opal, and made some observations thereon.

Paris.

Academy of Sciences, May 31.—M. Janssen in the chair.—On the condition of stability in the movement of an oscillating system connected with a pendular synchronic arrangement, by M. A. Cornu. A solution is here offered of a problem which presents itself in the adjustment of certain apparatus of great precision employed in physics and astronomy: how to render the oscillations of a given mobile system, such as a pair of scales or a galvanometer, exactly synchronous with a corresponding periodical motion, such as that of a clock's pendulum, and the like.—On some crystallized metallic alloys of platina and tin, by M. H. Debray. Resuming his former studies of these alloys, the author here deals with those of platina and tin, with formula, $PtSn_4$; of rhodium, $RhSn_3$; of iridium, $IrSn_3$; and of ruthenium, $RuSn_3$. Osmium yields no alloy with tin, in which metal it crystallizes.—Progress of the Arago Laboratory, by M. de Lacaze-Duthiers. An account is given of the improvements lately introduced at this marine zoological station, which has been established at Banyuls. It is now fitted with a 7 horse-power steam-engine for supplying the aquarium with water, and with submarine electric lamps for studying the habits of the Mediterranean fauna.—On a fossilized tendril of *Nymphæa Dumasii*, Sap., by M. G. de Saporta. Although traces of rhizomes of Nymphæaceæ in various Tertiary formations are far from rare, the present fossil is specially remarkable for its great beauty and excellent preservation. Apart from the inner structure, which has been replaced by some amorphous substance, it retains all the exterior outlines of the organ down to the minutest superficial details.—Report on the velocities set up by the tides of the Pacific and Atlantic Oceans in a canal establishing free communication between these two basins, by M. Bouquet de la Grye. This is the Report of the Commission appointed last year at the request of M. de Lesseps to study the influence likely to be exercised on the Panama Canal now in progress by the regular rise and fall of the surrounding waters. It appears that the tidal currents, much stronger on the Pacific than on the Atlantic side, can never exceed 2½ knots, and that this velocity will be reached only for a few hours at the equinoctial syzygies every year. It is incidentally stated that the Canal will be 72 kilometres long, 21 metres wide at bottom, with a slope of 45°, and a depth of 11·50 metres below the mean level at Panama, and of 9 metres below that of Colon.—Observations of Barnard's Comet (1887 *e*) made at the Algiers Observatory with the 0·50 m. telescope, by MM. Trépied and Rambaud. These observations give, in tabulated form, the apparent right ascension, the declination, and number of comparisons with other stars for the period from May 16 to May 24; also the positions of the stars and the apparent positions of the comet for the same period.—On simultaneous linear equations with partial derivatives of the second order, by M. Painlevé. Some remarks are offered in connexion with M. Goursat's recent paper on this subject, including the explanation of a different method for obtaining the same results.—On a melograph, by M. J. Charpentier. The apparatus here described and presented to the Academy have been devised and constructed for the purpose of offering a solution of the problem relating to the fixation of musical improvisations, and are applicable to the piano type of instruments.—On the vapour-tensions of liquid cyanogen, by MM. J. Chappuis and Ch. Rivière. While studying the compressibility of cyanogen the authors have had occasion to measure some maxima tensions of this gas, with results differing considerably from those obtained by Faraday and Bunsen. The discrepancies are attributed partly to the great difficulty of introducing cyanogen free from nitre into the barometric chamber; but chiefly to the manometric methods employed by those physicists, these methods being much inferior in accuracy to the open air manometer adopted by the authors.—On the reproduction of a carbonate of soda known as urao and trona, by M. Paul de Mondésir. These remarks are intended to throw some light on the subject of sesquicarbonate of soda, under which title are grouped various more or less unsatisfactory data and observations.—Action of selenious acid on the bioxide of manganese, by M. P. Laugier. During the course of his researches to discover an oxygenated product Se_2O_5 corresponding to S_2O_5, obtained by the action of sulphurous acid on the bioxide of manganese, the author has obtained some new compounds, here described, resulting from the combination of selenious acid with the sesquioxide of manganese.—On a simplified calcimeter, by M. A. Bernard. For the apparatus here described it is claimed that it possesses several advantages over that of Scheibler, although based on the same principle.—Researches on the relations existing between the spectrum of the elements of inorganic substances and their biological action, by Mr. James Blake. The author's further researches with over forty inorganic elements confirm his previous conclusions; all except nitrogen and potassium showing a definite relation between their biological action and their conditions of isomorphism.

BERLIN.

Physical Society, May 20.—Prof. Du Bois-Reymond, President, in the chair.—Dr. Gross spoke on the electrical condition of magnets during their magnetization. His experiments were made with Joule magnets. A cylindrical piece of iron was split along its axis, and the lower half of the cylinder surrounded lengthways by the spiral wire which conveyed the magnetizing current, completely insulated from it; the ends of the upper half of the cylinder were perforated by copper spikes, which were then connected by means of copper wires with a galvanometer so as to form a closed circuit. After this circuit, which included the upper half-cylinder, had been brought into electrical equilibrium, the magnetizing current (in the spiral surrounding the other half of the cylinder) was reversed, and the galvanometer gave a throw. The direction of the current thus indicated was always opposite to that of the magnetizing current passing along the inner surface of the half-cylinder. The speaker thought himself justified in excluding the possibility of this result being due to a simple inductive action of the magnetizing current on the galvanometer circuit, inasmuch as when the iron half-cylinder was replaced by one of copper the galvanometer then gave no throw. (In the discussion which followed it was remarked, in opposition to this view, that the resistance of the galvanometer was too great to admit of its indicating a simple induced current when experimenting with the copper half-cylinder.) Similarly, Dr. Gross is inclined to exclude as an explanation any induction of the magnet upon itself, and thinks that the cause of the current is the difference of potential between the inner and outer side of the cylindrical magnet. This point he proposes to investigate carefully in a future series of experiments.—Prof. Lampe criticised two papers which appeared last year in the *Repertorium für Physik*, of which one contained an explanation of gravitation, the other treated of the motion of a Foucault pendulum. The speaker pointed out very fully the mathematical and physical mistakes which had made it possible for the author of the first paper to regard gravitation as due to the rotation of the earth.—Prof. von Bezold gave an extremely lucid description of Sprung's balance-barograph.—Prof. C. W. Vogel communicated the most recent discovery in connexion with instantaneous photography, by which it is now possible to obtain instantaneous photographs not only at night but also in the darkest places. Messrs. Goedicke and Miethe have prepared a mixture of pulverized magnesium, chlorate of potash, and sulphide of antimony, which when ignited produces an explosive lightning-like illumination of such intensity that by means of it an instantaneous photograph can be taken. The speaker then gave a demonstration of the discovery by taking photographs of several persons present; he used the artificial light, of which each flash lasted one-fortieth of a second, and in a few minutes produced a picture during the meeting. The powders, as prepared by the discoverers, cost only a few pfennigs each, and will hence readily come into general use.]

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Papers and Proceedings of the Royal Society of Tasmania for 1886.—Guide to the Science of Photo-Micrography: E. C. Bousfield (Kent).—Questions on Physics: S. Young (Rivingtons).—Encyklopædie der Naturwissenschaften, Erste Abth. 51 Lief., Zweite Abth. 42 and 43 Lief. (Trewendt, Breslau).—Elements of Physiological Psychology: G. T. Ladd (Longmans).—Les Pigmées: A. de Quatrefages (Baillière, Paris).—Official Record, New Zealand Industrial Exhibition, 1885.—Report of the Metropolitan Board of Works, 1886.—British Dogs, No. 7: H. Dalziel (Gill).—Bees and Bee-keeping, vol. ii. Part 8: F. R. Cheshire (Gill).—Reports of Experiments with various Insecticide Substances (Washington).—Our Shade-Trees: C. V. Riley (Washington).—Bulletin of the Iowa Agricultural College, November 1886 (Iowa).—Report of the Felsted School Natural History Society, 1886.—Diseases of the Hair, &c.: J. Startin (Harrison).—International Journal of the Medical Sciences, April (Cassell).—Journal of the College of Science, Imperial University, Japan, vol. i. Part 2 (Tōkyō).

CONTENTS.

THURSDAY, JUNE 16, 1887.

THE JUBILEE.

BEFORE our next number appears, most of the celebrations connected with the fiftieth anniversary of the Queen's accession will have taken place; and in London, at all events, the gorgeous ceremonials which are now being prepared for next Tuesday will have been the admiration of hundreds of thousands of Her Majesty's loyal subjects. It is therefore quite right and fitting that in a journal devoted to the progress of science, which the history of the last fifty years has shown to be the main basis of modern civilization, we should for a moment turn aside from our true function—that of fostering and recording the progress of natural knowledge—and dwell for one moment on the subject now uppermost in all minds, and dear to most British hearts. We know that in loyalty the students of Nature in these islands are second to none; and their gladness at the happy completion of the fifty years' reign, and their respect for the fifty years' pure and beautiful life, are also, we believe, second to none. But the satisfaction which they feel on these grounds is tempered when they consider, as men of science must, all the conditions of the problem.

The fancy of poets and the necessity of historians have from time to time marked certain ages of the world's history and distinguished them from their fellows. The golden age of the past is now represented by the scientific age of the present. Long after the names of all men who have lived on this planet during the Queen's reign, with the exception of such a name as that of Darwin, are forgotten; when the name of Queen Victoria even has paled; it will be recognized that in the latter half of the nineteenth century a new era of the world's history commenced. Whatever progress there has been in the history of any nation during the last fifty years—and this is truer of England than of any other country—the progress has been mainly due to labourers in the field of pure science, and to the applications of the results obtained by them to the purposes of our daily and national life.

Space utterly forbids that we should attempt to refer to the various memoirs, discoveries, and inventions which at once are suggested to the memory when one throws one's self back fifty years and compares the then condition of England with the present one; and we do not suppose that the most Philistine member of any community in our land, from the House of Lords downwards, will urge any objection against the statement.

It is quite true that some men of science take a pride in the fact that all this scientific work has been accomplished not only with the minimum of aid from the State, but without any sign of sympathy with it on the part of the powers that be.

We venture to doubt whether this pride is well founded. It is a matter of fact, whatever the origin of the fact may be, that during the Queen's reign, since the death of the lamented Prince Consort, there has been an impassable gulf between the highest culture of the nation and Royalty itself. The brain of the nation has been divorced from the head.

Literature and science, and we might almost add art, have no access to the throne. Our leaders in science, our leaders in letters, are personally unknown to Her Most Gracious Majesty. We do not venture to think for one moment that either Her Majesty or the leaders in question suffer from this condition of things; but we believe it to be detrimental to the State, inasmuch as it must end by giving a perfectly false perspective; and to the thoughtless the idea may rise that a great nation has nothing whatever to do either with literature, science, or art—that, in short, culture in its widest sense is a useless excrescence, and properly unrecognized by Royalty on that account, while the true men of the nation are only those who wield the sword, or struggle for bishoprics, or for place in some political party for pay.

The worst of such a state of things is that a view which is adopted in high quarters readily meets with general acceptance, and that even some of those who have done good service to the cause of learning are tempted to decry the studies by which their spurs have been won.

If literature is a "good thing to be left," as Sir G. Trevelyan has told us, if Mr. Morley the politician looks back with a half-contemptuous regret to the days when he occupied a "more humble sphere" as a leader of literature, if students are recommended to cultivate research only "in the seed-sowing time of life;" are not these things a proof that something is "rotten in the State," even in this Jubilee year? It surely is well that literature, science, and art should be cultivated by men who are willing to lay aside vulgar ambition of wealth and rank, if only they may add to the stock of knowledge and beauty which the world possesses. It surely is not well that no intellectual pre-eminence should condone for the lack of wealth or political place, and that as far as neglect can do it each scientific and literary man should be urged to leave work, the collective performance of which is nevertheless essential to the vitality of the nation.

We venture to think that our view has some claims for consideration when we note what happens in other civilized countries. If we take Germany, or France, or Italy, or Austria, we find there that the men of science and literature are recognized as subjects who can do the State some service, and as such are freely welcomed into the councils of the Sovereign. With us it is a matter of course that every Lord Mayor shall, and every President of the Royal Society shall not, be a member of the Privy Council; and a British Barnum may pass over a threshold which is denied to a Darwin, a Stokes, or a Huxley. Our own impression is that this treatment of men of culture does not depend upon the personal feelings of the noble woman who is now our Queen. We believe that it simply results from the ignorance of those by whom Her Majesty is, by an unfortunate necessity, for the most part surrounded. The courtier class in England is—and it is more its misfortune than its fault—interested in few of those things upon which the greatness of a nation really depends. Literary culture some of them may have obtained at the Universities, but of science or of art, to say nothing of applied science and applied art, they for the most part know nothing; and to bring the real leaders of England between themselves and the Queen's Majesty would be to commit a *bêtise* for which they would never be forgiven in their favourite *coteries*. No subject—still less a courtier—should be compelled to demonstrate his own insignificance. That this is the real cause of the present condition of things, which is giving rise to so many comments that we can no longer neglect them is, we think, further evidenced by the arrangements that have been made for the Jubilee ceremonial in Westminster Abbey. The Lord Chamberlain and his staff, who are responsible for these arrangements, have, we are informed, invited only one Fellow of the Royal Society, as such, to be present in the Abbey; while with regard to literature we believe not even this single exception has been made. It may be an excellent thing for men of science like Prof. Huxley, Prof. Adams, and Dr. Joule, and such a man of literature as Mr. Robert Browning, that they should not be required to attend at such a ceremonial, but it is bad for the ceremonial. The same system has been applied to the Government officials themselves. Thus, the Department responsible for Science and Art has, we believe, received four tickets, while thirty-five have, according to Mr. Plunket's statement in the House on Tuesday, been distributed among the lower clerks in the House of Commons. Her Gracious Majesty suffers when a ceremonial is rendered not only ridiculous but contemptible by such maladministration. England is not represented, but only England's paid officials and nobodies.

While we regret that there should be these notes of discord in the present condition of affairs, there can be no question that Her Majesty may be perfectly assured that the most cultured of her subjects are among the most loyal to her personally, and that they join with their fellow-subjects in many lands in hoping that Her Majesty may be long spared to reign over the magnificent Empire on which the sun never sets, and the members of which Science in the future will link closer together than she has been able to do in the past.

IMPERIAL GEOLOGICAL UNION.

NO one interested in geological science could fail to be impressed with the evidence afforded by the Colonial and Indian Exhibition, in its display of natural products, in the conferences connected with it, and in the number of scientific men collected from all parts of the Empire, of the amount of geological work represented by Great Britain and its dependencies, and the commanding position of the Empire with reference to the geology of the world. The same fact was apparent in the importance attached to Colonial and Indian geology and geography at the meeting of the British Association at Birmingham. Influenced by these facts, I was induced to speak somewhat strongly in the address which I had the honour of delivering at Birmingham on the position of Britain and its colonies and the English-speaking world in general with reference to scientific progress. On my return to Canada, and more particularly after the (temporary, as I hope) failure of the project to hold a meeting of the British Association next year in Australia, it seemed desirable to give the matter some definite form; and after correspondence and consultation with friends, I was induced, in February last, to address a letter on the subject to Prof. Stokes, the President of the Royal Society. The reasons for this course were that both Prof. Huxley and his successor in the Presidential chair of the Royal Society had suggested an Imperial Scientific Union, and the subject was understood to be under the consideration of the Council of the Society, which from its central and commanding position has a right to the initiative in any movement of this nature. In this letter geological science is alone directly referred to, as being that with which the writer is more immediately connected and that which in some respects has already the best organization; but without excluding other departments of science. Special reference is also made to Canada, as affording an apt illustration of the extent and value of the geological domain of the Empire. I need scarcely add that the present year, distinguished as it is by many movements in the direction of Imperial Union, in connexion with its being the fiftieth year of the reign of Her Gracious Majesty Queen Victoria, seems especially auspicious for such a project. The following are extracts from the letter referred to :—

"It is, I think, evident from the report of the last meeting of the International Congress of Geologists, that great, if not insuperable, difficulties lie in the way of any general agreement as to geological classification, nomenclature, and mapping. These difficulties, however, depend so largely on difference of language and of habits of thought, that they would not affect a union for scientific purposes on the part of the geologists of the British Empire, and ultimately of all English-speaking countries. It therefore appears that such a more limited union might with advantage be undertaken in the first instance, and with the view not of obstructing but of aiding the wider movement.

"The British Empire also possesses exceptional facilities for taking the lead of other nations in so far as geology and physical geography are concerned. The British Islands, as is well known, are remarkable for the great variety of their formations and the excellence of their exposures, and much of the present classification and methods of representation in geology has originated in Great Britain, and has been adopted with slight variation in all English-speaking countries, and to a considerable extent in other countries as well. In Canada we have the larger half of North America, and much of this very satisfactorily explored. We have also the advantages of the best exposures of the older crystalline rocks, of a development of the Palæozoic series in the Eastern Provinces, more closely allied to that of Europe than to that of the interior American plateau, and of Pleistocene deposits so extensive and complete that they must ultimately decide many of those questions of glacial geology which have been so much agitated. In India, Australasia, and South Africa, with the western districts of Canada and various smaller dependencies, we hold a controlling influence in the geology of the great Pacific and Indian Ocean areas. Arctic and Antarctic geology and modern oceanic deposits have been worked principally by English observers, and English-speaking geologists have been and are exploring in many countries not under the British flag. More especially the large amount of geological work done in the United States is based on English methods, and is published and discussed in the English language, and the most intimate and friendly relations subsist between the geologists of the United States and those of Great Britain and the colonies.

"In these circumstances it would seem that a union of British and English-speaking geologists might overcome the difficulties which appear so formidable as between the different European nations, and might lay a broad foundation of geological fact, classification, nomenclature, and representation, which would ultimately be adopted by other countries as far as local diversities and differences of language might permit. Such a geological union would naturally be accompanied or followed by similar co-operation in other departments of investigation in natural science.

"It seems probable that the Geological Survey of Great Britain and the Geological Surveys of the Colonies and of India, with the British Association and the Geological Societies and geological sections of Societies in all parts of the Empire, would be willing to co-operate in such a movement under the auspices of the Royal Society, and that the Council might usefully invite communications on the subject from public departments and Societies, beginning with those of the mother country and its colonies and dependencies, but looking ultimately to union with those of the United States also.

"In the meantime, I propose to mention the subject to the Council of the British Association, to the English and American Committees of the International Congress of Geologists, and to the Council of the Royal Society of Canada, and shall be glad to have your permission to regard this communication as an open letter to be used in any way likely to promote the object in view."

Copies of the above letter were sent to representative men in every part of the Empire, and a large number of replies have been received, expressing an interest in the proposal and readiness to aid in carrying it out. In so far as Canada is concerned, Lord Lorne, the founder of the Royal Society of Canada, and his successor as Patron of that Society, Lord Lansdowne, have signified their hearty concurrence, and the Council of the Society appointed a Committee on the subject, consisting of Dr. Selwyn, F.R.S., Rev. Prof. Laflamme, and the writer, whose report was adopted at the recent meeting of the Society in Ottawa. The following are the conclusions and recommendations of this report:—

"(1) That the objects referred to seem of the greatest importance to the advancement of geological science, and deserve the consideration of this Society, and more especially of its Geological Section.

"(2) That the present year, when all the subjects of the British Empire are united in a common desire to celebrate the fiftieth year of the reign of Her Most Gracious Majesty, when the public mind is impressed with the recent gathering of the resources of the Empire in the Colonial and Indian Exhibition, when plans for Imperial Federation are before the public, and when a Conference of delegates from the colonies, for the purpose of promoting a more intimate connexion, is being held in London, appears eminently favourable to the realization of the idea of an Imperial Geological Union.

"(3) It would appear that the first steps towards such union should be taken by scientific bodies in London, and that the Royal Society of London should be requested to begin the movement by inviting in the first instance to a Conference, representatives of the Geological Survey of Great Britain and of the various Societies and Associations in Great Britain and Ireland prosecuting geological work, with representatives from similar bodies in the colonies. Such Conference might define the objects to be attained, might prepare a constitution and arrange for subsequent meetings and for reports to be sent in on important questions.

"(4) It appears to your Committee that when thus organized, the work of the 'Imperial Geological Union' might be carried on by local and general conferences and conventions; by regular reports from local branches for publication annually by the Officers or Council of the Union; by correspondence and conference with geological bodies abroad, and possibly by other methods which would develop themselves.

"(5) In so far as Canada is concerned, this work might be aided by the Geological Survey of the Dominion, by this Society and the Societies affiliated with it, and possibly also by the Universities.

"(6) The Director of the Geological Survey of the Dominion has intimated his willingness to co-operate in sending representatives of the Survey to any conference or convention, and also by furnishing information as to the work and methods of the Survey.

"(7) It appears to your Committee that this Society might co-operate by empowering the Council to continue its Committee and to select delegates to represent the Society in the event of a preliminary conference being called in London, and by inviting all the affiliated Societies which prosecute geological work in the Dominion to take similar action.

"Your Committee would therefore recommend that this report, with the letter appended, be printed and circulated to the different local Societies connected with this Society, and to such other bodies as may be interested in the matter, and that their aid and countenance be solicited in carrying out the scheme, and that the Society empower the Council, or a committee appointed for the purpose, to represent the views of the Society by correspondence, or by attending any conference on the subject which may be summoned. It will, however, be understood that no expense shall be incurred without consent of the Council of the Society.

"It appears to your Committee that while the usual language of the Union would necessarily be English, communications should be received in any language used within the Empire, and that in this Dominion the English and French languages would be recognized as in this Society."

It will be seen that we hope the initiative will be taken by the Royal Society, and the present communication is intended to aid in securing that general co-operation throughout the Empire which is essential to success. With the same object I have asked the Council of the British Association to throw its influence on the side of union; and propose, in resigning the office with which the Association has honoured me, to make it a personal request that this great Society, which, by its meeting in Canada and its proposed meeting in Australia, has assumed an Imperial character, will take a leading part in the promotion of Imperial union both in reference to geology and to other sciences.

I need scarcely add that the project is not intended to interfere with the operations of the International Congress of Geologists, which is to meet in London in 1888; but it would appear eminently desirable that the contemplated Imperial Geological Union should be organized before that meeting, so as to enable British geology to present a united front, and to assume the importance to which it is entitled. J. WM. DAWSON.

SOCIAL HISTORY OF THE RACES OF MANKIND.

Social History of the Races of Mankind. Second Division: "Papuo and Malayo Melanesians." By A. Featherman. (London: Trübner, 1887.)

MR. FEATHERMAN does not improve. Those who have read the severe criticisms evoked by previous volumes, and still more those who have read the volumes themselves, will understand how much is implied in these few words, which could be justified only by a stern sense of duty, and regard for the interests of scientific truth. But, as the huge work grows under his hands, it becomes more and more evident that he has undertaken a task entirely beyond his strength. The present volume brings especially into painful evidence the inherent defects of his method, his inadequate grasp of the subject-matter, and his many shortcomings betrayed at every step in the treatment of details.

And first as to the method. A "social history of the races of mankind," which, as he is careful to tell us, eschews both anthropology and ethnology "in the technical sense of these words," necessarily resolves itself into a history of social progress, such as, for instance, is presented in Mr. E. B. Tylor's "Primitive Culture," or his "Researches into the Early History of Mankind." But Mr. Featherman's work is in no sense a "history," that is, a systematic and orderly treatise on the various phases through which mankind has passed, or is passing, in its upward development from the crude beginnings to the highest aspects of human culture. Any such broad and philosophic exposition of the subject is at once excluded by his method, which consists of a disconnected and more or less accurate account of the habits and customs, social usages, language, religion, and tribal or national organization of the various races and their subdivisions, classified according to a system peculiar to the author. Here we have an interminable series of minute ethnographic pictures, involving endless repetitions, without unity, without point, without those comprehensive generalizations which are essential to give coherence to the whole, and which would flow of themselves from a systematic treatment. These *disjecta membra* may to some extent supply the raw material, but they never can "be considered as a manual of sociology," as is claimed for them by the author.

But owing largely to his inadequate grasp of the subject-matter, this raw material itself is often of a highly unsatisfactory nature, and is so arranged as to be almost worthless to the ordinary student, or in fact to any except those few anthropologists who have the leisure and knowledge needed to re-arrange it for themselves. When Mr. Featherman passed from the "Nigritians" (African Negroes) of the first to the "Melanesians" of this second division, he was at once confronted by one of the most tremendous difficulties in the whole range of anthropology; but of that difficulty, turning upon some rational or at least working classification of the Oceanic peoples, he seems to be absolutely unconscious. Hence in his grouping of these peoples he has fallen into an abyss out of which there is no redemption. It is all very well for him to protest that "it is not the object of this work to discuss contested ethnological questions"; but he himself feels the necessity of some kind of grouping, in establishing which he is fain to discuss some very abstruse questions touching the origin of mankind, the nature of species, the value of language as a racial test, and the like. In general he professes to base his classifications "principally upon physical characteristics and language" ("Nigritians," p. xv.), and this leads him to a classification in the present volume, which confounds the yellow and dark races, which identifies the Malays with the Papuans, which ignores the presence not only of the fair Indonesians, but of the pygmy Negritoes in the Eastern Archipelago, and which, as shown on the very title-page, recognizes in that region, and in fact in the whole of Australasia, eastwards to Fiji, one stock only—the "Melanesian." Of this stock there are two groups, the "Papuo-Melanesians," and the "Malayo-Melanesians," which is like saying the "Black-Blacks" and the "Yellow-Blacks," the latter comprising the Malay race in its widest sense, the former all the rest—that is, the Melanesians proper of Melanesia, the Papuans of New Guinea and neighbouring islands, the natives of New Britain and New Ireland, the Negritoes of the Philippines, of the Malay Peninsula, and Andaman, the Nicobarese, the Australians and Tasmanians. Certainly the Negritoes are nowhere mentioned by name, being ignored as such; but they are nevertheless described as Papuans or Melanesians under other names, such as Ayetas (in the Philippines), Semangs (in the Malay Peninsula), and Mincopies (in the Andaman Islands). On the last-mentioned he quotes somewhat disparagingly (p. 227) Mr. E. H. Man, to whom we are indebted for the very best memoir on this race. Yet even from him he might have learnt that the Andamanese "are Negritoes, *not* Papuans" (Journal of the Anthropological Institute, August 1882, p. 70), just as from the photographs taken by Mikluho-Maclay, and Dr. A. B. Meyer, whom he does not quote, he might have seen how profoundly the Negritoes of the Malay Peninsula and the Philippines differ from the Melanesians. This term Melanesian, which here receives such a prodigious extension, is nowhere very clearly defined, and from its free application to the yellow Malays, one is tempted to ask whether Mr. Featherman is aware that it is Greek for "black."

Of these Malays, again, it is dogmatically asserted (p. 420) that they "did not originate in Asia," although nearly all anthropologists regard them as true Asiatics, a branch of the Mongolic stock, who migrated southwards to the Archipelago while it possibly still formed part of the mainland. But Mr. Featherman has a curious theory about migrations, denying, in fact, "that either animals or plants ever migrate." Hence, for him, the Malays cannot be a branch of the Mongolic race, which they closely resemble, but must be "an island people," a branch of the Melanesians, whom they do not resemble at all. With the Melanesians they constitute one of his six stock races, which, although "zoologically varieties of the same species," nevertheless originated in six different centres, and are consequently not genetically connected. This inference he doubtless seems to repudiate in the present volume (p. viii.). But it is clearly and unequivocally stated in the passage in the previous volume, which he omits to quote in his reply to the critics who had, as he now says, "erroneously if not purposely" affirmed this of him. The omitted words run thus: "The peculiar physical characteristics and the *habitats* of the existing races tend to show that they sprang from distinct individual pairs, developed under a variety of surrounding conditions in different parts of the world" ("Nigritians," xxii.). In fact, the assumption is that like conditions inevitably produce like results, that "the same causes must necessarily produce the same effects under any given circumstances," hence that "plants and animals must have been produced and evolved not by a single pair, but by an indefinite number of pairs in different parts of the world" (xiv.). It follows that crocodiles, for instance, have not migrated, but have been independently evolved under like surroundings in the Old and New Worlds; and so with the "six" human types, "zoologically varieties of the same species," but nevertheless independently evolved (from what lower types it is not stated) in different centres. So it is argued that "the Darwinian theory of transformism" is entirely wrong, and is caricatured by being compared to the Australian theory of the evolution of man from the lizard, which Mooramoora enabled to walk erect by striking off its tail (p. 181). "A weighty argument against the sweeping transmutation theory of Mr. Darwin" is elsewhere drawn from the Australian quadrupeds, hardly any of which are found in any other country, and it is triumphantly asked, "Why did they not advance beyond the marsupial type?" (p. 112). Mr. Featherman evidently still thinks that under given conditions the marsupial should "advance" to a higher or placental type, unaware that Marsh has shown that there is no such evolution, but that the marsupial and placental mammals descend in independent lines from a common undifferentiated prototype (*American Journal of Science* for April 1887).

We come, lastly, to Mr. Featherman's many shortcomings in the treatment of details, of which it may be said, without any exaggeration, "*Che formicolan d'errori.*" Blunders and unaccountable inaccuracies in geology, history, geography, zoology, ethnology, seem to accumulate at almost geometrical ratio with each succeeding volume. But, as a previous critic is here said (p. xviii.) to have charged him with making mistakes of this sort without pointing them out, it will be only fair to specify at least a few of the more glaring errors occurring at almost every other page of the present volume. In the very first sentence of page 1, Borneo is connected geologically and biologically with the Australian instead of the Asiatic world, to which Wallace and others have conclusively shown that it undoubtedly belongs. A little further on "Borneo, Sumatra, Java, Celebes, and the smaller islands of the Archipelago," are said to have "formed a continuous peninsular dependence of Papua" (p. 1), with which the three first were certainly, and Celebes most probably, never connected. But this re-grouping of the Eastern Archipelago, and the removal of the Indo-Malayan to the Austro-Malayan region was necessary for the author's peculiar views regarding migrations and the "Melanesians," so the results of Wallace's labours in this field are quietly shelved. The members of the animal kingdom are shifted about in the same reckless way, and apparently for no purpose at all, unless it be to show the author's incompetency for the work he has undertaken. Thus the hippopotamus is transferred from Africa to Sumatra (p. 286); birds of paradise from New Guinea to the same region (p. 286), and to Borneo (p. 3); the babirusa from Celebes to "the islands nearest to Malacca" (p. 2), and even to New Guinea (p. 9), where it is described as the *Sus papuensis!* the gazelle from Africa and South-Western Asia to the Eastern Archipelago generally (p. 2); the emu from Australia to Java (p. 361); the orang-utan from Borneo to "Malacca" (p. x.); humming-birds from America to the Philippines (p. 469), after which long flight it was at least courageous to deny that "animals ever migrate" (p. ix.). Topography and geography fare no better, for we have Quettah transported from Baluchistan to the Malay Peninsula (p. 13). On the same page the Ayetas are said to be "found more especially on Alabat Island, where they inhabit the coast as well as the mountain regions." Think of "mountain regions" in Sheppey, for instance, at Thames mouth, for that is about the size of the islet of Alabat, on the east coast of Luzon! And think of this rock being the chief home of the Ayetas, who are scattered over tens of thousands of square miles in the Philippine Archipelago! Is Mr. Featherman poking fun at his readers, when he writes such stuff as this; or is it that he has not the remotest idea of the significance of the terms which he blindly copies from his mostly antiquated authorities? The latter alternative seems forced upon us, when we again read that the islet of Amboyna and the Sulus "are distinguished for their alluvial lands, their navigable rivers, &c." (2). Then the large Solomon Archipelago is reduced to "Solomon's Island" (11), while, by way of compensation, Palawan develops into "the Palaonans" (p. 491). Australia is divided at p. 114, into "five provincial States," which, however, are further on reduced to "four provinces" (p. 181), Queensland being here forgotten. So with the population of Fiji, given correctly at p. 183, and wrongly at p. 187; and of the Philippines, nearly right at p. 470, but entirely wrong (4,290,000) at p. 480; the laborious compiler, with no information of his own, being thus everywhere at the mercy of the authority he happens at the moment to be quoting. A glaring instance is his treatment of the Malay Peninsula and its inhabitants, for which he appears to have seen nothing more recent than Favre's "Wild Tribes" (1852), and an early edition of Wallace, quoting, however, Rosenberg's "Malayische Archipel" (1879), which has nothing at all about the peninsula. The result is ludicrous, the area of this region being given at "about 45,000 square miles" (p. 420), instead of 75,000, and the population at 374,266 instead of 1,200,000. Here, also, "the chief rivers" are said to be "the Lingie, the Malacca, and the Cassang" (p. 419), and the mountains —but without wearying the reader it will suffice to say that the mountains are worse than the rivers. Similar wild statements are made about the Malay language (p. 300) about the population of Java (p. 362), the Javanese language (p. 376), the "Kanaks" of New Caledonia (p. 77), and, to make an end of it, about the Bughis of Celebes, of whom we are gravely informed that their "commercial activity is extremely limited" (p. 447), these Bughis being far and away the most enterprising and commercial people in the whole Eastern Archipelago.

One word in conclusion. If Mr. Featherman sees good to continue this wearisome compilation on the old lines, let him at all events abstain from sneering at specialists like Mr. Man (p. 232, 235), Mr. Taplin (not *Tarplin*, p. 135), Messrs. Fisson and Howitt (p. 141), and others who have done such admirable ethnological work in this Oceanic domain. But above all let him respect the august name of Charles Darwin (pp. 112, 181).

A. H. KEANE.

THE FAUNA OF LIVERPOOL BAY.

First Report on the Fauna of Liverpool Bay and the Neighbouring Seas. By Members of the Liverpoo Marine Biology Committee, edited by W. A. Herdman, D.Sc., F.R.S., Professor of Natural History in University College, Liverpool. (London: Longmans, Green and Co., 1886.)

IN this volume are published the results of investigations carried on by a Committee of Naturalists belonging to Liverpool and its neighbourhood. The inquiries were suggested by Prof. Herdman, and his energy and influence have evidently contributed largely to the success of the work. It is intended that the Committee shall endeavour to found a sea-side laboratory and form a permanent organisation for marine biological research, but its first operations in the summer of 1885 were limited to expeditions for obtaining invertebrate specimens, by dredging, trawling, and tow-netting from steam-tugs, and collecting on the shore at low-tide.

The volume consists of a number of reports by the members of the Committee and other naturalists on separate portions of the collections made. The greater number of these reports are lists of species, with a record of the places where each occurred; one or two of the papers deal with matters of more general scientific importance. Prof. Herdman himself identified the Alcyonaria, the Echinodermata, the Nudibranchiata, and the Tunicata, and also is jointly with two other gentlemen responsible for the Hydrozoa. Mr. Hoyle records the Cephalopoda. The experience of these naturalists is a sufficient security for the correctness of their work. In the list of Vermes given by J. A. Harvey Gibson, there are one or two errors which lessen its value. *Cirratulus borealis*, Lamarck, and *C. cirratus*, O. F. Müller, are set down as separate species, and it is stated that the latter, of which a single specimen was dredged, has not previously been recorded from the locality. The two names are synonyms, and to what species the single specimen "in a rather mutilated condition" belonged remains an open question. *Nephthys hembergii*, And. and M. Edw., is given as a synonmyn of *N. longisetosa*, Oersted, but the two names undoubtedly refer to distinct species, and it follows that the specimens of Nephthys examined were not accurately discriminated.

Mr. Harvey Gibson contributes another paper on the structure of some of the Polychæta, in which he gives some interesting notes on certain anatomical points, and gives reasons for concluding that *Pectinaria belgica*, Pallas, and *P. auricoma*, Müller, are synonyms. A short paper by Prof. Herdman, on variation in the Tunicata, discusses the value of different characters in these animals as diagnostic marks, and points out the necessity of thorough anatomical examination in describing species, or even identifying individuals. A species of Sycandra which could not be identified with any already known, and which is therefore probably new, is described by Mr. Harvey Gibson under the name *S. aspera*.

Three introductory papers precede the more special part of the book: one in which Prof. Herdman gives a history of the origin and work of the Liverpool Marine Biology Committee; one by the Rev. H. H. Higgins, containing a review of previous work in the domain to which the volume refers; and one by Prof. Milnes Marshall on shallow-water faunas. In this last a short but interesting comparison is made between the peculiarities of the physical conditions of the littoral region and features commonly occurring in the life-cycle of its inhabitants. Prof. Herdman, in summing up the results of the first year's work of the Committee, gives the following figures:—913 species of invertebrates have now been recorded from the district under examination, of which 235 are new finds made by the Committee: 16 of these are new to the record of the British marine fauna, and 7 species and 3 varieties are new to science. These additions to zoological knowledge are illustrated by ten lithographic plates, which, with the exception of Plate II., containing coloured figures of Anthozoa, and Plate IV., devoted to small crustacean forms, do not attain a very high standard. There are also two maps showing the district explored.

OUR BOOK SHELF.

Oberpliocän-Flora aus den Baugruben des Klärbeckens bei Niederrad und Schleuse bei Höchst a M. T. Geyler und F. Kinkelin. (Frankfort, 1887.)

As a general rule, the more recent the fossil flora the more satisfactory the determinations of the plants comprised in it will appear, though the work of Williamson and others has made an exception of those of the Carboniferous period. In the late Tertiaries the species are so closely allied to those still living that comparisons are relatively easy; but as we go back in time they diverge more and more, and there is less to guide us. The Pliocene floras especially show us that innumerable species that are now exotic were indigenous probably down almost to glacial times, and their study sheds an immense light on the more problematical floras which preceded them.

This work describes a Pliocene flora recently discovered, and regarded as newer in age than those formerly described from the valley of the Maine. It deals chiefly with the fruits of well-known existing genera of north temperate regions. A remarkable exception is an Australian type of Callitris, *Frenelites*, which appears to be correctly determined. The pines are numerous, among them being *Pinus montana*, and two varieties which are raised to the rank of species—*P. cembra*, determined on part of a cone, *P. strobus* on a scale, and some perfect examples named *P. cortesii*, Ad. Brong. Other conifers are the larch, the silver fir, and the Norway spruce. The American swamp or deciduous cypress, so prevalent in Europe from the Eocene age onward, is represented by foliage. Among the rest are leaves and supposed seeds of the hornbeam, the cup of an acorn, an abundance of beech-nuts, described as *Fagus pliocænica*, and the horse-chestnut, representing the Old World; and fruits of *Liquidambar*, *Nyssa*, a walnut, *Juglans cinerea*, and another nearly allied to *J. nigra*, and three hickories, representing the New. The European and American forms thus appear about equal in number, and there is one Asiatic, the horse-chestnut, and one Australian form.

The data are more trustworthy than are ordinarily obtainable from fossil floras, and they bring into prominence one significant fact—namely, that whenever we get the oak, hazel, walnut, or chestnut in strata so recent as the Pliocene, or even as true Miocene, there is no uncertainty about the genera, for fruits and other organs besides leaves are present; but in the older Tertiaries no distinct fruits of the kind are ever associated with the leaves ascribed to these genera. The evidence I have personally collected in the field seems to show that the early Eocene and pre-Eocene Dicotyledons had small clustered fruits, like *Platanus*, *Alnus*, *Liquidambar*, &c.; that leguminous plants were an Eocene development; while the larger-seeded oak, beech, walnut, hazel, are of later origin. The reliance placed on the mere similarity in the outward appearance of leaves of common types has not been justified by later discoveries, and an immense amount of revision is requisite before the botanist and geologist can safely put his trust in the descriptions of the older Tertiary floras.

J. STARKIE GARDNER.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

British Association Sectional Procedure.

As the time for holding the Manchester meeting of the British Association approaches, it seems natural to inquire whether any action will be taken by the Council of the Association toward carrying out the important suggestions made in the columns of NATURE (vol. xxxiv. p. 495), by Prof. Oliver Lodge, immediately after the late meeting at Birmingham. I can vouch for the fact that many of the active workers in the Section meetings of the Association can heartily indorse the expression of discontent which fell from Prof. Lodge as to the inadequacy of the present arrangements. The effect of the attempt to shirk holding Section meetings on the Saturday, and on the succeeding Wednesday, has been to cause a most undesirable pressure upon the time available on other days, and has rendered serious and effective discussion of the subjects of the papers almost impossible. It is understood that at Manchester, thanks to the generous hospitality of the local leaders, the Association will be graced by the presence of an unusual number of foreign men of science, including some of the most distinguished of chemists, physicists, and biologists. This fact is in itself an additional reason for expecting earnest and lively discussions to arise in the Section meetings, —discussions such as add greatly to the interest of the meetings, and are of extreme value to those who are actual workers in science. It would indeed be cause for regret if the anticipated discussions were to be burked or spoiled by want of due attention to the arrangements of the meetings. The suggestions of Prof. Lodge are indeed so timely that I fear to weaken their force by adding to them or emphasizing any of them. Yet I cannot refrain from urging two points: one the extreme undesirability of scamping the Wednesday sitting; and the other the advisability of reconsidering the hours of holding Committee meetings. Why should not the Sectional Committees meet from 3 to 4 o'clock, and the Sections at 10.30? A clear half-hour would be gained; Committee-men might slip out for lunch instead of attempting to sit out a dwindling meeting in a famished state; and they would continue their attendance to the end of the Section meeting because of the Committee meeting at its close.

Further, much good would accrue if the Council would cause to be published from the first the days and hours appointed for the reading of papers or the holding of discussions on the various topics. Last year I succeeded in inducing the Sectional Secretaries to begin this practice, in spite of the cold water thrown upon my suggestion by more than one of the ancient lights of the Council. If the Council would only, as a matter of good business-like arrangement, issue instructions that this should be done in No. 1 of the Journals, the benefit would be double. As an instance I will only mention that many of the members of the Committee on Electrolysis, of which Prof. Lodge is Secretary, are looking forward to a full and interesting discussion of their report, in which discussion they especially anticipate that an important part will be taken by their distinguished Continental visitors. Knowing this to be the case, why cannot the Council fix beforehand a day and hour for this matter, which is to many of the physicists and chemists the most important event of the meeting, more important than the addresses of Presidents of Sections, more important than the set evening discourses, more important even than the address of the President himself?

SILVANUS P. THOMPSON.

20 Arundel Gardens, W., June 4.

The Recent Earthquakes in Mexico and Turkestan.

IN vol. xxxiv. p. 570 of NATURE you kindly allowed me to bring forward some facts in support of a view advanced by me and mentioned in your review of the "Catalogue of European Earthquakes" which appeared in your number of September 16, 1886 (vol. xxxiv. p. 465), that earthquake localities lie on or are connected by great circles representing main lines of fissuring and therefore coast-line directions. Since then I have observed and noted two or three other remarkable cases, but the earthquakes recently reported (May 30) from Mexico, and June 9 from Turkestan, are so interesting in this respect that I venture to ask you for permission to point out how a great circle connects them.

This great circle is a coast-line direction which I had laid down and called "Coast of Coromandel Great Circle." It passes through or near the following localities and points. Parting from the mouth of the Musi River on that coast it takes in the coast-line to Pulicat; traverses the Indian and Southern Oceans and South Polar region (passing not far from the South Magnetic Pole); traverses the South Pacific and cuts the coast of Mexico at Talipa; passes at Oaxaca (the province of the same name is named as having been affected by the recent earthquake), also between Puebla and Vera Cruz (also similarly affected); runs parallel to the west coast of the Mexican Gulf; traverses the United States, about 200 miles west of the boundary shown in Major Powell's map of the earthquake of August 31, 1886 (see NATURE, vol. xxxv. p. 31), and roughly parallel to it; cuts the west coast of Hudson's Bay at the mouth of the Nelson River; passes at about 1° east of the North Magnetic Pole; traverses the North Polar region; crosses Nova Zembla, and the promontory to the north of the Sea of Obi and Siberia; and passes at about fifty-two miles to the west of Vernoje.

It may be of interest to remark that a great circle representing the Riviera coast-line, so lately and so disastrously shaken, passes in a north-west and south-east direction about 4° to the north-east of this point, and as the Turkestan earthquake has evidently extended beyond Vernoje, the actual distance between this great circle and the district affected may be less than 4°. In any case it is an interesting relation, and all the more so as this Riviera great circle cut New Zealand in the vicinity of the earthquake district of June 10, 1886, itself antipodal to that of Andalusia of December 25, 1884.

J. P. O'REILLY.

Dublin, June 11.

The Late Earthquake on the Riviera, February 23, 1887.

HAVING been at Nice during the late earthquake, I was much interested in the accounts published in NATURE (vol. xxxv. pp. 419 and 442), from which I have drawn up the following table:—

Earthquake of February 23, 1887.

Local time.	Greenwich M.T.	Duration.	Distance, miles. (From	Time, minutes. Nice.)	Velocity miles per minute.
Nice—					
h. m. s.	h. m. s.				
(1) 5 59 0 a.m.	5 30 0	55 secs. 25 intense.			
(2) 6 10 0	5 41 0	5 ?			
(3) 8 30 0	8 1 0	10 ?			
(4) 11 15 0 p.m.	22 46 0				
Marseilles—					
(1) 5 55 0 a.m.	5 33 40	90 secs.	100	$3\frac{2}{3}$	30
(2) 6 5 0	5 43 40	15 ,,	100	$2\frac{2}{3}$	37
Turin—					
(1) 6 22 0	5 32 0		100	2	50
(2) 6 31 0	5 41 0		100	? simul	taneous.
(3) 8 53 0	8 3 0		100	2	50
Basle—					
(1) 6 4 7	5 34 5		270	4	67
Paris—					
(1) 5 45 0	5 35 30		420	$5\frac{1}{2}$	76
Greenwich—					
(1) 5 38 0	5 38 0	20 secs.	650	8	81
(2) 5 45 0	5 45 0		650	4 ?	? 160
6 0 0	6 0 0				
7 40 0	7 40 0				
7 50 0	7 50 0				

I am indebted to M. Perrotin, of the Nice Observatory, for some useful information. He tells me that "the first shock lasted *nearly one minute*; it began by being very slight at first, and then became *very intense*: this latter phase lasted from 20 to 30 seconds."

We were all accused of great exaggeration in our accounts of the earthquake (I put the first shock at half a minute), but this more than confirms our estimates. A gentleman from South America, accustomed to earthquakes every week, told us that it was "a pretty good shake up."

According to Lyell, Mallet, and others, we at Nice being on alluvial deposits—gravel, clay, &c.,—felt not only the original shock, but also the rebounds from the rocks on either side; this would account for the very violent shaking that we had. I have compared notes with dozens of people, and feel sure that it was quite different from the sort of shock they felt at Cannes, Monte Carlo, and other places on rock, even the east bay at Mentone (the west bay suffered more than Nice). What saved us from being knocked down was, I suppose, that the amplitude of the vibrations was small, probably only a few inches. In the Italian Riviera (Diano Marina, &c.) they must have been more severe. Most people think that one more shock of the same strength would have brought half the houses down. A railway carriage going at 60 miles an hour gives the best idea of what our rooms were like during the first shock; it was impossible to stand on the floor without holding on to something, like a landsman on board a ship in a storm.

It would appear from the times given in the table that the velocity of this earthquake was high: 76 miles a minute to Paris, and 81 to London—a curious case of velocity increasing with distance.

The second shock seems to have gone faster than the first. The ordinary rate of earthquake-shock velocity is (according to Prestwich's "Geology"):—

1857.	Neapolitan earthquake	9 miles per minute.
1843.	United States "	32 " "
1869.	Cachar (India) "	83 " "

The centre of the shock was somewhere in the Gulf of Genoa, near Savona. The second shock was slight. The third was strong, but short.

The noise before the first shock was very loud, like a large steam blast. There were more than half a dozen other shocks in the two following days, but they were slighter, and chiefly oscillatory; curiously enough, we did not mind them so much as the vibrations, though I believe they are much more dangerous, if severe.

The night before the earthquake some horses were nervous and refused food, and dogs howled, but I naturally supposed that it had something to do with the Carnival which was being celebrated at the time. J. E. H. PEYTON.

108 Marina, St. Leonards-on-Sea, May 19.

The Shadow of Adam's Peak.

I HAVE recently seen a paper, read before the Physical Society by the Hon. Ralph Abercromby, which apparently shatters an explanation proposed by me to the same Society of the phenomena of the shadow of Adam's Peak in Ceylon. Whilst not anxious to support my own theory, if one more consistent with the phenomena has been discovered, I venture to think that there are certain considerations which militate against the new theory, and render it incomplete; and, with your permission, I will enumerate them.

(1) Mr. Abercromby says that it is the intervention of near and moving mist which produces the apparent uprising of the shadow. Is it possible that such a simple explanation could have escaped the notice of the hundreds of observers who have witnessed the phenomenon, and returned with the impression that there was something inexplicable about the shadow? It is difficult to imagine observing and reasoning faculties so rudimentary as not to be able to observe that a shadow was on mist, and reason from that to an explanation of the apparent approach and uprising of the shadow.

(2) Mr. Abercromby's theory depends on the intervention of near and moving mist rising from the Maskeliya Valley. This valley stretches away behind the observer in a south-east direction as well as to the north-west, and mist rising from it would be quite as likely to intercept the sun's rays behind as to form a curtain in front for the shadow to be projected on, and it would be only on very rare occasions, such as Mr. Abercromby describes, that the mist would keep entirely to the north-west of the Peak. Why it should do so is not explained. Therefore the uprising of the shadow could only be seen on such very rare occasions.

(3) Mr. Abercromby says: "Our fortune was in the unsettled weather, which made the mist so coarse and close that the unequivocal bow left no doubt as to the true nature of the cause;" "the sky was covered with a confused mass of nearly every variety of cloud;" "below and around us cumulus and mist;" "a pale moon with an ill-defined *corona*;" "sometimes masses of mist coming up from the valley enveloped us with condensed vapour;" "driving condensed vapour was floating about, and a fragment of rainbow-tinted mist appeared near the top of the shadow." Under such conditions, what else could Mr. Abercromby have seen than what he describes, the shadow on the mist, a circular rainbow, spectral figures like those of the Brocken, the rising and falling of the shadow as the mist intervened or passed away? Instead of "fortunate" in his conditions, I think Mr. Abercromby was the very reverse. To be "fortunate" he should also have seen the shadow in a clear atmosphere, and noted the *absence* of any appearance of uprising. Mr. Whymper, in his famous descent of the Matterhorn after the accident, saw in the evening a fog-bow very similar to that described by Mr. Abercromby, and the presence of mist was noticed in that case. But I ask, and I am willing to rest my theory on the answer, Is not the phenomenon of the apparent uprising of the shadow, witnessed when no mist is visible, and the atmosphere to the north-west is clear? This furnishes a simple *crux* of the two theories; for any observer can notice whether mist is visible or not, and if not, whether there is any appearance of the uprising of the shadow or not. Until corrected by future observers, I maintain that the phenomenon is seen when there are not "around us cumulus and mist" and "masses of mist coming up from the valley;" in fact, when the air is so calm and clear that the coast-line can be traced at a distance of seventy miles or more. If I am proved to be correct in this opinion, the new theory has not advanced the explanation by a single step. My theory of total internal reflection depends on the difference of temperature between the air in the low country and on the Peak, which is most marked in clear calm weather, ice forming at such times on the Peak, while a fall of the thermometer to 70° F. in the low country is commented on as noteworthy by the newspapers. The conditions described by Mr. Abercromby render the idea of mirage absurd; but they also suggest, if the new theory be correct, the absurdity of there ever having been any mystery about the phenomena of the Shadow of the Peak.

May 30. R. ABBAY.

Upper Wind Currents near the Equator and the Diffusion of Krakatão Dust.

I REGRET that Mr. Abercromby, before writing his interesting and suggestive article under the above heading, had no opportunity of making himself acquainted with the conclusions arrived at by the Krakatão Committee regarding the rate at which the finest ejecta were carried round the world. The velocity he ascribes to the material, viz. 120 miles an hour, deduced apparently from the few observations he quotes, is quite 40 miles an hour in excess of that deduced from the numerous cases treated by Mr. Russell and myself. In one or two cases in the Indian Ocean the velocity does apparently approach to that given by Mr. Abercromby, but these are both exceptional and doubtful, since they were probably due to minor outbursts antecedent to that which gave rise to the grand stream which encircled the globe at an average pace of 80 miles an hour.

Mr. Abercromby has thus accidentally made the problem appear far more formidable than it really is. A constant velocity of 120 miles an hour right round the world, though not outrageous to anyone who reflects on the great mobility of the atmosphere at the height of 100,000 feet or more, certainly makes a considerable demand upon our powers of scientific imagination, while a velocity of only 80 miles an hour, even though constant over the entire equatorial belt, does not appear, at such a height, to be opposed to what is already known of the motions of the atmosphere at the far inferior elevation of the cirrus clouds.

The height of the stratum is certainly a factor which cannot be overlooked, for if we find the average velocity of the wind continually increase as we ascend to the cirrus, it is reasonable to conclude that it rises beyond this limit, and if so a constant wind of 80 miles an hour at an elevation of 100,000 feet (less than the height deduced by Verbeek for that reached by some of the ejecta) might theoretically co-exist with a trade wind of ordinary velocity at the earth's surface.

It is not so much with reference to the velocity as to the direction of the upper currents near the equator that Mr. Abercromby's itinerary observations are valuable, since they correspond both normally and exceptionally with what might be expected from the laws of aëro-dynamics. Theory is naturally perhaps, though still somewhat singularly, silent as to what is supposed to be the motion of the air in the upper regions of the belt bounded by 15° on either side of the equator. Ferrel's equations are not very satisfactory for this space, owing to the smallness of the term $2v\omega \sin\theta$ representing the deflecting force of terrestrial rotation (*ablenkungskraft*), and close to the equator fail altogether at the surface. That the wind there, however, still maintains its westward component under the normal conditions which accompany the north and south trades is plain both from Mr. Abercromby's and other observations. Higher up, owing to the absence of friction, the air tends to move in the "inertia curve" corresponding to its motion at the surface, whose radius of curvature is $\frac{v}{2\omega \sin\theta}$; and since near the equator $\sin\theta$ is very small this curve is very nearly a straight line parallel to the equator. Whatever therefore happens to the surface wind through local influences such as latitudinal shift of thermal equator or doldrums, or the establishment of a local heat maximum on a land surface causing a deflection of the normal trade wind into a local monsoon, ought not to interfere sensibly with *the general tendency of the upper air to stream from east to west for a considerable space on either side of the equator.* I may just remark, *en passant*, that the belt bounded by 15° N. and S. latitude embraces an area of more than one-quarter of the entire surface of the globe.

The apparent anomalies as well as rules exhibited by Mr. Abercromby are thus seen to be in complete accordance with the above principle. It is only when we get some distance away from the equator that the gradient towards the poles in the upper atmosphere becomes large enough to change the westward into an eastward motion. As the air slides down this slope the radius of the "inertia curve" becomes smaller, and it veers through S.E. and S. to S.W., the normal direction of the upper current at the boundaries of the trade zones.

That the barometer gradient at a height of 13,000 feet over the equator is very small either from or towards the poles may be gathered from the following extract from a table given by Dr. Sprung in his "Lehrbuch der Meteorologie" (Hamburg, 1885):—

	Height 13,123 feet. Mean pressure in inches.
Lat. 20° N.	18·504
10°	18·532
0	18·543
10° S.	18·547
20°	18·547

Above this height the gradient towards the poles would increase, but theoretically there might be no change in the direction of the wind near the equator.

June 3. E. DOUGLAS ARCHIBALD.

Mammaliferous Gravel at Elloughton, in the Humber Valley.

I WAS informed a short time ago that a large bone had been found in a gravel-pit near Brough, on the Humber, and went at once to examine the place. I found the "bone" to be a mammoth's tusk of large size, and learnt that other teeth and bones had not infrequently been exhumed in the pit. As this seems to be a new locality for mammalian remains, I think a short description of the deposit may be found useful.

The excavation was commenced about twelve months ago on the top of a small isolated hill known as Mill Hill, which rises out of the Humber Flat to a height of about 90 feet, close to the village of Elloughton, and since that time there has been a constant and steady removal of the material, so that a good section is now exposed. The hill forms an outlier of the Wold Range, from which it is separated by low ground nearly a mile in width, the north shore of the Humber lying about one mile to the south of it. It is composed of Oolitic rocks overlain by gravel. The section at present shown is as follows:—

A. Top-soil, &c.... ...	2½ feet.	A British burial found in this layer, on the west side of the pit.
B. Rough stony gravel, with sand...	about 9 feet.	Contains pebbles of flint, sandstone, red chalk, Oolitic limestone, and other local rocks, along with a few well-worn erratic pebbles of felstone, quartzite, &c.; also rolled lumps of clay and streaks of carbonaceous matter like decayed vegetation.
C. Yellow sand, with stony layers	about 5 feet.	The mammoth's tusk and other bones were found in this bed.
D. Hard gray clay, forming floor of pit		Of doubtful age, but probably belonging to the Estuarine Oolites.

In the rough gravel, B, there are some boulders of local rocks so large as to suggest the idea that floating ice has been the agent of their transportation, especially as it seems as though the blocks must either have been raised from a lower level, or floated over the depression intervening between this hill and the Oolitic exposures in the flanks of the adjacent Wolds.

The junction of B with C is very well marked, and there are signs of erosion, and unconformity between them; but as the whole of the beds are current-bedded and irregular, this line of separation may be of no importance. On the other hand, since fossils seem only to be found in the sand, C, this may be the remains of an older deposit which has been denuded during the deposition of the overlying unfossiliferous gravel, B, and this latter bed may be a continuation of similar rough unfossiliferous gravels seen on the lower ground to the westward.

If the clay exposed on the bottom of the pit really forms part of the Oolites, I see no means of determining the age of these gravels; but my impression is that at any rate they are not older than the oldest boulder-clay of Holderness, and are probably not later than the newest. At Hessle, six miles to the eastward, bones have been found in a chalky rubble underlying boulder-clay, which Prof. Phillips regarded as pre-Glacial. At Bielbecks, seven miles to the northward, similar remains were obtained in 1829 from a fresh-water deposit which I think was regarded as post-Glacial. It may be that these deposits will eventually prove all to be of one age.

The size and condition of the tusk were such that I do not think it can have been carried hither by water-currents alone. It has more probably either been dropped from the floating or living carcase of the animal or from a mass of floe-ice. Its length, as it lay exposed on the floor of the pit at the time of my visit, was 90 inches, but the workmen said they had broken up about two feet of the "thick end" before they were aware; and as the apex was also blunted and badly preserved, I think its length when first deposited cannot have fallen short of 10 feet. Its diameter was 6 inches at a distance of 10 inches from the apex; 7½ inches at 20 inches; 8 inches at 30; 8½ at 40, beyond which it did not seem perceptibly to thicken. It lay in a water-logged gravel, and was in a very friable state; and though I was enabled, through the kindness of Mr. H. Lyon, the owner of the pit, to strengthen the specimen with cement, it crumbled into small splinters when an attempt was made to remove it, and was irretrievably ruined. Its curvature was not great, and would lie within a breadth of about 20 inches.

The only other remains I have yet obtained from the pit are some portions of the teeth of the mammoth and a few irrecognizable fragments of bone.

In the top-soil on the west side of the pit a British burial has been cut through, wherein lay the bones of a human skeleton, together with a fragmentary vase with the characteristic ornamentation. G. W. LAMPLUGH.

Bridlington Quay, June 6.

Fall of Peculiar Hailstones in Kingston, Jamaica.

SHORTLY after midday on the 2nd inst. a thunderstorm visited this city; the rain began with the wind from the east, as is usual with our May seasons, but it speedily changed to the west, accompanied with much lightning and thunder. Immediately hailstones became mingled with the rain, attention being drawn to their advent by the sharpness with which they struck on the shingled roofs. The west door of the laboratory being open to the air, the hail came in freely, nearly covering the floor for more than 12 feet. The hailstones were of clear ice, inclosing a few bubbles of air, varying from mere points to bubbles of the size of a split pea. The shape of the stones

was singular. Suppose a shallow and very thick saucer to have a shallow cup, without a handle, inserted in it, and you will have a good idea of the form of the hailstones when unbroken. Many had more or less lost the "saucer" by violence, while some were entirely without it, presenting the appearance of a double convex lens with faces of different curvature.

By actual measurement the hailstones were found to vary from one-quarter to three-quarters of an inch in diameter, and from one-eighth to one-quarter of an inch in thickness at the thickest part. I observed that in very many of the larger stones the air-bubbles could move about, showing the interior to be still liquid; as melting proceeded the bottom of the "saucer" would suddenly give way and become concave. The storm lasted about 15 or 20 minutes, hail falling for the greater part of the time. The hail which fell on grass remained unmelted for ten or fifteen minutes after the rain ceased. The fall of hail was very local, none falling at my house a mile away. I am informed that hail last fell in Kingston in 1839.

JAMES JOHN BOWREY.

Government Laboratory, May 12.

Singular Nesting-place of Linnets

IT may be interesting to some of your readers to know of the recurrence of a strange freak on the part of a pair of linnets. Last year they selected, as the scene of their nest-building and other parental operations, the interior of a Maltese water-bottle, hung against a brick wall, at the back of the house of Capt. G. Wood, and in a sort of half yard, half garden. The bottle is of porous ware, 10 inches high, 7 inches wide at its broadest part, which is mid-way between the bottom of the neck and the base, and having an upright constricted neck 4 inches long and only $1\frac{1}{4}$ inches in diameter on the inside. In this singular receptacle the birds contentedly built, laid their eggs, and successfully reared their brood.

This year, strange to say, the same pair, or one identically like them, have returned to the old haunt, deftly repaired and slightly added to the old nest, laid their eggs, and now have a vigorous progeny of five or six unfledged youngsters.

How the birds came, in the first instance, to select such a shelter, seeing that they could only pass in or out with folded wings, and by a sort of dart, and that to enter the neck from within in this way must have been a task of considerable skill and no little difficulty, is a mystery; but that they should have retained such a happy memory of their first sojourn as to lead them to return to their old quarters, is more interesting still.

H. VIAN-WILLIAMS.

3 Waterloo Place, North Shields, June 2.

A Brilliant Meteor.

YESTERDAY I saw a very brilliant meteor with train, resembling a firework in shape, colour, and other features. It was coming from Ursa Major, and vanished midway between α Lyræ and δ Cygni. Motion very slow; 21h. 19m. mean Turin time.

Turin, June 11. F. PORRO.

ELECTRICITY AT OXFORD.

IT is with very great regret that we learn that the study of natural science in the University of Oxford received last week a blow which is all the more to be deplored in that it was, in part at all events, delivered by those from whom such an onslaught was least to have been expected. Professed hostility or indifference to the great scientific movement of the day, injudicious economy,—these are obstacles which promoters of that movement must be prepared to face, and will in the long run overcome. It is not, however, to be expected that progress will be made if each forward step is checked by those who have themselves enlisted on the side of science.

The cardinal point which the University had to decide was whether it should or should not provide itself with a laboratory for the development of the teaching of electricity. The Clarendon Laboratory was, we believe, the first building in this country which was planned and erected for the study of experimental physics alone. It was designed about twenty years ago by Prof. Clifton, and, if we except the provision made for electricity, nothing better or more complete is to be found within the four seas.

Rooms were, it is true, originally set apart as electrical laboratories. The rapid growth of the science would have sufficed to render them inadequate now, but we gather from a statement circulated by Prof. Clifton that other causes have combined to strengthen the case for an extension of the building.

Optics has been a favourite subject among students of physics at Oxford, and optical apparatus now occupies the space intended for electrical instruments. Thus it has come to pass that "the important branches of electricity and magnetism are," in the words of the Professor, "necessarily excluded from the practical course."

In consequence of this unsatisfactory condition of affairs, Prof. Clifton has for some years lectured almost exclusively on electricity, and has been compelled to discuss methods of manipulation and details as to instruments which are usually mastered in a laboratory. The Lee's Reader in Physics, Mr. R. Baynes, has also established a practical course on electrical measurements, in Christ Church. Although the work he has thus done is excellent, we believe that it is not contended that Christ Church is in a position to make a permanent provision for instruction in electricity on a scale adequate to the requirements of the University.

For some time past, therefore, the University has been urged to add a wing for electrical work to the Clarendon Laboratory.

The necessity of providing for other University requirements has caused a long delay, but at length the turn of physics seemed to have come. Plans prepared by Prof. Clifton were submitted to the Hebdomadal Council. Mr. Henry Wilde, F.R.S., generously promised a gas-engine, dynamos, and an electric lamp. The Delegates of the Museum (who superintend the laboratories of the University), the Curators of the Chest (who have charge of its financial affairs), approved the scheme. It was adopted by the Council, and nothing remained but for the graduates in Convocation assembled to give their assent.

At the last moment, however, unexpected opposition arose. Balliol and Trinity Colleges have for some years combined their provision for the teaching of natural science, and the President of Trinity, acting for these Colleges, issued a pamphlet hostile to the grant for the proposed new laboratory. This step was taken on two grounds, both of which appear to us mistaken.

In the statement above referred to, Prof. Clifton had mentioned as an advantage incidental to the erection of the new laboratory that he would be able to abandon the lecture course of electrical demonstrations, as the instruction given in them would be better provided for in laboratory work. He proposed to substitute a general course on physics, addressed not only to the comparatively few students who aim at high honours in that subject, but to the larger body who enter for the first or preliminary stage of the honour examination. To this the authorities of Trinity objected that instruction of this kind was already given by one of their lecturers.

Into the merits of a dispute on a question of organization of this kind it is difficult, and perhaps unnecessary, to enter. The main fact is that, even if Balliol and Trinity are right in claiming as against the University something like a monopoly of general lecture instruction in physics, they have enforced that claim by placing, for an indefinite period, the Oxford school of physics at a serious disadvantage. Not a penny of the grant asked for was to be expended on apparatus for elementary lectures. It was all required for a lodge, for expenses connected with Mr. Wilde's installation, and for an electrical laboratory such as other Universities have and the University of Oxford has not. The President of Trinity no doubt thought that he was striving to prevent the University incurring an unnecessary expense. Could not some of his scientific advisers have informed him that the questions as to whether an electrical laboratory should be built, and as to whether Prof. Clifton should spend the time which its erection would place at his disposal in delivering a particular course of lectures, are separate and distinct? Are there no Boards or Faculties in Oxford in which the arrangement of lectures can be discussed without the friends of progress obstructing progress in Convocation? Opposition to the extension of the Clarendon Laboratory (the necessity for which they did not deny), lest as a secondary result of that extension a course of lectures should be given, which would involve no cost to the University, but which they feared might infringe their own real or supposed rights, is not an attitude for which the combined Colleges can expect much sympathy.

The second point which was raised by the President of Trinity was dealt with in an equally unsatisfactory manner. In 1886, Trinity College built and opened the Millard Laboratory for instruction in theoretical and practical mechanics and engineering. The Laboratory contains a steam-engine and three dynamos. It is about to be further extended, and it is claimed that it contains all the apparatus required for technical work in electricity. The President recited these facts in his pamphlet, and then added: "But with the question of advanced work I must leave others, who have more knowledge, to deal."

We venture to think that before describing the Millard Laboratory in detail in a pamphlet opposed to the Clarendon Laboratory grant, it would have been well for the President to have obtained from experts such information as would have enabled him to make up his mind as to what the two laboratories had or had not in common.

If it is really intended to concentrate the teaching of electricity in Balliol and Trinity, and, while placing it in the hands of College lecturers, to prevent the University Professor of Physics from acquiring the facilities for teaching it properly himself, we can only say that a most mistaken policy has been adopted. Physics, on account of the cost of the apparatus required, is a subject in which centralization is desirable, and, considering the place which electricity now occupies among the physical sciences, it would be absurd to exclude it from the University Laboratory, and from the curriculum of the only teacher of physics whom the University herself appoints. To do them justice, the combined Colleges did not directly make any such proposal; but, if they did not mean to make it, why was the Millard Laboratory imported into the controversy? As far as we can judge from the description given of it by the President of Trinity, it is a technical laboratory which may develop into something analogous to that of Prof. Stuart at Cambridge. If so, it does not—and those connected with it ought to have known that it does not—occupy the gap which the new building was to fill. "Theoretical and practical mechanics and engineering," coupled with electrical technology, afford plenty of scope for the energies even of such active Colleges as Balliol and Trinity. It is a pity that, with all this zeal, they have yet to learn that pure science is an ally and not a rival, that a dynamo is useful in a physical laboratory in which no technology is taught, and that the way for a young institution like the Millard Laboratory to earn respect is to do good work, and not to signalize its appearance on the field of labour by preventing others from doing it.

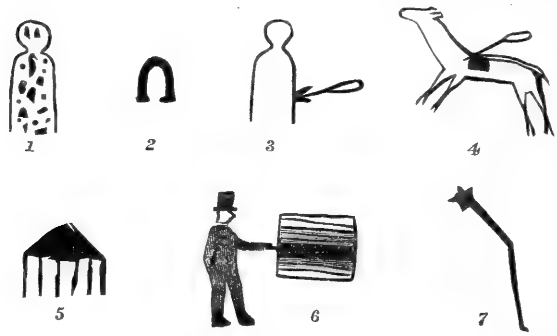
NORTH AMERICAN PICTOGRAPHS.[1]

THIS remarkable volume contains no fewer than 83 lithographed plates, and 209 separate woodcuts, and is an admirable compendium of the curious pictographs of the North American Indians. Large as it is, it professes to be only the forerunner of a still larger work that shall treat of pictographs generally. The author, Colonel Garrick Mallery, has already published an almost equally interesting memoir on "Gesture Language," in the first Annual Report of the Bureau of Ethnology.

One of the most striking features of the present work is the account it gives of the newly-discovered custom of the Sioux, or, more correctly speaking, of the Dakota Indians (for Sioux is some barbarism, repudiated by the natives), to keep national calendars. The custom is sufficiently ancient to have become generally established among this great branch of the Indians, but it is not old enough to have spread to the west of the Mississippi. One of the most important of the calendars begins with the year 1800; its historiographer was a man, still living in 1876, yclept "Lone-Dog." His calendar is painted on a buffalo hide, which appears to have been exhibited and explained to Indian audiences from time to time, and greatly admired by them, for four copies at least have been made from it (with variations of arrangement), and every intelligent adult Dakota knows its contents, and can read them in part. One of these copies is imitated in a beautiful plate, which is the most effective of all the many illustrations of this volume. The process of making the calendar is inferred to have been as follows:—During the dreary periods of their six winter months, certain elders of the tribe amused themselves with talking over the events of the past year, and Lone-Dog discussed with them which of those events should be selected by general suffrage as the representative of that period. Suppose it was an outbreak of the small-pox: then Lone-Dog drew the outline of Fig. 1 on one part of his buffalo robe, and dabbed it with red spots. Then that year became ever after known as the small-pox year, and the Dakotas would say so-and-so happened in the small-pox year, just as we should say in the year 1801. Or, again, the event might be that for the first time horses were seen by them that had been shod with iron: then the symbol of the year became a horse-shoe, Fig. 2. Lone-Dog's calendar is particularly graphic. Its earlier entries are probably derived from preceding chroniclers or from tradition; anyhow it covers the entire period from 1800 to 1871. The first entry is made in the middle of the robe, and the others are arranged year after year successively in an oblong spiral, the whole series being included in three turns and a half. They are drawn in black and red, the latter usually representing blood, of which plenty seems to be spilt in murder or in hunting. Thus Fig. 3 is a case of murder; Fig. 4 is a year in which a vast number of elk were killed, identified in the rude drawing by their cloven feet. Fig. 5 celebrates the erection of a trader's station; and Fig. 6 tells us that striped Spanish blankets were first introduced in that

[1] "Pictographs of the North American Indians." A Preliminary Paper by Garrick Mallery. Extract from the Fourth Annual Report of the Bureau of Ethnology. (Washington: Government Printing Office, 1886.)

year by a trader. Fig. 7 is the year of a great aërolite. Lone-Dog's system is not the only calendar. Others have recently been found by Dr. Corbussier which are drawn more elaborately, though not more intelligibly. The most important of these is only described, and not reproduced in this volume. It is by Battiste Good, and professes to date from prior to the year 1700. Being



1 2 3 4 5 6 7

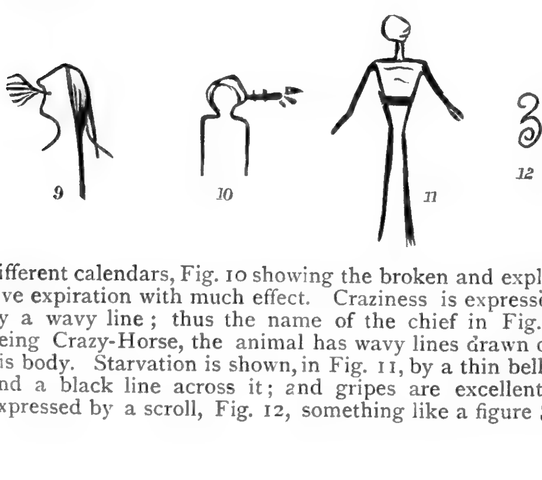
drawn in five colours, it would require much cost to imitate, and is withheld for the present.

Other curious features of the work are the pictorial censuses that it contains. One consists of eighty-four heads of families in the band of "Big-Road." Each is represented with the symbol expressive of his name attached to his head, after the manner of Fig. 8. Another

8

census is of the 289 adherents of Red-Cloud, who are for the most part portrayed on a similar principle.

Some of the more symbolic representations are amusing and instructive. Those for various diseases are as follow:—We have already seen the representation of small-pox, and that of measles is much the same. A whooping-cough year is typified by Figs. 9 and 10, in



9 10 11 12

different calendars, Fig. 10 showing the broken and explosive expiration with much effect. Craziness is expressed by a wavy line; thus the name of the chief in Fig. 8 being Crazy-Horse, the animal has wavy lines drawn on his body. Starvation is shown, in Fig. 11, by a thin belly, and a black line across it; and gripes are excellently expressed by a scroll, Fig. 12, something like a figure 3, only more twisted and tortured, scrawled over the abdomen.

The question is fully discussed whether this calendar-making was in any way due to the influence of the whites, but is decided on good grounds in the negative. The whole conception and the way of carrying it out seems to be thoroughly Indian, but it is not every Indian who is born with the historiographic capacities of Lone-Dog. The author testifies to the variety of individual aptitudes when speaking of the inhabitants of Queen Charlotte's Islands, who are beautifully tattooed. He says, "nor is it everyone who can tattoo. Certain ones, almost always men (let Mr. Romanes make a note of it), have a natural gift which enables them to excel in this kind of work."

The events that a group of persons are most apt to associate with an epoch during which they have lived together, are not necessarily the most important ones. They are those occurrences which are simple and well-defined, and have struck the fancy on that account, as well as from their unlikeness to former experiences. Such events are recalled with ease, and a partial or symbolic act of recollection suffices to identify them.

When pictorial nick-names are given to each successive year in council, as in the case of the Dakotas, the process must be very like that of giving verbal nick-names to new boys at school. This used to be a far more prevalent custom than it is now, and I have a vivid recollection of two old Etonians describing how it was done in their time. When the new boy made his appearance, he was of course well looked over and watched. Then, as the fancy took them, first one lad and then another addressed him with tentative nick-names. At the beginning, these trial names were apt to fall flat; at length one stuck; other lads adopted it, and usually in a few days the new boy was fitted with a generally-accepted name, by which he was afterwards known almost exclusively during the whole time he was at school. The appropriateness of the nick-name was by no means always obvious to strangers; it might even be due to some passing event with which by pure accident the boy was in some indirect way associated. I think this giving of nick-names is an excellent illustration of the manner in which many common words must have arisen.

The process of determining the most typical event of a year, and then of portraying it by a simple and bold design of a higher order of art than was known to Lone-Dog, and introducing no more detail than is necessary for identification, is well worth trying as an experiment. I tested it myself by attempting to construct such pictographs for the last few years of my life, under the condition that each should be included within a circle of the size of a shilling, and at last I succeeded fairly well, in my own, but probably too partial, judgment. I may add that, having done this, I laid a florin over my drawing, and traced a second circle round the florin. In the ring that lay between the two circles there was space for fully twenty-five bold capital letters, which I distributed among words that referred to other leading events of the year. The whole formed a by no means inartistic series of designs suitable for medallions.

I soon became so absorbed in my pictographs that I think others might interest themselves in the same way. It would be an amusing test of skill in a round game, to try who could make the most artistic and vigorous design within the compass of a circle traced, say, round a halfpenny—that is, of exactly one inch in diameter—to commemorate some recent event known to all. What a capital prize subject for art students it would be, to refer them to some brief register of events during the fifty years of Her Majesty's reign, and to ask for fifty such medallions, one for each year. Then, again, many persons carve in wood or paint on china, and want designs; let them take episodes in their own histories, and make friezes that should illustrate the events in families, schools, houses, &c., such as the stained-glass windows in churches do those in the history of the Bible. How prettily girls might design pictographs to record notable events in a pleasant tour, and interchange them with their fellow-travellers as presents. Such designs as these could be made subjects of embroidery, or, if on a larger scale, of that brass *repoussé* work which is, or was, so much in fashion. It would be by no means difficult to convert them into actual medallions. First the wax model, then the plaster cast, then the cast in white fusible metal, then the covering with an electrotyped coating of silver, just in the way that the ancient coins are reproduced at the British Museum, at the cost of about three shillings each, which are now so frequently used in rows for necklaces. What a delightful memorial of twenty-five years of wedded history might be given by a husband to his wife, in the form of a necklace of such medals. It would be a pleasant labour to make a set of designs, which an artist could afterwards put into better forms, and construct from them the wax medallions for the electrotypist to cast and turn into metal. I commend this idea of commemorative pictographs and *glyptographs* (as works in relief ought to be called) to the notice of amateur artists, whether they work in pencil, ink, colour, carving, embroidery, *repoussé* work, china painting, or in modelling.

This volume is an excellent example of the growing variety and wealth of material now available to inquirers into the origin of language. We meet in it with abundant evidence of the rapidity with which pictographs become abbreviated into conventional symbols, and are thereby adapted to play the same important part in reasoning that is usually played by words. I cannot see that it makes any fundamental difference in the use of symbols whether they appeal to the ear or to the eye, though I fully grant that on many grounds, not worth entering into here, the former is more generally convenient, and best suits the idiosyncracies of the majority of persons. The unassisted sense of touch, as we have learnt from the case of Laura Bridgeman, may afford an adequate basis for the exercise of a considerable amount of reasoning. And for aught I can see to the contrary, a dog who "ponders," to use a dog-trainer's expression, may occasionally be carrying out some real act of thought by the aid of imagined and symbolic odours.

FRANCIS GALTON.

COCOA-NUT PEARLS.

THE following letter has been sent to us by Dr. Sydney J. Hickson :—

"During my recent travels in North Celebes I was frequently asked by the Dutch planters, and others, if I had ever seen a 'cocoa-nut stone.' These stones are said to be very rarely found (1 in 2000 or more) in the perisperm of the cocoa-nut, and when found are kept by the natives as a charm against disease and evil spirits. This story of the cocoa-nut stone was so constantly told me, and in every case without any variation in its details, that I made every effort before leaving to obtain some specimens, and eventually succeeded in obtaining two.

"One of these is nearly a perfect sphere, 14 mm. in diameter, and the other, rather smaller in size, is irregularly pear-shaped. In both specimens the surface is worn nearly smooth by friction. The spherical one I have had cut into two halves, but I can find no concentric or other markings on the polished cut surfaces.

"Dr. Kimmins has kindly submitted one half to a careful chemical analysis, and finds that it consists of pure carbonate of lime without any trace of other salts or vegetable tissue.

"I should be very glad if any of your readers could inform me if there are any of these stones in any of the Museums, or if there is any evidence beyond mere hearsay for their existence in the perisperm of the cocoa-nut."

On this letter Mr. Thiselton Dyer, to whom we sent it, has been good enough to make the following remarks :—

Dr. Hickson's account of the calcareous concretions occasionally found in the central hollow (filled with fluid—the so-called "milk") of the endosperm of the seed of the cocoa-nut is extremely interesting. It appears to me a phenomenon of the same order as tabasheer, to which I recently drew attention in this journal.

The circumstances of the occurrence of these stones or "pearls" are in many respects parallel to those which attend the formation of tabasheer. In both cases, mineral matter in palpable masses is withdrawn from solution in considerable volumes of fluid contained in tolerably large cavities in living plants—and in both instances they are Monocotyledons.

In the case of the cocoa-nut pearls the material is calcium carbonate, and this is well known to concrete in a peculiar manner from solutions in which organic matter is also present.

In my note on tabasheer I referred to the reported occurrence of mineral concretions in the wood of various tropical Dicotyledonous trees. Tabasheer is too well known to be pooh-poohed; but some of my scientific friends expressed a polite incredulity as to the other cases. I learn, however, from Prof. Judd, F.R.S., that he has obtained a specimen of apatite found in cutting up a mass of teak-wood. The occurrence of this mineral under these circumstances has long been recorded; but I have never had the good fortune to see a specimen.

Returning to cocoa-nut pearls, I send you a note which the *Tropical Agriculturist* for April last quotes from the *Straits Times*:—

"A trade journal appearing in Java gives the following particulars regarding a peculiar kind of pearl found in this part of the world:—It is well known that pearls have been met with within oysters and mussels. Sometimes even trees yield pearls. In the Proceedings of the Boston Society of Natural History, there is a paper by Mr. J. Bacon regarding the kind of pearls often found within cocoa-nuts. The specimens shown have been bought at Singapore. They are said to be so rare in the East Indies as to be highly prized by the native rajahs, and worn by them as precious stones. Mr. Bacon himself possessed a small pearl of this sort. It is said that when allowed to grow, they will reach the size of cherries. This pearl resembles the common variety in smoothness, whiteness, and scant lustre of surface. It is harder than it, and almost as hard as feldspar or opal. The common pearl varies in hardness, but is never harder than feldspar. The cocoa-nut pearl consists of carbonate of lime, with very few organic substances remaining after treatment with acid solutions. This organic matter is insoluble, shows no trace of vegetable substances after microscopical examination, and seems to be akin to albumen in structure. In the common pearl there is also found an albuminous substance, but the latter remains unchanged in appearance and lustre even after the calcareous constituent parts have been dissolved away. In other respects microscopical research has brought out the fact that the cocoa-nut pearl is formed of concentric layers without any nucleus. The whole mass is made up of layers of fine crystalline fibres. Prof. Bleekrode, in commenting on the former in a Dutch scientific periodical, says that Rumphius, the famous botanist, had in his 'Herbarium Amboinense,' given full particulars of this petrifaction in the cocoa-nut. Rumphius has even illustrated his account of it by accompanying drawings of the two forms in which this kind of pearl is met with—pear-shaped and round, either of uniform appearance or with red edges. Hardly one in a thousand cocoa-nuts on the average displays this strange peculiarity. The formation of the latter is always a remarkable phenomenon, hard to account for, from the water in the nuts generally lacking the chemical substances favouring abnormal growth of the kind. Rumphius states for a fact that cocoa-nuts from Macassar yield more pearls than those from other places. This scientist, in 1682, sent, as a present to the Grand Duke of Tuscany, a ring in which a cocoa-nut pearl had been set. Similar pearl-like formations are met with in other East Indian fruits, such as the waringin, the pomegranate, and the kechubong."

To this may be conveniently added two brief extracts from the long and admirable account given by Rumphius:—

"*Calappites*, Belgis *Calappus-Steen*, Malaicensibus *Mestica Calappa*, albus est lapillus instar marmoris seu silicis albi, durus, planus, ac glaber, cujus putaveram alio loco inter lapides ac mineras descriptionem dedisse, quum vero in *Calappa nuce* inveniatur, ac sollicitus sim, opus illud a me forte non absolutum iri, animo induxi hic loci ejus exhibere descriptionem. Est itaque albus ac politus, seu glaber lapillus in interiore *Calappæ nucis* parte concrescens, nunc putamini fixus, nunc vero media in lympha natans, diversæ ac duplicis potissimum formæ" (Rumphius, "Herbarium Amboinense," vol. i. pp. 21, 22).

"Incolæ plurimum omnes *Mesticas* amant, quarum quasdam tanti æstimant, ut optimis etiam præferant gemmis; plurimas enim ipsis tribuunt immo sine dubio superstitiosas etiam virtutes, gestant enim has ad nudum corpus, in annulis, et armis, ad prosperum conatuum successum obtinendum. Elegantissimos ac rotundissimos hujus Calappi lapillos, seu *Calappites* imponunt annulis suis, vel etiam telis adpendent, non auro, sed argento circumdatos, dicentes melius hoc cum natura *Calappites* convenire" (p. 22).

If Dr. Hickson would present one of his pearls to the Kew Museum, it would, I am sure, interest a great many persons who would be glad to see an authentic specimen of so interesting a curiosity.

NOTES.

THE Annual Meeting of the Royal Society for the election of Fellows was held at the Society's rooms in Burlington House on Thursday last, when the following gentlemen were elected into the Society: John Young Buchanan, M.A., John Theodore Cash, M.D., Sir James Nicholas Douglass, M.I.C.E., Prof. James Alfred Ewing, B.Sc., Prof. George Forbes, M.A., William Richard Gowers, M.D., Prof. Alexander B. W. Kennedy, M.I.C.E., George King, M.B., Sir John Kirk, M.D., Prof. Oliver Joseph Lodge, D.Sc., Prof. John Milne, F.G.S., Rev. Octavius Pickard-Cambridge, M.A., George James Snelus, F.C.S., Thomas, Lord Walsingham, William Whitaker, B.A.

THE Council of the London Mathematical Society have awarded the second De Morgan medal to Prof. Sylvester, F.R.S., for his numerous and brilliant contributions to pure mathematics. The medal will be presented at the Council meeting in November next.

THE preparations for the forthcoming meeting of the British Association in Manchester are progressing very favourably. A strong Local Committee has been formed, and a guarantee fund of over £10,000 has been raised to meet the necessary local expenses. The reception-room will be in the recently-built Natural History Museum of the Owens College, and the Section rooms in the College or its immediate neighbourhood. A prominent feature of the meeting will be the presence of a large number of eminent foreign men of science, of whom more than a hundred have already accepted invitations to attend.

ARRANGEMENTS for the dinner to Prof. Tyndall are progressing satisfactorily under the direction of the Executive Committee consisting of Prof. Stokes (Chairman), Sir F. Abel, Sir W. Bowman, Sir F. Bramwell, Dr. Evans, Prof. Frankland, Dr. Hirst, Prof. Huxley, and Sir Henry Roscoe. Circulars announcing the dinner have been largely issued. It is, however, for obvious reasons impossible to send notices to all those who might wish to attend, and applications for tickets are daily made by gentlemen who have received no special notification of the event. There is no doubt that a body of scientific men will meet at the dinner such as has seldom or never been brought together on a similar occasion. Nor will the gathering be confined to scientific men alone. Among others, the following have also expressed their intention of being present: Lord Derby, Lord Lytton, Earl Bathurst, Sir F. Pollock, Sir F. Leighton, Lieut.-General Smyth, Prof. Bonamy Price, and Messrs. Leslie Stephen, W. Lecky, and Wemyss Reid.

THE Ladies' *Soirée* at the Royal Society, as we stated last week, was largely attended. Careful preparations had been made for it, and it was a great success. At intervals, in the Principal Library, a cornet solo was telephoned from Brighton. A large number of objects of great scientific interest were exhibited. Photographs of clouds, and photographs of the Firth of Forth Bridge, were shown with the lime-light; the former with demonstrations by the Hon. Ralph Abercromby, the latter with demonstrations by Mr. Baker. The microscopic structure of pearls was also shown with the lime-light, by Dr. George Harley. The Zoological Society of London exhibited a fine living specimen of the electric eel, from which shocks were taken. Prof. A. W. Rücker exhibited—(1) Colours of soap-films rotating under the influence of an air-current. A jet of air is directed on to the film so as to form a vortex, the colours of which change as the film becomes thinner. This experiment is due to Sir David Brewster. Attention has been recently called to it by Lord Rayleigh. (2) Artificial imitation of the colours of the setting sun. Light is passed through a glass cell containing a solution of sodium hyposulphite. If a little hydrochloric acid is added, the sulphur is deposited in fine particles which scatter the blue end of the spectrum. The transmitted light becomes redder, and colours like those of sunset are produced. This experiment is due to Capt. Abney. (3) Apparatus to illustrate the passage of light through lenses. An application on a large scale of the method of tracing the rays by passing them through air in a closed space charged with a small quantity of smoke. Chrysalides and living larvæ showing the influence of surroundings upon their colours were exhibited by Mr. E. B. Poulton; and Dr. E. Klein exhibited the following specimens of microbes under the microscope and in cultivation:—*Bacillus anthracis; Bacillus tuberculosis;* Bacillus of leprosy; Bacillus of swine fever; Bacillus of septicæmia; Bacillus found in typhoid fever; Spirillum found in Asiatic cholera; several other species of Spirilla; several species of *Bacterium termo;* Micrococcus of foot-and-mouth disease; Micrococcus of scarlet fever; Micrococcus of vaccine; different species of coloured microbes. Mr. Chichester A. Bell showed apparatus for reproducing audibly the vibrations of liquid jets. Vibratory motions of the orifice from which a liquid jet escapes, give rise to slight swellings, and constrictions of the liquid column. The swellings increase and the constrictions diminish as the jet travels downwards, finally causing it to break into drops. When the jet strikes upon a flat surface, the swellings are continued as waves in the thin sheet of liquid, which spreads out from the point of impact. The jet liquid being a conductor of electricity (acidulated water), and two platinum electrodes in circuit with a battery, and a telephone being immersed in the liquid sheet or nappe, the jet vibrations are reproduced as sound in the telephone.

A DEPUTATION consisting of Mr. Mundella, Mr. Joseph Chamberlain, Sir John Lubbock, Sir Henry Roscoe, Sir Lyon Playfair, Mr. John Morley, and others, will wait upon the Chancellor of the Exchequer on the 30th inst. to urge the claims of University Colleges upon the Government. Mr. Goschen has always taken so deep an interest in questions connected with education that he may be expected to consider carefully the arguments which will be submitted to him. It is almost certain that unless the University Colleges receive aid from the State some of them will have to be closed, for it has never been found that institutions of this kind can be maintained by fees alone. All that is asked is that the nation shall do for the University Colleges of England what is already done for like institutions in Wales, and for the Universities of Scotland.

THE foundation-stone of the new College of Science at Newcastle-upon-Tyne was laid yesterday by Sir William Armstrong.

THE Rev. J. E. Leefe has presented his botanical collections to Kew. Since the death of Borrer in 1862, Mr. Leefe has been universally recognized as the principal authority on British willows. In early life he lived in Essex and Yorkshire, but for the last generation he has held the living of Cresswell on the coast of Northumberland, opposite Morpeth. Here he got together a very fine collection of living willows, which had been obtained from Borrer, Darwall, the Duke of Bedford's collection at Woburn, and the Botanic Gardens of Kew and Cambridge. His sight having failed, he has retired to live at Coatham, near Redcar, and has given to Kew the collections he is no longer able to use. His principal publication was issued in 1842 under the title of "Salictum Britannicum." This contains ninety specimens, with printed tickets, and has been the recognized standard, ever since its publication, by which British willows have been named. His principal published papers are in the *Journal of Botany* for 1871—one entitled "An Arrangement of the British Willows," and another "On Hybridity in Salix, and the Growth of Willows from Seed." The collection he has now presented contains his own copy of the "Salictum," accompanied by a quarto manuscript; a valuable set of types from Hoch, the author of the classical "Flora Germanica"; a great many specimens dried from his living collection at Cresswell; and American types received from his correspondents in the United States. The general herbarium is of a miscellaneous character, principally British, but containing also a number of plants from Central Europe, Abyssinia, America, and other parts of the world. Amongst the British plants, the collection contains a curious *Ammophila*, gathered near Cresswell in 1872, with the glumes of ordinary *A. arenaria*, but with the decidedly tropical inflorescence of *A. baltica*, for which one of the only two known British stations is in Ross links, also on the Northumbrian coast, and which has by some botanists been regarded as a hybrid between *A. arenaria* and a *Calamagrostis*.

Apropos of our note last week on the invitations to the ceremony at Westminster Abbey, a correspondent writes to us from Dublin:—"We have the same state of things here. Neither the President of the Royal Irish Academy, nor the President of the Royal Dublin Society, as such, have got invitations. Invitations have been sent lavishly to Mayors, all of whom, save in three cases, have refused them, thus leaving, as one would have thought, a chance for science coming No. 2."

IN a despatch received at New York from Mexico on the 12th inst. it was announced that shocks of earthquake had been felt throughout Guerrero State on the 29th ult. and on the 1st and 2nd inst., and that several of the smaller towns had been injured.

SEVERE shocks of earthquake were felt last week in some parts of Turkestan. At Vernoje they began about 5 o'clock on the morning of the 9th inst. Almost all the buildings in the town were destroyed, and much life was lost. Great damage was also done at Kashelensk, Tsharkent, and other places. The telegraph line in the neighbourhood of Vernoje was broken down for a distance of about 200 versts.

THE last number of the *Annuaire de la Société Météorologique de France* (February, 1887) contains an interesting article by M. Hervé-Mangon on the distribution of rainfall and its duration in Paris, from observations taken during the years 1860-70. These observations, which were made with Hervé-Mangon's pluvioscope, show that rain falls on an average 19 hours a month. The month with the shortest duration of rain was August, which had only 12½ hours, while March had 26, and October and November a little more than 22 hours each. An examination of the hours of the rainfall during the night and during the day shows that on an average there are fewer hours of rain during the night than during the day. The longest interval without rain was 26 days, from September 11 to October 6, 1865. The greatest number of consecutive days of rain was 18, from October 3 to 20, 1867. The month of March had, on an average, the greatest number of rainy days, viz. 21·2, and the month of June the least, viz. 13·1. The months of greatest and least amount of rainfall do not correspond with these months, the maximum being 2·21 inches in September, and the minimum 1·00 inch in February.

THE *Monatsbericht der Deutschen Seewarte* for the whole year 1886 has been issued simultaneously with the Report for January 1887. The delay in the issue of the Reports is owing to some important alterations in the form of the work, especially the extension, considerably to the west, of the chart showing barometric minima. The above Report takes the place of the *Monatliche Uebersicht der Witterung*, which completed its tenth volume with the year 1885. It is proposed to issue it regularly in the third month after that to which it refers, and to include in it (1) a review of the atmospheric conditions over Central Europe, (2) preliminary communications respecting the weather in the North Atlantic, and (3) meteorological tables for Europe generally, and charts of the paths of the barometric minima over ocean and continent. This monthly report will be supplemented by a more complete quarterly review of the weather, which will appear after a lapse of about two years, and will serve as the explanatory text of the daily synoptic charts for the North Atlantic Ocean and adjacent continents lately referred to (May 26, p. 88). This text will be issued separately by the German Admiralty, and will be of great value to all interested in meteorological investigations.

FROM a recent report by Dr. Hellmann on statistics of lightning-damage in Schleswig-Holstein, Baden, and Hesse, it appears that the danger from lightning in these parts (unlike the case of other parts of Germany) has been decreasing of late years. Soft-roofed houses are fired about 7 times oftener than those with hard roofs. Windmills are struck 52 times, and church and clock towers 39 times, oftener than ordinary houses with hard roofs. The marshy regions in Schleswig-Holstein are the most dangerous; and the land about inlets of the east coast the safest. With like conditions, the relative danger decreases the more houses are grouped together. In Baden the danger varies more than in any part of Germany (about Heidelberg it is 24, and in Waldshut 265 per million houses). In Hesse, the low plain of the middle Rhine is the most dangerous part. In the fifteen years 1869-83, there were killed by lightning for every million men, in Prussia, 4·4; in Baden, 3·8; in France, 3·1; and in Sweden, 3·0. The geological nature of the ground, and especially its capacity for water, has important influence. Thus, calling the danger on lime 1, that for sand is 9, while for loam it is 22. This is partly why most of South Germany and Austria is less dangerous than North Germany.

There are four factors affecting the lightning-danger to buildings; two physical—unequal frequency of storms, and geological character; and two social—variable population, and mode of building. Of all trees, oaks are most frequently damaged, beeches most rarely (in the ratio 54 to 1).

An electric trumpet has been recently devised by M. Zigang (*La Nature*, June 4). It consists of a short brass tube mounted on wood and containing an electro-magnet whose ends face a vibrating plate, on which is fixed a small piece of soft iron. Against this plate-armature rests a regulating screw with platinum point, which serves for automatic interruption, by vibration of the armature. With two Leclanché elements a musical sound is had, which may be varied in pitch, intensity, and timbre by means of the screw. This instrument may be usefully employed in signalling on ships, railways, tramways, &c.; it may also serve as a receiver for signals of the Morse type.

Prof. Christensen, of Copenhagen, has recently (*Journal für praktische Chemie*, 1887, No. 11) made a redetermination of the atomic weight of fluorine, with the satisfactory result that this element is to be added to the already large list of those whose atomic weights are whole numbers and simple multiples of that of hydrogen. The determination was based upon the analysis of a double fluoride of ammonia and manganese, $4NH_4F \cdot Mn_2F6$, the extreme precautions displayed in the preparation and purification of which show the peculiar difficulties attending work upon this singular element. It is very interesting to read of the filtrations through platinum gauze placed in gutta-percha funnels, of the drying of the beautifully crystalline red salt spread out upon wide expanses of platinum-foil, and of the sklIful manner in which all traces of silicon were eventually eliminated. The results of the numerous analyses show that, if Stas's value for oxygen be taken as the standard, the atomic weight of fluorine is 18·94, but if, as Mendelejeff concludes, oxygen be 16, then the atomic weight of fluorine becomes 18·99, or, in round numbers, 19·0.

It will probably be remembered that, early in the year 1883 (*Ber. der Deut. Chem. Ges.*, xvi. 324), a number of chemical reactions, especially the formation of arsenides from mixtures of metals and arsenic, were brought about, by Dr. W. Spring, by subjecting the powdered mixtures to the immense pressure of 6000 atmospheres. A still more striking experiment, entirely unique in its way, has just been made by Dr. Spring, in conjunction with Dr. Van't Hoff (*Zeitschrift für physikalische Chemie*, i. 5). In the course of a study of chemical dynamics these workers found that the blue-coloured double acetate of copper and calcium, $(C_2H_3O_2)_4CaCu \cdot 8H_2O$, is perfectly stable at atmospheric pressure as far as 75°; above this temperature it is decomposed into its constituent acetates, three-quarters of its water of crystallization being set free. This decomposition is atended with contraction in volume, and the salts dissolve in the liberated water. The idea was at once suggested, Could this decomposition be effected by means of Dr. Spring's powerful compressing machinery? The idea was carried out, and no sooner was the pressure upon the solid double acetate increased to 7000 atmospheres, at a temperature of 40°, than the solution of the separated constituent salts spurted from every joint of the apparatus, and on releasing the pressure the resolidified mass was found to consist of a mixture of the white calcium and the green copper salt.

Last week we referred to the fact that the Council of the Meteorological Society are anxious to obtain photographs of flashes of lightning. The *Photographic News*, dealing with the conditions under which such photographs should be taken, notes the following points as important:—"First, the exact position of the camera. In many countries, ordnance maps can be obtained on such a scale that a minute dot will indicate the position of the camera within a foot or two, and it will often be easy to record the position of the apparatus with far greater exactness, as, for example, when the camera is placed at a window. A thread with a plummet should be allowed to range from the optical centre (say the diaphragm in case of a doublet) to the floor, where a mark should be made. Second, the time at which the exposure was made. Third, the aspect of the camera. When the locality is exactly recorded, this datum may be approximate, as there will generally be the means of exactly determining it upon the plate itself. Fourth, the equivalent focus of the lens, but the determination of this may well be left until it it is found that something valuable may be deduced from the photograph. Fifth, the distance of the flash. The recording of this is a very important matter, as, when the focus of the lens is known, it will be easy to determine the actual distance between cloud and earth, also the horizontal angle subtended to the observer at the camera. To determine the distance, the observer should note as accurately as possible the time elapsing between the flash and the report, and in doing this, even such a chronograph watch as may now be had for five or six pounds will be found of great service."

At a meeting of the Middlesex Natural History and Science Society, at the Natural History Museum, South Kensington, on the 11th inst., Prof. W. H. Flower, F.R.S., gave an address on the teeth of the Mammalia, especially referring to those specimens exhibited in the case in the Index Museum. Details of the structure, growth, and disposition of the teeth in the jaw were given, and the peculiarities of vestigial and rudimentary teeth pointed out. Prof. Flower referred to the value of these index museums, calling special attention to that of mineralogy, arranged by Mr. Lazarus Fletcher, and which for greater convenience was placed in the mineral gallery.

This evening, Prof. A. W. Williamson will deliver an address, in the Chemical Theatre, to the London University College Chemical and Physical Society. He has chosen as his subject, "Atomic Motion." Sir Henry Roscoe will take the chair.

"My Microscope, and Some Objects from My Cabinet," a simple introduction to the study of "the infinitely little," by a Quekett Club man, is announced for immediate publication by Messrs. Roper and Drowley. The little volume is dedicated to the President and Members of the Quekett Microscopical Club.

Messrs. Whittaker and Co. will publish early next week Mr. E. C. Robin's book on "The Design and Construction of Applied Science and Art Buildings, and their Suitable Fittings and Sanitation."

The Thuringian Fisheries Union held their tenth meeting at Jena on May 26. One of the members stated that 64,000 young salmon had been placed in the Saale last year. The Grand Duke of Saxe-Weimar was present at the meeting, and took part in the debates.

Dr. Moritz Wagner, Professor at the Munich University, died at Munich on May 31. He was well known as a scientific traveller, and author of some excellent works of travel.

During the five months ended May 1887 the total value of the fish landed on the east coast of Scotland was £335,366; on the west coast, £75,290; in Orkney and Shetland, £34,516; the total value for five months being £445,172. As compared with the corresponding period of last year, this was a decrease of £4,113. The last month, however, showed an increase of £6,478 over the corresponding month of the year 1886.

Artificial clouds for the protection of vines from frost were produced in a vineyard at Pagny on the Franco-German frontier during the night of May 13. About 3 a.m., when the thermometer had gone down to −1°·5 C., the signal was given to ignite liquid tar, which had been poured into tin boxes, and pieces of solid tar which had been placed in the ground near the vines. Large clouds of smoke quickly enveloped the vineyard. The fires lasted for about two hours, but the smoke did not clear off till a considerable time after. The object of the experiment was completely gained, as not one young shoot was destroyed by the frost.

THE American Institute of Electrical Engineers, organized three years ago, is making arrangements for the purchase of a suitable building in New York. It is proposed that there shall be an electrical library and museum, and, if space permits, an experimental laboratory. Suitable accommodation will be provided for council and general meetings, and the entertainment of members and their guests, and the house will be open "at all reasonable hours."

THE additions to the Zoological Society's Gardens during the past week include a Squirrel Monkey (*Chrysothrix sciurea*) from Guiana, presented by Miss Grace Williams; a Negro Tamarin (*Midas ursulus*) from Guiana, presented by Miss Julia Neilson; a Rhesus Monkey (*Macacus rhesus*) from India, presented by Miss R. M. Hurt; a Common Marmoset (*Hapale jacchus*) from South-East Brazil, presented by Mrs. Constance Hoendorff; a Common Raccoon (*Procyon lotor*) from North America, presented by Mr. G. F. Van Zandt; two Lanner Falcons (*Falco lanarius*), European, presented by Mr. William Thomson; two Scaly Ground Doves (*Scardafella squamosa*) from Brazil, presented by Mr. William de Castro; a Cockateel (*Calopsitta novæ-hollandiæ*) from Australia, presented by Mr. H. H. James; a Ring-necked Parrakeet (*Palæornis torquatus*) from India, presented by Mrs. Hill; a Yellow-billed Sheathbill (*Chionis alba*) from Cape Town, presented by Mr. R. C. Ashton; nine Barbary Turtle Doves (*Turtur risorius*) from Africa, presented by Mr. E. L. Armbrecht; a Red Brocket (*Cariacus rufus*), a Great American Egret (*Ardea egretta*) from Brazil, deposited; three Sandwich Island Geese (*Bernicla sandvicensis*) from the Sandwich Islands, a Wryneck (*Inyx torquilla*), European, purchased; a Wapiti Deer (*Cervus canadensis*), a Barbary Wild Sheep (*Ovis tragelaphus*), a Variegated Sheldrake (*Tadorna variegata*), nine Summer Ducks (*Æx sponsa*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE GREAT SOUTHERN COMET (1887 *a*).—Dr. J. M. Thome, of the Cordoba Observatory, has published in the *Astronomical Journal*, No. 156, some interesting particulars as to the appearance and observed positions of the great comet which he discovered on January 18. On the 21st it became evident that the comet was, in effect, all tail, the head being much the fainter part of the object, and being at least 15′ in diameter, very thin, and without nucleus or condensation of any kind. After various attempts at determining its co-ordinates, Dr. Thome adopted the plan of moving the telescope along the axis of the tail, until reaching a point beyond which nothing of a nebulous character could be distinguished, and determining its position. These points were approximately half a degree in advance of the true centre of the nebulosity, and nearly in its axis. The observations of position extend from January 21 to January 27. With regard to the appearance of the comet to the naked eye, Dr. Thome remarks that it was a beautiful sight—a narrow, straight, sharply-defined, graceful tail, over 40° long, shining with a soft starry light against the dark sky, beginning apparently without a head, and gradually widening and fading as it extended upwards.

The same number of the *Astronomical Journal* contains a discussion of the orbit of the comet by Mr. S. C. Chandler, Jun. The observations extend from January 20 to 29, and were made at Melbourne, Cordoba, the Cape, and Windsor, N.S.W. Two sets of elements—which do not materially differ, considering the extreme uncertainty of the observations—have been obtained; the first by taking the Cordoba observations as they stand, the second by attempting to determine the true centre of the nebulosity from Dr. Thome's statement that the recorded positions are 30′ in advance of the true centre and nearly in its axis. The elements are:—

	I.	II.
T (G.M.T.)	1887 Jan. 9·080	Jan. 8·730
ω	173° 36′·2	174° 48′·6
☊	130 46·2	132 48·6
i	61 48·9	57 52·1
log q	8·30484	8·36280

Mr. Chandler points out that these elements are very unlike those of comet 1880 I., with which this comet was at first associated. In fact the orbit found resembles more those assigned to the comets of 1680 and 1689, than that of the group 1843–80–82.

NEW MINOR PLANETS.—A new minor planet, No. 267, was discovered by M. Charlois at Nice on May 27. Another, No. 268, was discovered by M. Borelly at Marseilles on June 9.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 JUNE 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 June 19.

Sun rises, 3h. 44m.; souths, 12h. 0m. 58·8s.; sets, 20h. 17m.; decl. on meridian, 23° 26′ N.: Sidereal Time at Sunset, 14h. 8m.

Moon (New on June 21) rises, 2h. 48m.; souths, 10h. 20m.; sets, 18h. 1m.; decl. on meridian, 15° 57′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	5 21	13 39	21 57	23 39 N.
Venus	7 21	15 9	22 57	19 11 N.
Mars	2 50	11 2	19 14	22 50 N.
Jupiter	14 28	19 47	1 6*	8 50 S.
Saturn	5 43	13 47	21 51	21 41 N.

* Indicates that the setting is that of the following morning.

June.	h.	
21	18	Sun at greatest declination north; longest day in northern latitudes.
23	5	Saturn in conjunction with and 2° 26′ north of the Moon.
23	10	Mercury in conjunction with and 3° 27′ north of the Moon.
23	19	Jupiter stationary.
25	0	Venus in conjunction with and 2° 1′ north of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	June 23,	0 14	*m*
R Virginis	12 32·8	7 37 N.	,, 21,		*M*
δ Libræ	14 54·9	8 4 S.	,, 25,	1 0	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 20,	1 46	*m*
			and at intervals of	20 8	
W Sagittarii	17 57·8	29 35 S.	June 25,	2 0	*m*
U Sagittarii	18 25·2	19 12 S.	,, 25,	1 0	*m*
η Aquilæ	19 46·7	0 43 N.	,, 20,	1 0	*M*
S Sagittæ	19 50·9	16 20 N.	,, 24,	23 0	*m*
R Capricorni	20 5·0	14 36 S.	,, 23,		*M*
δ Cephei	22 25·0	57 50 N.	,, 21,	23 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.
Near β Ursæ Majoris	168°	55° N.
α Cephei	315	60 N.

GEOGRAPHICAL NOTES.

EMIN PASHA contributes to the Scottish Geographical Society's Journal an account of an exploration he made recently of part of Lake Albert Nyanza, which contains some data bearing on the probable origin and the physical geography of the lake. Off the station of Mabagi, on the north-west shore of the lake, Dr. Junker found a long, low, sandy island, which he recognized as of quite recent formation; for in 1879 he noticed that the spot where it now lies was covered with shallow water. Its length is 1067 yards, and maximum breadth 99 yards. Tall grass and weeds grow at the water's edge, and a species of acacia (*A. mellifera*) on the higher parts. The island, Emin Pasha states, is due to the deposition of the detritus brought down by the two rivers which enter from the south-west. From what he observed on the lake, he is inclined to believe that the foreshore on the west is gradually encroaching on its waters; in other words, the lake in this part is gradually filling up. As for the lake itself, Emin Pasha attributes its origin solely to erosion. He thinks it more than probable that formerly a large stream may have made its way from between the two ranges to east and west of the lake, so that its erosive action, combined with that of inundations, heavy rains, caving-in, and the influence of the sun and weather, are quite sufficient to account for the result. The geological formation of both ranges is the same; their altitudes differ but little, and the terrace-like formation of their descent lakewards is in each case exactly alike. Emin Pasha hoped to examine the problem much more minutely. He landed at Kibiro, on the opposite side cf the lake, and gives an interesting description of the valuable salt-mines of the region. Emin Pasha afterwards made two other journeys on the lake, during one of which he discovered what he believes to be a new river, called Kakibbi by the Wasongora, and Duéru by the Wamboga. It flows from the Ussongora Mountains, and is of considerable size, and enters the lake at the south, having a large island near its *embouchure*. It abounds with cataracts, and is therefore unnavigable. To the south-west, Emin Pasha was informed, there is a large river on the banks of which there is a colony of Akkas—called Balia by the Wanyoro people, but by themselves Betua; the latter a name suggesting the Batua recently discovered by Lieut. Wolf on the Sankuru, to the south of the Congo. Is it not possible that the Kakibbi is the same as the "red river" discovered by Mason Bey in 1877, entering the south extremity of Lake Albert?

IN the *Bulletin* of the Lyons Anthropological Society will be found an interesting paper by M. Bertholon on the "Arab Colonization of France," in which the author, mainly on the basis of place-names, seeks to identify the existing effects of the Saracenic invasion of France. Dr. Collomb has also a useful paper on the peoples of the Upper Niger, their manners and their history.

M. ÉDOUARD DUPONT, Director of the Brussels Natural History Museum, is about to leave for the Congo, to make a geological investigation of the region along the south bank of the river, between Boma and Stanley Pool. He will endeavour especially to determine the epoch when the river broke through the coast range, and the age of these mountains. He will also explore any caves which may exist, in order to discover if there are any remains of a primitive population.

THE new number of Petermann's *Mitteilungen* is one of special scientific interest. The first paper gives the results of a series of researches by Japanese botanists on the botanical zones of Japan, in which the relation of these zones is shown to the configuration of the surface of the country. A much longer and perhaps more important paper in the same department is Herr Ernst Hartert's account of the botanical results of the expedition to the Niger under the late Herr Flegel; it abounds with detailed information on the plants collected by the expedition. Dr. Alex. Supan, the able editor of the *Mitteilungen*, who takes a special interest in meteorology, contributes a carefully elaborated paper on the mean duration of the chief heat periods in Europe. Then we have a series of extracts from Emin Pasha's letters, from 1883 down to 1886, much of which has already been published.

THE NEPHRIDIA OF "LANICE CONCHILEGA," MALMGREN.[1]

SEVERAL accounts of the nephridia of *Terebella conchilega* have been given. H. Milne-Edwards (*Ann. d. Sci. Nat.* (2) *Zoologie*, x., 1838, p. 220), in a paper published in 1838, on the circulation in Annelids, describes the vascular system in a species to which he gives this name, and gives a figure of the animal opened along the dorsal median line. In this figure four looped nephridia are distinctly shown, situated behind the branchial region. The representation of the position and character of these organs is perfectly correct so far as it goes: they are the upper parts of the four nephridia belonging to somites 6–9. But the paper I refer to does not describe the nephridia, as it deals with another subject: they are shown in the figure, and that is all; and in the description of the figure the organs are referred to as organs of generation.

Keferstein (*Zeitschrift für wiss. Zoologie*, Bd. xii., 1862) mentions that the structure and number of the nephridia in *T. conchilega* are the same as in *T. gelatinosa*, Kef.: in both cases he says there are six pairs, each organ consisting of a tube bent on itself, of which one half is darker, the other lighter: the organs belong to segments 3–9.

Cosmovici[1] gives an erroneous description of the organs: he says there are two pairs without internal openings, which he calls "organs of Bojanus," one of these situated in front of the cephalic diaphragm, the other immediately behind it, each organ having an external opening; and two other pairs, each of which has an internal as well as external opening, and is shaped like an urn: the internal opening is large, and surrounded with a circular lip. The gonad is attached to the posterior part of each of these latter organs, which Cosmovici calls segmental organs, and which he says serve as efferent excretory ducts.

The species referred to by these three authors is the *Nereis conchilega* of Pallas, *Terebella conchilega* of Gmelin; and this is called *Lanice conchilega* by Malmgren. My specimens were identified from Malmgren's description, and there is no doubt of the identity of my specimens with the species of that author; but there is room for some uncertainty regarding the specific identity of the specimens referred to by the authors I have mentioned. For instance, Cosmovici identified his species by means of Quatrefages' "Histoire des Annelés," 1865, and there it is stated that the tube of *Terebella conchilega* possesses no hollow fringes at its mouth: these fringes are always present in the tube of *Lanice conchilega*, Malmgren. This species is distinguished by some marked characters: two of them are the presence of a large vertical lobe on the 3rd somite (second branchiferous) and the coalescence of the ventral scutes usually present into a continuous ventral plate.

The true relations of the excretory system are as follow:—Enumerating the somites from before backwards, and counting the buccal as the 1st, we find that the branchiæ belong to somites 2, 3, and 4: the first notopodial fascicle of capillary setæ is on the 4th somite, the third branchiferous; the first neuropodial uncinigerous torus is on the 5th: the neuropodial tori are repeated on every succeeding somite to the end of the body; the notopodial fascicles occur only on seventeen consecutive somites. There are traces of transverse septa behind the 1st, 2nd, 3rd, and 4th somites, but none in the rest of the thoracic region, which bears the notopodial fascicles. On dissection, four long double nephridial tubes are seen projecting dorsalwards with the body-cavity; the lower parts of these tubes are covered by strands of the oblique muscles which pass from the nerve-cord to the neighbourhood of the notopodial bristles: careful examination shows that these tubes belong to somites 6, 7, 8, and 9. Their internal openings can be found immediately behind the fascicle of bristles belonging to somites 5, 6, 7, and 8 respectively, but their efferent tubes are seen to pass down beneath the fascicles of somites 6, 7, 8, and 9. The lower parts of these efferent tubes are very wide, and it is impossible to separate them from one another. Beneath the fascicles of the following four somites (10–13 inclusive) are seen membranous nephridial sacs, which externally at least are inseparable from one another. These sacs are simple, that is, they are not composed of a tube bent on itself like the anterior nephridia: they scarcely extend above the level of the oblique muscles, and no internal opening or nephrostome can be found in them. In front of the most anterior nephridium, that belonging to somite 6, are seen traces of a rudimentary nephridium. In order to trace out the relations of these nephridia more accurately, the anterior part of a specimen was cut into a series of horizontal longitudinal sections, commencing with the ventral surface, and the reason why the successive nephridia could not be isolated from one another was seen on examination of these sections: the lower parts of the efferent limbs of the four anterior normal nephridia, in somites 6–9, and the whole of the nephridial sacs in somites

[1] A Paper read before the Royal Society of Edinburgh by Mr. J. T. Cunningham, on Monday, May 16.

[1] "Glandes génitales et Organes segmentaires des Annélides polychètes" (*Arch. de Zool. Exp.*, t. viii., 1879–80).

10–13, are in open communication, forming a wide continuous longitudinal tube extending from somite 6 to somite 13. Openings to the exterior from this tube were found in somites 6–9 inclusive, corresponding to the four large looped nephridia: each of these openings was close behind the upper end of an uncinigerous torus. The internal openings of the same four nephridia could be traced with ease and certainty: they are attached to the body-wall close behind the notopodial fascicles of somites 5–8. These openings are wide, and are overhung dorsally by a longitudinal lip furnished with a series of small ciliated digitate processes: lower down, the anterior and posterior lips of the opening are simple, thick-walled, and ciliated. The aperture leads into a thin tube, which passes inwards and backwards, curving round the inner end of the fascicle of bristles behind the aperture, and then, crossing the continuous tube, passes up on the inner or medial side of the loop, at the apex of which it is continued into the efferent wider limb of the loop, which passes down on the outer side to open into the longitudinal tube. Neither internal nor external openings could be found in that part of the longitudinal tube which is behind the loops: it seems evident that this part of the tube represents four somewhat reduced nephridia which have coalesced, but whose openings have disappeared. Anteriorly to the four looped nephridia are traces of three others: the longitudinal tube extends forwards into somite 5 as if it included a nephridium belonging to that somite, but I could find no external opening in this somite: at the angle between the septum behind somite 4 and the body-wall is a very obvious nephrostome, which ought to lead into the longitudinal tube, into that part of it corresponding to somite 5, but the connexion could not be traced. Nephrostomes were also present attached to the anterior face of the septa behind somites 2 and 3 (the first and second branchiferous), and leading into tubes seen in somites 3 and 4, but I could find no external openings in these somites. I could find no nephrostome in somite 1 (the buccal) nor any trace of a tube in somite 2. Gonads are present in the form of clumps of deeply-staining small indifferent cells attached to the exterior of all the nephrostomata mentioned, seven in all. The germinal cells, when still quite undifferentiated, separate from the gonads, and undergo further development in the cœlome. But I found no reproductive elements in the cavity of the nephridial system, though the body-cavity contained them in quantity, and it is probable that at the right season they are expelled through the nephridial system. The body-cavity contains, besides the reproductive elements, a large number of spherical, vacuolated, nucleated cells. This is the first case in which a communication between successive nephridia has ever been discovered in any adult invertebrate. It is true that in the development of Polygordius, according to Hatschek, each nephridium gives off backwards a prolongation of itself, from which the next nephridium is formed, and the two remain in communication for a time; but the connexion is soon severed, and in the adult the successive nephridia are isolated and independent. In *Lanice conchilega* the nephridia have coalesced together after coming in contact from before backwards, the separating membranes having disappeared. The case is extremely interesting in the fact that we have in it an approximation to the condition of the excretory system in Vertebrata: the presence of a metameric series of nephrostomata in vertebrate embryos has long ago been seen to constitute a resemblance between them and Chætopoda, but hitherto no Chætopod was known which resembled the vertebrate in having a number of nephridia coalesced to form a continuous longitudinal tube.

It is surprising to find that, as far as I have been able to discover, no resemblance to the condition seen in *Lanice conchilega* occurs in any of its near allies. The only species of the genus Terebella as defined by Malmgren that occurs in the Firth of Forth is *Terebella Danielsseni*, but of this I have only one specimen, and have not examined its nephridia. Of Amphitrite there are two species in the Forth: *Amphitrite cirrata* I have not examined anatomically; in *Amphitrite Johnstoni* there are a large number (15–17) of nephridia forming long loops projecting dorsalwards into the body-cavity, in the anterior region: each has its own internal and external openings, and is isolated and independent. In *Terebellides Strœmii* there is one pair of large dark-coloured nephridia in the anterior end, and three pairs of small rudimentary ones posterior to this. In *Pectinaria belgica* there are three pairs: they are all independent. In *Melinna cristata* there are several pairs, all separate. Figures showing the interesting relations which exist in *Lanice conchilega*, together with a more complete description of the nephridia in other forms of Polychæta, will I hope shortly be published in a paper on the anatomy of Polychæta.

NOTES ON THE GEOLOGY OF PART OF THE EASTERN COAST OF CHINA AND THE ADJACENT ISLANDS.

SURGEON P. W. BASSETT-SMITH, R.N., has forwarded to the Hydrographical Department of the Admiralty a brief Report on this area, embodying the results of ob ervations made in the course of last summer during the cruise of H.M.S. *Rambler*. Specimens of rocks were collected at certain points on the mainland and on the neighbouring islands, stretching from Chusan on the north to Ockseu Island, south of Hai-tan Strait, opposite the northern part of Formosa.

All the islands, with a single exception, appear to consist of crystalline rocks. They usually present sharp rugged outlines, with bold cliffs—more or less fissured and veined—rising, in many cases, vertically from moderately deep water. In the following notes, the stations from which the specimens were collected are described in succession from north southwards.

Tou-wah Island, the most northerly station, consists of an irregular range of hills trending in a north-west and south-east direction, and reaching an elevation of 1600 feet. A gray granitic rock was obtained from the summit. Thornton Peak, on the mainland in the province of Chi-kiang, separated from the Chusan group by a narrow sea, is composed of a pink granite. From Ta-fou Island, in San-moon Bay, a fine-grained purplish quartz-felsite was obtained. The group of Hae-shan Isles seems to be composed of a dark gray quartz-felsite, and a similar rock forms the Tai-chow Islands.

Another group of stations visited by the *Rambler* lies off the coast of the province of Fu-kien. Fuh-yan Island consists of hills reaching a height of 1700 feet, and yielding a fine-grained greenish rock, apparently a diabase. Coney Island is composed for the most part of a coarse pinkish granite, with veins of quartz, and dykes which appear to consist of diabase and hornblende-porphyrite. The two islands known as Tung Yung are formed mainly of quartz-felsite; the specimens obtained from the larger of the two isles containing much opaque white feldspar, porphyritically distributed through the rock. In a cove at the south-west end of the latter island, the rocks split up into irregular columns, and in certain parts these columns exhibit considerable curvature.

The third group of stations is situated on the River Min, and in the neighbourhood of its mouth. Chang-chi is a large irregular-shaped island of red porphyrite. The island known as Matsou is particularly interesting, the principal rock being a white quartz-felsite, with a complicated network of basaltic dykes. In a small sandy bay, a deep water-course exposes a layer of dark earth, about a foot in thickness, crowded with land shells. Two small neighbouring islands known as White Dog consist of dark gray quartz-felsite.

On the north side of the mouth of the River Min is an island, termed Sharp Peak, about three miles in length, which culminates in a rocky peak 1500 feet high. The island is formed, for the most part, of a hard conglomerate, associated with slates and shales, and with a talcose schist penetrated by veins of quartz. A cliff at the north-east point of the island displayed a clear section, in which this schist was seen to alternate with beds of slate and conglomerate, inclined at about 45°.

A small low island off the south point at the entrance to the River Min, consists of granite, gneiss, and mica-schist. A specimen of red granite, with crystals of iron pyrites, was obtained from the rugged mountains of the neighbouring mainland. Temple Point, on the north side of the Min, a few miles from its mouth, yielded a greenish-yellow steatitic rock, with dendritic markings. At Pagoda Anchorage, up the river, a fine-grained pink gneiss was obtained, and this locality also yielded a fragment of a large crystal of smoky quartz. About twelve miles further up the Yuen Fu branch of the Min River are some hot springs having a maximum temperature, in November, of 114° F. The rock is here a quartz-felsite. An orthoclase porphyry occurs about five miles further up the river, and quartz-felsite again occurred ten miles higher. Here, in a curious recess in the hill-side, in which a temple has been built, are numerous stalactites, some of large size. The mountains all up the Yuen Fu are very fine, presenting a succession of bold outlines and rocky peaks. A dark-gray quartz-felsite was obtained from a high peak in a range of hills bounding the water-shed of the Min on the south. From the base of the hills a stretch of low flat reclaimed land extends to the coast. The soil of the hills is of a bright red colour, contrasting with the dark tints of the felsitic rocks.

The fourth group of stations includes a number of localities around Hai-tan Strait. Here the hills present vivid colouring, which contrasts very markedly with the white sands of the shore, especially on Hai-tan Island itself. This consists of three ranges of hills, with intermediate barren plains. Near the north point is a group of reddish sand-cliffs, from 20 to 30 feet high, horizontally stratified, and presenting flat summits, which form a miniature plateau deeply trenched by numerous gullies. At the mouth of the strait is a small barren island—Tessara Island—composed of gneissose rocks, which carry iron pyrites. Slut Island, about 400 feet high, yielded a dark porphyritic felsite, and a weathered surface of the rock displayed evidence of fluxion structure. Syang Point, at Hai-tan, shows granitic rocks running up into high hills. Kiang-shan, on Hai-tan Island, is a hill 1800 feet high, composed of dark-gray quartz-felsite. Mount Bernie, on the mainland, at the south end of the strait, about 1400 feet in height, is composed of a similar rock, weathering to a reddish earth; and in Hungwah Sound the hills are of similar character. In Ockseu, a small rocky island, about twenty miles south of Hai-tan, is a dark-coloured rock, apparently dioritic, and certain masses of this rock when struck, emit a ringing sound, like that of a phonolite. There are here numerous veins of quartz, some showing rather bold crystals, and a good deal of schorl, or black tourmaline. It is notable that the island of Ockseu is especially subject to seismic disturbances.

THE METEOROLOGY OF INDIA[1]

It is perhaps inseparable from the mode of issue of the "Indian Meteorological Memoirs" that their titles (*e.g.* Vol. III., Part I., I.—Rainfall, Part I.) are rather complex. It is stated that this memoir is to be in three parts, whereof the present part treats only of the normal rainfall of India; Part II. is to treat of its variations in past years; Part III. is to contain the tabular data: the whole to form Vol. III. of the series.

As India depends chiefly on agriculture, the investigation of the conditions affecting its rainfall is of the highest practical importance to it. The registers of rainfall available are, except a few private ones, all official work done under Government orders. Some few extend from 1844, but the most of those accepted as trustworthy, after a critical examination, date from about 1862; the discussion includes the data only down to 1883, *i.e.* covers pretty nearly a complete record for twenty-two years. Altogether, the registers of 424 stations are reviewed: for purposes of discussion these are grouped into twenty-five "rainfall districts," *i.e.* districts with similar rainfall.

From all these it appears that the average rainfall of the whole of India, excluding Burmah and the Himálya, is about 42 inches. The range of rainfall over this wide area is one of the most wonderful in the world, viz. from about 500 or 600 inches in Cherra Púnjí to from 1 to 5 inches in Sindh. The average annual range over the whole of India (as above) is about 13 in the whole 42 inches. The rainfall is discussed under four heads:—

(1) Summer Monsoons. (2) Autumn Rains in South-East.
(3) Winter Rains. (4) Spring Storms.

The local distribution of 1, 3, and 4 is well shown by tints of various shades on three maps. For the connection with the state of air-pressure, twelve maps are given, showing the isobars for the mean pressure of each month; the discussion of this connection is complicated, and difficult to summarise.

(1) *Summer (South-West) Monsoon.*—By some, the south-west monsoon is considered to be an extension of the south-east trade-winds, but the author considers their connection to be very doubtful, and gives a rough calculation, showing that the evaporation from the Northern Indian Ocean, land of India, and Bay of Bengal is enough to account for the whole of this season's rain. This rainfall is far the heaviest of the four seasons, and the most important for agriculture for most part of India, being, in fact, popularly styled "the rains." On its sufficiency depend the lives of millions. The distribution is at once seen by the tinted map. The west coasts of India and Arakhan catch the first and heaviest fall of over 100 inches: this does not top the coast range of mountains. The next heaviest is from the head of the Bay of Bengal to the Himálya, thence all along the lower Himálya, of from 50 to 70 inches. The amount decreases thence steadily with distance from the head of the Bay of Bengal, and from the Himálya, dwindling to almost nothing on the south-east coast and north-west border.

The effect of a mountain-range in intercepting rain is clearly brought out, *e.g.* in the Western Ghâts this rainfall, coming from the south-west, decreases from 250 inches on the coast to 40 inches at 30 miles inland, and to 20 inches at 60 miles from the coast. Again, very little rain crosses the outer snowy range of the Himálya. In fact, it seems to be an established law that the precipitation of rain from damp air is greatest in an ascending current from the chill produced in the ascent, and only moderate in a horizontal current.

(2) *Autumn Rains in South-East.*—The author shows that these are not (as often stated) a part of the north-east monsoon, but are, in fact, a late part of the south-west monsoon, corresponding to the late and heaviest part of the same on the Arakhan coast.

(3) *Winter Rains.*—These are popularly styled the north-east monsoon, and are popularly said to be due to a reversal of the conditions of the south-west monsoon. Their distribution is, roughly speaking, the opposite of that of the south-west monsoon, and is well shown on the map given. The south-east coasts, which scarcely feel the south-west monsoon, catch the maximum of over 10 inches of this season, the North-West Himálya catch from 5 to 10 inches, the head of the Bay of Bengal from 3 to 5 inches, and the rest of the country less and less with increased distance from these places.

Small as these quantities are (compared to those of the south-west monsoon), they are of the greatest importance to some of the localities named, especially to North-West India, as on them depends the growth of the valuable crops of temperate climates, *e.g.* wheat, the staple of North-West India; indeed, in the extreme north-west the winter is the dampest season.

(4) *Spring Storms.*—This rainfall is distinguished by increasing with the advance of the season, *i.e.* with the rising temperature, and mainly restricted to the south and east provinces. It is often accompanied by hail and thunderstorms, and is common in the evenings. This rain is usually very local, of short duration, heavy, and frequently repeated.

Altogether, this is a most elaborate and valuable monograph on its subject—the normal rainfall of India.

Part I. of Vol. IV. of "Indian Meteorological Memoirs" contains three memoirs, each a short monograph on its own subject, by different authors: these will be dealt with separately.

I.—"Account of the South-West Monsoon Storm of May 12–17 in the Bay of Bengal and at Akyab," by J. Eliot (pp. 38, and 2 plates). The history of this storm has been worked out from the meteorological reports of fourteen coast stations, and the logs of fourteen vessels passing through the Bay of Bengal. The states of the barometer and wind are shown for four days on four charts, and the track of the storm-centre on another. The meteorological conditions seem to have been remarkably uniform over the Bay of Bengal for a fortnight preceding the storm; indeed, this seems to be the normal state of things before a cyclone. The south-east trade-winds seem to have extended north of the equator on May 10 and 11, and gradually advanced into the Bay of Bengal, as strong south and south-west squalls, with rain, increasing in violence within the Bay. In front of these, a barometric depression was formed about the 12th, round which, as a vortex, the wind became cyclonic. This cyclone advanced in a curved path north and east (whereas most cyclones advance north and west up the Bay), increasing from 6 miles per hour on the 15th to 15 miles per hour on the 17th, and broke up on the Arakhan Hills close over Akyab on the 17th, doing great damage to property.

II.—"On the Diurnal Variation of the Rainfall at Calcutta," by H. F. Blanford, F.R.S. (pp. 8, and 1 plate). This is a

[1] "Indian Meteorological Memoirs," Vol. III., Part I. I.—The Rainfall of India, Part I. Pp. 116, and 9 Plates. A Monograph by H. F. Blanford, F.R.S. (Calcutta: Government Printing Press, 1886.)
"Indian Meteorological Memoirs," Vol. IV., Part I. Pp. 57, and 4 Plates. Edited by H. F. Blanford, F.R.S. (Calcutta: Government Printing Press, 1886.)
"Report on the Meteorology of India in 1884," by H. F. Blanford, F.R.S. Pp. 305 and 3 Plates. (Calcutta: Government Printing Press, 1886.)

iscussion of the hourly frequency and quantity of rain in a period of seven years (1878–84), derived from a self-registering Casella's hyetograph. The results do not seem of much practical importance. In the rainy season the rain is least frequent at the hour of maximum pressure, and most frequent at the coldest hour. At other seasons, dust-storms, with rain, are commonest in the evening. The greatest and least rainfall occur in general at the hours of greatest and least frequency.

III.—"The Meteorological Features of the Southern Part of the Bay of Bengal," by W. L. Dallas (pp. 11, and 1 plate). This is a discussion of the meteorology of a square district of 4° by 4° of the Indian Ocean, about half way between Ceylon and Sumatra, derived from the logs of ships. The air-pressure is at a maximum in January and at a minimum in May, with slight minima in July and October, which seem related to the occurrence of cyclones. The diurnal variation is extremely regular, the minima falling about 3h. 30m. and 15h 40 n., and the maxima about 9h. and 22h. The range is markedly largest in April and September, *i.e.* at the two great seasonal changes. The mean temperature is 80°·9, and the range of the mean monthly temperature is only 3°, which is smaller than at any coast station: the diurnal range of the year is about 2°·7, varying from 3°·75 in April to 1°·8 in May, the maximum and minimum being thus close together. In the summer (south-west) monsoon calms are rare. From April to September the wind is pretty steady from south-west to west-south-west, and, from December to March, generally from north to north-east. Only thirteen gales are recorded in twenty-five years, and none of them over force 9 of the Beaufort scale.

Mr. Blanford's "Report" for 1884 is a discussion of the meteorology of India in 1884, on the same general plan as adopted for the ten years preceding. The discussion rests on observations supplied from 134 reporting-stations. Each meteorological element is discussed separately, beginning with the solar radiation as being the prime cause of all meteorological change; next, earth-radiation, temperature, humidity, cloudiness; and, lastly, rainfall. The great extent of India, and its isolation by ocean and mountain from other countries, render it a country most favourable for meteorological study. One singular feature is, that most considerable variations are of a somewhat lasting character, sometimes lasting two seasons, *e.g.* heavy snow in the spring in the Himálya is followed by steady north-west winds over the plains of Northern India, afterwards turning into the hot west winds.

The year under review was in some ways peculiar. Perhaps the most striking feature brought out is that, ever since 1878, the temperature of insolation and of the air have both steadily fallen, and were lowest in 1884 (1°·2 less than in 1878), although the sky was slightly less cloudy than in 1883: it seems likely that this is part of a cyclic change connected with that of the sunspots, the temperature being highest at the sunspot minimum, and *vice versâ*. The mean air-pressure was slightly (0"·01) above that of past years, and also much steadier. The average humidity was rather lower, and the average clearness of sky somewhat greater than in the recent years, and yet the total rainfall was somewhat greater: this was chiefly due to excess of rain in North-West, Central, and South-East India. Heavy snow fell in the North-West Himálya early in the year, bringing rain to the North-West Punjab, and dry north-west winds in North India generally, followed by a hotter summer than usual. The south-west monsoon bringing the rain sets in in North India in June. The storms of the year were somewhat singular. From July to September a series of cyclones formed in the Bay of Bengal, and followed a north and west course far into the plains of India: this course seems to be the usual cyclone track of the Bay of Bengal. One of these, in July, crossed the entire breadth of India, and one, in September, lasted over a fortnight. Heavy snow fell in the outer Himálya in September and October, followed by north-west winds in North India, and by an unusually cool winter in India generally. Twelve charts accompany this Report, showing the mean monthly temperature, air-pressure, and wind; the isotherms, isobars, and wind-resultants being plotted in colours on each monthly chart. This annual Report, of which a very brief summary only is here given, is the outcome of an enormous amount of labour: the detailed tables of data covering 305 quarto pages, these tables being themselves mostly the result of laborious computation from the data furnished by the observatories.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Mathematical Examiners have bracketed as Senior Wranglers Messrs. Baker and Flux of St. John's, and Iles and Michell of Trinity. It is unprecedented to have a bracketed Senior Wrangler. No women students have this year been placed as Wranglers.

The following women students have been placed in the first class of the Natural Sciences Tripos, Part I., E. E. Field, A. J. Flavell, and M. M. Smith, all of Newnham College.

The Honorary Degree of Doctor in Science has been conferred on Prof. Asa Gray, of Harvard.

SCIENTIFIC SERIALS.

Annalen der Physik und Chemie, No. 6, June.—R. Emden, on the vapour-pressures of saline solutions. Criticism of prior results, and fresh experiments conducted according to the method of Konowalow. Babo's law, that the vapour-tension of saline solutions is always proportional to that of pure water at the same temperature, is shown to be true between 20° C. and 95° C.—Max Planck, on the principle of increase of entropy. Application of this principle in the study of dissociation of gases.—C. R. Schulze, on the amount of water of crystallization held in various salts. Proves the existence of a new form of sulphate of magnesia having density 1·8981, containing six molecules of water, and therefore differing from Mitscherlich's salt of same composition of density 1·6151.—W. Voigt, on the theory of light for absorbing isotropic media. A development of the theory propounded by the author three years ago.—C. L. Weber, on the galvanic conductivity of amalgams. The amalgams examined were of tin, bismuth, lead, cadmium. Addition of tin increases conductivity of mercury; bismuth increases it until 10 per cent. of bismuth has been added, after which further addition decreases the conductivity; lead shows a maximum at about 25 per cent.; cadmium produces a steady increase in conductivity.—Adolf Koepsel, determination of magnetic moments and absolute strength of currents by means of the balance. The method is due to R. von Helmholtz, and is independent of the earth's magnetic field or its variations. The author has made by this method a new determination of the electro-chemical equivalent of silver, which he gives as 0·011740 ± 0·0000022 in C.G.S. measure. Lord Rayleigh's value was 0·011794.—Walter König, magnetic researches on crystals. A very careful research on magnetic susceptibility of quartz and calc-spar in magnetic fields of various degrees of intensity. The two principal permeabilities in calc-spar possess a constant difference in fields of various strengths up to 3000 C.G.S.; for quartz, the difference diminishes as the field is strengthened, and is less than that of calc-spar.—R. Clausius, reply to some remarks of Lorberg upon dynamo-electric machines.—A. Foeppl, electricity as an elastic fluid. A speculative paper: the author thinks the existence of the Hall effect a criterion of his theory.—K. Wesondonck, on the absence of polar difference in spark-potential.—G. Meyer, note on the index of refraction of ice; the value for sodium light is 1·3133.—E. Ketteler, on the dispersion of rock-salt. The author thinks he has established the law that the absorbing power of substances for heat-rays is proportional to the negative coefficient of the term in λ^2 in the formula which he uses in place of Cauchy's for the law of dispersion.—W. Voigt, reply to Wernicke's remarks on elliptic polarization.—F. Braun, on the diminution of the compressibility of solutions of sal-ammoniac with increase in temperature.—A. Overbeck, on the signification of the absolute system of measurement.

SOCIETIES AND ACADEMIES.

LONDON.

Geological Society, May 25.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—On the remains of fishes from the Keuper of Warwick and Nottingham, by Mr. E. T. Newton; with notes on their mode of occurrence by the Rev. P. B. Brodie and Mr. E. Wilson.—Considerations on the date, duration, and conditions of the Glacial period with reference to the antiquity of man, by Prof. Joseph Prestwich. After showing how the discoveries in the valley of the Somme and elsewhere, twenty-eight years ago, led geologists who had previously been disposed to restrict the age of man to exaggerate the period during which the human race had existed, the author proceeded to discuss the views of Dr. Croll on the date of the Glacial epoch. Dr. Croll, who had at first referred this to an earlier phase of orbital eccentricity, commencing 980,000 years ago, subsequently regarded it as coinciding with a minor period of eccentricity that commenced 240,000 and terminated 80,000 years since. This last estimate was chiefly supported by the amount of denudation that had subsequently taken place. The efficacy of the increased eccentricity of the earth's orbit in producing the cold of the Glacial epoch was shown to be very doubtful; for as similar changes in the eccentricity had occurred 165 times in the last 100 millions of years, there must have been many glacial epochs in geological time, several of them much more severe than that of the Pleistocene period. But of such glacial epochs there was no valid evidence. Another inference from Dr. Croll's theories, that each glacial epoch consisted of a succession of alternating cold and warm or interglacial phases, was also questioned, such alternations as had been indicated having probably been due to changes in the distribution of land and water, not to cosmical causes. The time requisite for such interglacial periods as were supported by geological evidence was more probably hundreds than thousands of years. Recent observations in Greenland by Prof. Helland, Mr. V. Steenstrup, and Dr. Rink, had shown that the movement of ice in large quantities was much more rapid, and consequently the denudation produced much greater than was formerly supposed. The average rate of progress in several of the large iceberg-producing glaciers in Greenland had been found to be 36 feet daily. Applying these data and the probable accumulation of ice due to the rainfall and condensation to the determination of the time necessary for the formation of the ice-sheet, the author was disposed to limit the duration of the Glacial epoch to from 15,000 to 20,000 years, including in this estimate the time during which the cold was increasing, or preglacial time, and that during which the cold was diminishing, or postglacial time. Details were then given to show that the estimate of 1 foot on an average being removed from the surface by denudation in 6000 years, on which estimate was founded the hypothesis of 80,000 years having elapsed since the Glacial epoch, was insufficient, as a somewhat heavier rainfall and the disintegrating effects of frost would produce far more rapid denudation. It was incredible that man should have remained physically unchanged throughout so long a period. At the same time, the evidence brought forward by Mr. Tiddeman, Dr. Hicks, and Mr. Skertchly of the occurrence of human relics in preglacial times, had led the author to change his views as to the age of the high-level gravels in the Somme, Seine, Thames, and Avon valleys, and he was now disposed to assign these beds to the early part of the Glacial epoch, when the ice-sheet was advancing. This advance drove the men who then inhabited Western Europe to localities such as those mentioned which were not covered with ice. Man must, however, have occupied the country but a short time before the land was overwhelmed by the ice-sheet. The close of the Glacial epoch, *i.e.* the final melting of the ice-sheet, might have taken place from 8000 to 10,000 years since. Neolithic man made his appearance in Europe 3000 to 4000 years B.C., but may have existed for a long time previously in the east, as in Egypt and Asia Minor civilized communities and large States flourished at an earlier date than 4000 B.C. After the reading of the paper there was a discussion, in which the President, Dr. Evans, Dr. Geikie, Prof. Boyd Dawkins, Dr. Hicks, and others took part. —Notes on some Carboniferous species of *Murchisonia* in our public museums, by Miss Jane Donald. Communicated by Mr. J. G. Goodchild.

Zoological Society, June 7.—Mr. E. W. H. Holdsworth, 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, and called attention to a Tooth-billed Pigeon (*Didunculus strigirostris*) brought home from the Samoan Islands, and presented to the Society by Mr. Wilfred Powell; to two Red-spotted Lizards (*Eremias rubro-punctata*) obtained at Moses' Well, in the Peninsula of Sinai, and presented to the Society by Mr. G. Wigan; and to a small scarlet Tree-Frog (*Dendrobates typographus*) from Costa Rica, presented to the Society by Mr. C. H. Blomefield.—Mr. Sclater called attention to examples of two North American Foxes now living in the Society's Gardens, which he referred to *Canis velox* and *C. virginianus*.—A communication was read from Mr. A. O. Hume, containing some notes on *Budorcas taxicolor*, the Gnu-goat or Takin of the Mishmee Hills, and some remarks on the question of the form of the horns in the female of this animal.—A communication was read from Mr. E. Symonds, containing notes on various species of Snakes met with in the vicinity of Kroonstadt, Orange Free State, specimens of which had been forwarded to Mr. J. H. Gurney, and determined by Dr. Günther.—Mr. Martin Jacoby, gave an account of a small collection of Coleoptera obtained by Mr. W. L. Sclater in British Guiana.—Prof. G. B. Howes, read a paper on a hitherto unrecognized feature in the larynx of the Anurous Amphibians. This was the existence in many individuals of various species of a rudimentary structure, which appeared to correspond to the epiglottis of Mammals, and which in some instances attained a remarkable development as an organ of voice.

Institution of Civil Engineers, June 7.—Annual General Meeting.—Mr. Woods, President, in the chair.—The Report of the Council on the condition of the Institution, and the annual statement of the accounts, were received. The number of members on the roll of the Institution, on March 31, 1887, was 4347, of whom 20 were honorary members, 1568 members, 2275 associate members, and 484 associates. This was a net increase of 173, or 4·19 per cent., on the 4174 members of all classes recorded last year. The elections had included 34 members, 234 associate members, and 6 associates, while the deaths, resignations, and erasures were 106. Many deaths had occurred among the older members of the Institution during the past twelve months, chief among whom must be placed Sir Joseph Whitworth, whose world-wide renown as a mechanician it was unnecessary to dwell upon. By his will he bequeathed to the Institution 80 shares, of £25 each, in the firm of Sir Joseph Whitworth and Company, Limited. During the twelve months under review, 211 candidates were admitted as students. On the other hand, 82 were elected into the corporation as associate members, and 106 ceased, from various causes, to belong to the class. The total number of students on March 31 last was 949, as against 926 at the same date in 1886. There were twenty-six ordinary meetings during the session, when twenty original communications were read and discussed. The Howard Quinquennial Prize had been adjudged to Dr. John Percy, in recognition of his researches on the uses and properties of iron. To the authors of some of the papers read and discussed at the ordinary meetings medals and premiums had been awarded, viz.: Telford Medals and Telford Premiums to Alexander B. W. Kennedy, Dr. J. Hopkinson, Colonel E. Maitland, and W. Willcocks; a Watt Medal and a Telford Premium to E. A. Clowes; Telford Premiums to W. J. Dibdin, W. S. Crimp, J. J. Webster, and J. Kyle; and the Manby Premium to L. H. Ransome. For papers printed in Section II. of the Proceedings, without having been publicly discussed, the following awards had been made: a Telford Medal and a Telford Premium to J. G. Gamble; a Watt Medal and a Telford Premium to W. J. Last, and Telford Premiums to J. Hetherington, K. W. Hedges, C. J. Wood, A. Leslie, and D. A. Stevenson. Twelve students' meetings had been held on alternate Friday evenings, at which thirteen papers were read and discussed.—The ballot for 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, Sir Douglas Fox, Mr. C. Hawksley, Mr. J. Mansergh, Mr. W. H. Preece, F.R.S., Sir Robert Rawlinson, 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 Council.

Chemical Society, June 2.—Mr. William Crookes, F.R.S., President, in the chair.—The following papers were read:—The equivalent of zinc, by Lieut.-Colonel Reynolds, late R.E., and Prof. W. Ramsay.—The magnetic rotation produced by chloral, chloral hydrate, and hydrated aldehyde, by Dr. W. H. Perkin, F.R.S.—Note on a new class of voltaic combinations in which oxidizable metals are replaced by alterable solutions, by Dr. C. R. Alder Wright and Mr. C. Thompson. It appeared to the authors probable that just as a liquid capable of parting with oxygen, chlorine, &c., can be used in conjunction with an electrode of unchangeable material at one side of a voltaic cell (as in Grove's nitric acid battery and analagous combinations), or may be replaced by a solid conducting electrode, itself capable of losing oxygen (*e.g.* a plate of strongly compressed peroxide of lead), so conversely might a conducting plate of oxidizable material (*e.g.* zinc) at the other side be replaced by an unchangeable electrode in conjunction with a liquid capable of taking up oxygen, chlorine, &c., without producing any fundamental change in the character of the actions

…ing place in the cell whilst generating a current. The …ctrode immersed in this oxidizable substance, like the zinc of … ordinary cell, would acquire the lower potential, and the …posed plate the higher potential; *i.e.* the wire connected …h the latter would be the "positive pole" of the construction …reference to the external circuit. On trial, it has been found …t such is the case, and that in consequence a large variety … novel forms of cell becomes easy of construction. For …ample, sodium sulphite or potassium ferrocyanide solution …posed to chromic-sulphuric acid solution; preferably with an …ermediate layer of some neutral salt solution, such as sodium …phate, to prevent the direct action of the two fluids on one …other. During the passage of a current, sodium sulphate or …tassium ferrocyanide is formed in quantity proportionate to … electricity passing, *i.e.* to the amount of silver thrown …wn in a silver voltameter included in the circuit; whilst …romium sulphate is produced at the other side Various …alogous cells are described, in particular one where lead oxide …solved in caustic soda is opposed to alkaline hypobromite: … this case lead dioxide is produced and separates out in the …id form; and one where chromium sesquioxide dissolved in …istic soda is opposed to chromium trioxide dissolved in sul…uric acid; here sodium chromate and chromium sulphate are …rmed, an E.M.F. about equal to that of a Daniell cell being … up.—The composition of Prussian blue and Turnbull's …ie, by Mr. Edgar F. Reynolds.—Phlorizin, by Prof. E. H. …ennie.—Further notes on the chemical action of *Bacterium …ti*, by Mr. Adrian J. Brown.—Note on the cellulose formed … *Bacterium xylinum*, by Mr. Adrian J. Brown.—The …idation of ethyl alcohol in the presence of turpentine, by Mr. … E. Steedman, Williamstown, Victoria.

Royal Microscopical Society, May 11.—Rev. Dr. …llinger, F.R.S., President, in the chair.—Mr. Crisp called …ention to a number of slides of hair which Dr. Ondaatje, of …ylon, had forwarded to the Society with a request for infor…ition as to its peculiarities of structure; also to a donation by …. Deby of sixty-two slides, chiefly of Micro-Hymenoptera, …ich came from the collection of the late Mr. F. Smith.—Mr. …Mayall, Jun., said that he took it for granted that the Fellows …re interested in whatever concerned the history of the micro…pe, and would therefore be glad of any new facts which …ded to throw light upon the subject. He had lately come …oss evidence which showed that magnifying glasses were used … least as early as 1513–20, for, in the celebrated portrait of …o X., by Raphael, the Pope is shown holding one in his hand. …e picture was painted between 1513 and 1520, as the Pope …s elected in 1513 and Raphael died in 1520. He brought to … meeting a large volume (lent for the purpose by Mr. …aritch) which contained an engraving of Raphael's portrait of …o X. During a recent visit to Florence he also paid some …ention to the microscopes which had been attributed to …lileo. It was of course rather difficult to say in such matters …at was really authentic and what was not. He could not, …wever, help noting that all the telescopes made in 1660, …about that time, had cardboard tubes, and wood or horn cells … the lenses, whereas these microscopes were made with sub…ntial brass body-tubes with strong and well-made screw …eads and firm tripod support. He could only say, therefore, …t if the microscope-makers had arrived at that stage of per…ion in Galileo's time, they had reached a point not attained …his successors until many years afterwards.—Mr. J. Mayall, …., also exhibited a microscope which had come from Japan. …was made after one of the old upright tripod models and had …ng of inlaid silver ornamentation at both top and bottom.— … Maddox's paper, on the different tissues found in the muscles …a mummy, was read.—Professor Bell gave an account of a …ent visit which he had paid to M. Pasteur's laboratory in …is.—Mr. Deby called attention to a series of double-stained …ions of the rare parasitical plant, *Brugmansia Lowii*, one of … Rafflesiæ, but differing in its being hermaphrodite. It grows … the overground roots of a species of Cissus, and was col…ed by him in 1884 in the Raritau range of mountains in …tral West Sumatra. The sections showed the development …ne plant from the time it begins to raise the bark of its host … minute tubercle up to the complete maturity of its ovules. … double staining allows of distinguishing between the tissues …ne parasite and of its host, which in unstained sections can…be determined. The formation of the locula of the ovary is … remarkable, and partakes more of a fungoid growth than …nerogamic.

PARIS.

Academy of Sciences, June 6.—M. Janssen in the chair. —Researches on the density of sulphurous acid in the state of liquid and of saturated vapour, by MM. L. Cailletet and E. Mathias. Having already described the method employed by them for determining the density of ethylene, of the protoxide of nitrogen, and of carbonic acid as liquids and saturated vapours, the authors here generalize their method by applying it to the study of a substance (sulphurous acid), whose critical point, approaching 156° C., is much higher than that of the former gases. Their researches show that the densities of the liquid and of the saturated vapour have a common limit, which is opposed to the conclusion arrived at by Avenarius; also that the critical density is 0·520.—Heats of combustion, by MM. Berthelot and Recoura. Continuing their studies of the heats of combustion by the new calorimetric method, the authors have determined the mean for glucose at 3·762 calories; for quinone, 6·102; for napthalene, 9·688; for benzoic acid, 6·345; and for salicylic acid, 5·326. These studies are being continued with a view to determining the heat of combustion of liquid and volatile bodies, and the measure of the heat of combustion of pure carbon in its various states. Notwithstanding its fundamental importance for calculating the heats of formation of organic compounds, this element has been neglected since the time of Favre and Silbermann.—Heats of combustion, by MM. Berthelot and Louguinine. Mean determinations are given for several compounds, such as napthalene, 9·6961 calories; phenol, 7·8105; benzoic acid, 6·3221; cuminic acid, 7·5533; quinone, 6·0613; hydroquinone, 6·2295; pyrogallol, 5·0262. —A new endless tape odograph, by M. Marey. The ingenious instrument here described has been prepared for the purpose of automatically recording the velocity of men walking or running with or without burdens, and under the varying conditions of level or inclined, smooth or rugged tracks, with or against the wind, and so forth. It is especially applicable for determining the marching capacity of troops, as well as the velocity of locomotives and other engines, of water and atmospheric currents.—Action of oil on troubled waters, by Admiral Cloué. The author has studied the results of over two hundred experiments, made especially in England and the United States, and concludes that the question is now definitely settled. There can no longer be any doubt that oil has a most efficacious effect in calming storm-tossed waters, and thus saving vessels in danger of foundering at sea. Fish oils appear to be the best, mineral oils owing to their lightness the least effective, but kitchen refuse of all sorts and similar substances floating compactly on the surface, tend to produce the same result.—On the character and results of the improved methods of amputation lately introduced into hospitals, by M. Trélat. The author's observations for the Charité and Necker Hospitals in Paris show that since 1880, when the antiseptic methods came into general use, the mortality under all kinds of amputations has fallen from 50 and upwards to an average of about 15 per cent. —On the density of the celestial vault, in relation to the radiant-points, by M. Alexis de Tillo. According to their right ascensions the 1315 radiant-points of the northern hemisphere are shown to be disposed in such a way as to make it evident that the regions traversed by the Milky Way (0°–90° and 270°–360°) have a perceptibly greater meteoric density than the others (90°–180° and 180°–270°) which lie mainly beyond that stellar zone.—On the melotrope, a new musical apparatus, by M. J. Carpentier. This instrument is intended to serve as a complement to the recently described melograph, the automatic records of which it faithfully reproduces on any piano. But it may also be so adjusted as to constitute itself an independent instrument suitable for the performance of automatic music generally.—Action of an electro-static field on a variable current, by M. Vaschy. It is shown that in a magnetic field of varying intensity a closed conductor placed in this field is traversed by induced currents, and in general there arises in each point of the space an electric force capable of being calculated. In other words, the variations of the magnetic field develop a true electrostatic field exercising a mechanical action on the electrified bodies. In virtue of the principle of equilibrium between action and reaction, the latter must react on the magnets or variable currents to which is due the magnetic field.—On the conductibility of abnormal salts and of acids in extended solution, by M. E. Bouty. The author's previous conclusion is here confirmed, that in respect of their conductibility these acids differ greatly from each other, not even excepting sulphuric, nitric, and hydrochloric acids; further, that these varying degrees of conductibility are not directly comparable with those of the neutral salts.—On cyanoacetic acid, by M. Louis Henry. These researches show, as anticipated, that the hydrogen element (CH_2) in this acid, $CN—CH_2—CO(OH)$, has a basic character; also that the acid itself may be obtained in well-defined and perfectly white crystals, and that it dissolves, not at 55° C., as indicated by Van't Hoff, but at 65°–66° C.—On the periodicity of magnetic perturbations and solar rotation, by M. Ch. V. Zenger. A comparative study of observations recorded at the Parc Saint-Maur and Paulovsk Observatories shows that the dates of magnetic perturbations largely coincide either with the days of the solar period or with those of the periodic shooting-stars. This coincidence is observed at points far distant from each other on the surface of the globe, and in years of least (1878) as well as of greatest solar activity (1883–84).

BERLIN.

Physiological Society, May 27.—Prof. Munk, President, in the chair.—Dr. Lœwy spoke on the respiratory centre in the medulla oblongata. His experiments were carried out on rabbits in the laboratory of Prof. Zuntz. He found that severing the medulla from the brain had no influence on either the frequency, depth, or rhythm of the respiration. On cutting one vagus in the animal operated upon as above, he observed a slight slowing of the respiratory movements; in order to produce any marked alteration of the respiration, he found it necessary to cut both vagi. After this operation the frequency of the movements was considerably lessened, the inspirations being very deep, while the expirations either did not take place at all, or were passive: in some few cases active expiration continued. The volume of the respired air was considerably diminished, while the rhythm was normal. By the above experiments it was shown that the centre in the medulla is able to maintain the rhythm of the respiratory movements after it is severed from both the brain and the peripheral parts of the vagi. Moreover the centre when thus isolated was found to be equally susceptible to stimuli, whether applied directly or arriving from the periphery, as when it was still connected with the brain and lungs. In one experiment after the medulla was separated from the brain and both vagi were divided, the spinal cord was cut through, and the muscles of the hind-limbs tetanized; this produced a quickening of the respiratory movements similar to that observed in normal animals, in accordance with the experiments of Zuntz and Geppert. (Muscular contractions lead to the formation of some product of their metabolism which has not yet been isolated, but which stimulates the respiratory centre when brought to it in the blood.) Similarly an excess of carbonic acid gas in the respired air had the same stimulating effect on the isolated respiratory centre as on the centre of normal animals. The irritability of the centre was not altered either qualitatively or quantitatively by its severance from the brain and lungs; thus equal percentage increments of carbonic acid gas in the respired air produced an equal increase of the respiratory movements in animals with isolated and unisolated respiratory centre. Dr. Lœwy has also endeavoured to find experimentally an answer to another important question connected with respiration. The vagus, as is well known, is the only nerve that is in a state of continuous stimulation. Hering and Breuer have explained this as the result of the distension of the lung-alveoli during respiration, which acts as a stimulus to the endings of the vagus in the lungs. But inasmuch as they found that this continuous stimulation of the vagus does not entirely disappear when the lungs are no longer distended, after making an incision into the thorax, they assumed the existence of other unknown factors to explain the phenomenon. Dr. Lœwy spoke against this view, pointing out that even in the collapsed lungs the alveoli are distended beyond their real size and that they are of normal size only in the atelectatic lung, and will then no longer stimulate the endings of the vagus. Experiments made by him confirmed this opinion: by occluding the bronchus of one lung, this lung became completely atelectatic, and then the vagus of the other side was severed. The immediate result of this was a considerable diminution in the frequency of the respiratory movements, greater in fact than is usually observed by section of only one vagus. Subsequent section of the vagus of the atelectatic lung produced no further effect on the respiration, thus showing that this vagus was not in a state of tonic stimulation.—Dr. Gad has carried on researches in his laboratory on the reaction-time for stimulation and inhibition. The experiments were made on the masseter muscle of ma[n]; the lower jaw was fixed so that the muscles antagonistic to th[e] masseter did not come into play, and the contraction or relax[a]tion of the muscle was graphically recorded on a Marey dru[m] by means of a specially constructed muscle forceps. The e[x]periments showed that as nearly as possible the same tim[e] elapsed between a given signal and the subsequent contracti[on] of the muscle as between the given signal and its relaxatio[n]. According to this, the will has an equally exact control of th[e] inhibitory as of the stimulatory process.—Dr. Benda recom[]mended the use of the kidney of mice for studying the structu[re] of the glomeruli, and demonstrated this structure on a series [of] preparations which he exhibited.

BOOKS, PAMPHLETS, and SERIALS RECEIVE[D]

A B C Five-Figure Logarithms: C. J. Woodward (Simpkin).—Elementa[ry] Microscopical Technology, Part 1: Dr. F. L. James (St. Louis).—Summa[ry] and Review of International Meteorological Observations, July to Decem[ber] 1885 (Washington).—Histoire des Sciences Mathématiques et Physiqu[es], tome xi.: MM. Marie (Gauthier-Villars, Paris).—A Dictionary of Ph[ilo]sophy: J. R. Thomson (Dickinsons).—Results of Observations of the Fix[ed] Stars made with the Meridian Circle at the Government Observatory, Madr[as], in the years 1862–63–64: N. R. Pogson (Madras).—Rustic Walking To[urs] in the London Vicinity: W. R. Evans (Philip).—Encyclopædia Britanni[ca], 9th ed., vol. xxii (Black, Edinburgh).—Ocean Birds: J. F. Green (Porte[r]).—Philips' Handy-Volume Atlas of the World (Philip).—Scala Natur[æ]: J. Cleland (Douglas, Edinburgh).—British Dogs, No. 8: H. Dalziel (Gi[ll]).—Annals of the Astronomical Observatory of Harvard College, vol. x[v]. The Almucantar: S. C. Chandler (Cambridge, Mass.).—Higher Algeb[ra]: Hall and Knight (Macmillan).—Beiträge zur Biologie der Pflanzen, Vier[ter] Band, Drittes Heft (Breslau).—Journal of the Liverpool Astronomi[cal] Society, vol. v.—Bulletin of the U.S. National Museum, No. 31: Synopsis of the North American Syrphidæ: S. W. Williston (Washingto[n]).

CONTENTS.

THURSDAY, JUNE 23, 1887.

THE AGRICULTURAL PESTS OF INDIA.

The Agricultural Pests of India and of Eastern and Southern Asia, Vegetable and Animal, Injurious to Man and his Products. By Surgeon-General Edward Balfour, Author of "The Cyclopædia of India," &c. (London: Bernard Quaritch, 1887.)

CONSIDERABLE attention has been directed lately to agricultural pests of all kinds, and especially to insect pests, in various countries, because the injuries occasioned to crops by their agency have greatly increased, and in some instances altogether new disorders and diseases attributable to them have appeared. The universal international exchange of agricultural produce and other commodities has tended and must tend to distribute insects, fungi, and other sources of evil to mankind, animals, and plants, throughout the world. Thus the terrible scourge of the vine, the *Phylloxera vastatrix*, was first introduced into the French vineyards with plants, or cuttings, of vines imported from the United States. Very many insects most noxious to agricultural, fruit, and garden crops, in the United States were brought there with plants, cuttings, fruits, and seeds. The elm-leaf beetle, *Galeruca xanthomelæma*, which is now seriously damaging elm-trees, was not known in the United States until 1837, and came probably from France, or Germany, where it had been a troublesome pest long before that date. The hop fly, *Aphis humuli*, called the "barometer of poverty" by a Kentish historian of hop culture, has only recently visited the hop plantations of America; yet it caused almost a total blight last year in those of the Eastern States, upon an area of nearly 40,000 acres. Without any doubt this insect was conveyed from England in "hop-sets." The Hessian fly has been conveyed to Great Britain by some means or other not yet discovered, during the last year, and bids fair to be a dangerous and permanent scourge to the wheat and oat crops of this country.

It is the same with moulds, or mildews, or "blights," occasioned by fungi. The vine mildew, *Oidium tuckerii*, was not dreamed of in France until 1845. The potato mould, *Peronospora infestans*, had shown no important sign in Great Britain until 1844. The coffee mildew, *Hemileia vastatrix*, did no serious harm in the coffee plantations of Ceylon until after 1870; but during the last ten years it has enormously decreased their yield.

Diseases of animals have also been greatly intensified during the past thirty years in Great Britain and in other countries. In India, as we gather from this little book of Surgeon-General Balfour, anthrax, pleuropneumonia, rinderpest, foot-and-mouth disease, are so rampant that the Madras Government has recently appointed an inspector of cattle diseases with a sufficient staff under him.

There is no doubt that the attacks of certain insects and parasitic fungi are more frequent and more fatal than formerly. Hop blights from aphides and mildew, *Spærotheca castagnei*, are far more common and destructive in England than they were fifty years back; and the orange-growers of Florida, California, and other places where oranges are cultivated, are at their wits' end to combat the ravages of scale insects, Coccidæ, which have greatly increased since 1870.

It is a moot point as to whether this is due, or not, to modern and more artificial systems of cultivation, which may be more favourable to the spread of insects and parasitic fungi. Or it may be that these new systems interfere with the balance of Nature by decreasing parasitic and other insects, and birds and other animals, which are the natural foes of injurious insects. It has been discovered by Prof. Forbes, of Illinois, that several species of the Carabidæ and Coccinellidæ eat the spores of fungi; therefore an unusual increase in the number of birds, or other foes of these insects, might occasion a serious spread of mildews.

The importance of the subject of agricultural pests cannot be overrated. It is now fully recognized by the Government of the United States, who have a distinguished entomologist upon the staff of the National Agricultural Department. Besides this, many of the States have their own entomologists, who furnish frequent and valuable reports and advice as to methods of treatment. In England the Agricultural Department of the Privy Council have lately issued a series of reports upon insects injurious to crops, written by Mr. Charles Whitehead; and Miss Ormerod, the entomologist of the Royal Agricultural Society, has published annual reports for upwards of ten years, which have been of the utmost value and practical benefit to agriculturists. And in India, as Surgeon-General Balfour tells us in this work, the serious injuries caused by insects and other animals, fungi, and bacilli, to mankind, animals, and plants, have at last attracted the attention of the Government of India, and it is proposed to invite communications from those engaged in agriculture, forestry, and horticulture in that country, to furnish matter for periodical reports like those issued from time to time by Miss Ormerod. These would of course be published in the vernacular, and should be illustrated by woodcuts, as Miss Ormerod suggests in her comprehensive letter in the preface of "Agricultural Pests of India." It is much to be hoped that a competent entomologist may be appointed in India to direct this work.

Surgeon-General Balfour, so far back as 1880, recommended the Secretary of State for India to obtain reports on the diseases of cattle and plants, and on creatures noxious to mankind and vegetation. In his admirable "Cyclopædia of India and of Eastern and Southern Asia," published in 1885, he gave a general view of the entomology of these regions, and described the losses sustained by agriculturists from these and similar causes. He has followed this up with the work now under review.

Though a small book, the "Agricultural Pests of India" is very ambitious in design, as it treats not only of insects and fungi and animals injurious to mankind and agricultural crops, but of all manner of birds, beasts, and fishes. Several of these cannot, even by the greatest stretch of the imagination, be classified as pests to agriculture, and seem to be altogether out of place in this category. Under the heading "Fish," sharks and siluroids are described, though it is not by any means clear in what way they are agricultural pests, except, perhaps, that they might bite off the limbs of unwary agriculturists disporting in the sea. The book should have been styled the "Natural History of India," or "A Manual of the Natural History of India," rather than the "Agricultural Pests of India." But the fact that rather too many subjects are dealt with cannot be held to be a very serious fault in a compilation containing an immense amount of serviceable information arranged alphabetically, together with a good index, so that any head can be quickly found. The author had great opportunities of acquiring knowledge of the branches of natural history he has here discussed while he was engaged in forming the Government Central Museum at Madras, and other museums in various parts of India, as well as in the preparation of "The Cyclopædia of India" and his work on "The Timber Trees of India." He was therefore very well qualified to prepare this manual or dictionary of natural history, which will serve to show Indian agriculturists what are the principal foes of their crops and herds. No remedies or methods of prevention are given in detail. Some general instructions appear in the introductory chapters, such as to farm cleanly, and to use certain washes and powders in case of the attack of some insects. These, however, have evidently been taken from lists of remedies prescribed by American and English practical entomologists, and have not been actually tried in India. Now that Surgeon-General Balfour has demonstrated the dangers, and indicated general remedies which have been found advantageous in other climes, the farmers, the foresters, and fruit-growers of India should at once make experiments, and prove for themselves whether these are as efficacious in the fiery heat of the East as in the temperate climates of Great Britain and America.

This notice cannot be concluded without an allusion to some of the errors which have been carelessly allowed to remain in the book, having evidently escaped the notice of the eminent scientific man who "revised nearly the whole in manuscript, and the proofs as they passed through the press." It is not to be expected that Surgeon-General Balfour should be a skilled entomologist, but it is very unfortunate for him that those on whom he relied for assistance should have so signally failed him. He says that the *Cecidomyia tritici* is the Hessian fly of Europe and America. In reality the Hessian fly of Europe and America is *Cecidomyia destructor*, named so by Say long ago, and is completely and specifically distinct from *Cecidomyia tritici*, which is the true wheat midge of Great Britain. This is a mistake which appears unpardonable in a scientific reviser. On p. 45 it is stated that "the species of Necrophorus and Silpha are useful; they feed on carrion, and by scratching the ground from under dead animals they partially bury them." As a fact the *Silpha opaca*, and another species, the *Silpha atrata*, eat and seriously injure plants of beet and mangel-wurzel, as has been shown by Curtis and Miss Ormerod in England, by Guérin Méneville in France, and Taschenberg in Germany. It need hardly be said that correct information as to the habits of insects is as necessary as accurate nomenclature—at least to agriculturists.

Again, under the heading Buprestidæ and Elateridæ (click beetles) it is remarked that the larvæ feed on living wood, and are more or less injurious. The wire-worm, the larva of *Elater lineatus*, is fearfully destructive to the roots of crops of all kinds. In the description of Elateridæ, further on, this kind of mischief is attributed to their larvæ; so that there are two utterly conflicting accounts of the habits of these insects, calculated to puzzle the inquiring Indian farmers.

A sweeping statement that "all the weevil family insert their eggs in the stigma of the flower" cannot be supported, and is utterly opposed to the experience of observers. A few species do this, but others deposit their eggs in a variety of places. Of weevils it is also said that they "attack principally in their larval stage every part of vegetable tissues." As a fact, many weevils do incredible harm to vegetation in their perfect or weevil form, and it would be difficult for the larvæ—mere maggots—to hold on to leaves.

Sitonas, described as attacking stored grain and seed, have been evidently mistaken for species of Bruchi.

These and other mistakes ought to be corrected before the work is put into the hands of the agriculturists of India as a text-book for their guidance.

CELL-DIVISION IN ANIMALS.

La Cytodiérèse chez les Animaux: Étude comparée du Noyau et du Protoplasme. Par T. B. Carnoy, Professor of Biology in the University of Louvain. (Louvain: A. Peeters, 1886.)

IN this work the learned biologist of Louvain has ably resumed and discussed the latest researches made concerning the phenomena of cell-division in arthropods and worms. It is, of course, impossible in a short article to do justice to the great labour and admirable patience here displayed by the distinguished author; nor can we discuss as fully as the subject deserves the several points on which Dr. Carnoy appears to differ essentially from other workers in the same field, as, for instance, from Prof. E. Van Beneden and from Mr. Nussbaum. But the questions raised by the Louvain Professor are of such importance that even a summary of his present views cannot fail to be of interest.

First, as regards cell-division in arthropods, Prof. Carnoy maintains that in them the direct mode of division may be observed in various tissues, young and adult, and must be admitted to have all the characters of what he terms "un processus normal."

This direct mode occurs either by "étranglement" or by the help of a partition, just as in vegetable cells, and this is verified for the protoplasm itself as well as for the nucleus.

Then, contrasting the direct with the indirect or karyokinetic mode of cell-division, he remarks that those two processes have in reality the same morphological significance and physiological value; that the characters of karyokinesis are inconstant, and that they may often be seen passing through many intermediate stages into the characters proper to the akinetic mode. Nevertheless, our author admits that karyokinesis is of considerable importance to cell-life, inasmuch as it affords an easier and surer method for making the cell dicentric; it also leads to the division of the nuclear element into two equal parts; it enriches the protoplasm with plastine; and, lastly, it renders possible the total regeneration of the nucleus. In the present state of our knowledge, however, there is obviously much that is hypothetical in the respective importance of these consequences.

It is chiefly in his researches on the embryology of Nematoda that Prof. Carnoy has reached conclusions which are totally at variance with those already arrived at on the same subject by Messrs. Nussbaum and E. Van Beneden. We allude especially to the mode of formation of the polar bodies in the egg of *Ascaris megalocephala*. For the Louvain Professor, the two successive divisions which take place in the germinal vesicle assume the following characters:—

(1) The nuclear element ("élément nucléinien typique") of the egg of *Ascaris megalocephala* becomes at an early stage broken up into eight nearly equal rod-like portions; these at once separate into two groups of four rods ("bâtonnets"), thus constituting the Wagnerian spots.

(2) When a spermatozoid has made its way into the egg, sometimes very soon afterwards, occasionally later, an alteration of the germinal vesicle becomes visible; its membrane dissolves away, and subsequently, by a process of true karyokinetic division accompanied by the formation of asters of remarkable variety and complexity, the first polar body is expelled. This he finds to consist of four nuclear rods and a portion of the protoplasm of the egg. At this stage, therefore, according to Dr. Carnoy, four rods only remain within the egg.

(3) Now the same process begins again, in all essential respects resembling that which has just been described; finally, the second polar body is expelled in its turn. It consists of two nuclear rods, so that only two rods remain now in the egg for the formation of the female pronucleus. We are thus in a position to calculate accurately the amount of nuclein lost by the germinal vesicle during the expulsion of the polar bodies. According to Prof. Carnoy, the loss, for *Ascaris megalocephala*, would amount exactly to three-fourths of the nuclein originally present in the egg.

We are not sure whether Prof. E. Van Beneden's views on this delicate question may not be to a certain extent reconciled with those of the eminent biologist of Louvain, especially as regards the number of nuclear portions contained in the first polar body. But respecting the constitution of the second polar body the views of the two Belgian observers are certainly difficult, if not impossible, to reconcile.

Prof. Carnoy's book reads easily, and his statements are always clear and definite. The text is illustrated by a large number of figures, beautifully executed, which greatly enhance the value of this most interesting and important work.

L. MARTIAL KLEIN.

OUR BOOK SHELF.

The Climatic Treatment of Consumption: a Contribution to Medical Climatology. By J. A. Lindsay, M.A., M.D. (London: Macmillan and Co., 1887.)

DR. LINDSAY does not profess to have written a systematic and exhaustive treatise upon the climatic treatment of consumption. He holds that we are only on the threshold of climatological investigation: and "for its exhaustive discussion," he says, "prolonged inquiry will be necessary, and more exact methods than those hitherto generally employed." He has made, however, an important contribution to the study of a very difficult subject, and his book ought to be of much service not only to physicians but to many sufferers who may still hope to find in climatic treatment a powerful adjunct to hygienic and medical measures. Having discussed the causes of consumption and the general principles of climatic treatment, Dr. Lindsay presents a general view of the chief sanatoria for consumption. He then describes mountain sanatoria and the ocean voyage, and gives a full and trustworthy account of sanatoria he himself has visited, including Australia, Tasmania, New Zealand, California, the Cape, Algeria, Southern France, and the home sanatoria. The value of the book is, of course, greatly increased by the fact that he has relied for his information mainly on personal observation.

Illustrations of the British Flora. Drawn by W. H. Fitch, F.L.S., and W. G. Smith, F.L.S. Second Edition. (London: L. Reeve and Co., 1887.)

WHEN the illustrated edition of Bentham's "Hand-book of the British Flora" was exhausted, the wood engravings of that work were reproduced in a volume intended to serve as a companion to the "Hand-book" and other British Floras. The volume has been so popular that the publishers have found it necessary to issue a second edition; and they have taken pains to secure that it shall be more useful than ever to students of botany, and especially to beginners. Five cuts have been added, and the arrangement of all the illustrations has been brought into accordance with Bentham's "Hand-book" as it has been revised by Sir J. D. Hooker. To facilitate reference from other Floras, the index has been greatly enlarged, and there is a new index of English and popular names.

Sketches of Life in Japan. By Major Henry Knollys, R.A. With Illustrations. (London: Chapman and Hall, 1887.)

IN this book Major Knollys undertakes to tell us something of "the minor lights and shades" of the social life of Japan. He is a careful observer, and writes brightly and pleasantly; and no doubt the lively record of his impressions will interest a good many readers who would not have cared to study a more elaborate and systematic account of the Japanese people. The substance of the book was written "on the spot," but all statements with regard to matters of fact have been carefully revised.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Thought without Words.

THERE appears to be some ambiguity about this matter as discussed in the correspondence which has recently taken place in your columns. In the first instance Mr. Galton understood Prof. Max Müller to have argued that in no individual human mind can any process of thought be ever conducted without the mental rehearsal of words, or the *verbum mentale* of the Schoolmen. Now, although this is the view which certainly appears to pervade the Professor's work on "The Science of Thought," there is one passage in that work, and several passages in his subsequent correspondence with Mr. Galton, which express quite a different view—namely, that when a definite structure of conceptual ideation has been built up by the aid of words, it may afterwards persist independently of such aid; the scaffolding was required for the original construction of the edifice, but not for its subsequent stability. That these two views are widely different may be shown by taking any one of the illustrations from the NATURE correspondence. In answer to Mr. Galton, Prof. Max Müller says, "It is quite possible that you may *teach* deaf-and-dumb people dominoes; but deaf-and-dumb people, left to themselves, do not *invent* dominoes, and that makes a great difference. Even so simple a game as dominoes would be impossible without names and their underlying concepts." Now, assuredly it does "make a great difference" whether we are supporting the view that dominoes could not be *played* without names underlying concepts, or the view that without such means dominoes could not have been *invented.* That there cannot be concepts without names is a well-recognized doctrine of psychology, and that dominoes could not have been invented in the absence of certain simple concepts relating to number no one could well dispute. But when the game has been invented, there is no need to fall back upon names and concepts as a preliminary to each move, or for the player to predicate to himself before each move that the number he lays down corresponds with the number to which he joins it. The late Dr. Carpenter assured me that he had personally investigated the case of a performing dog which was exhibited many years ago as a domino-player, and had fully satisfied himself that the animal's skill in this respect was genuine—*i.e.* not dependent on any code of signals from the showman. This, therefore, is a better case than that of the deaf-mute, in order to show that dominoes can be played by means of sensuous association alone. But my point now is that two distinct questions have been raised in your columns, and that the ambiguity to which I have referred appears to have arisen from a failure to distinguish between them. Every living psychologist will doubtless agree with Prof. Max Müller where he appears to say nothing more than that if there had never been any names there could never have been any concepts; but this is a widely different thing from saying what he elsewhere appears to say, *i.e.* that without the mental rehearsal of words there cannot be performed in any case a process of distinctively human thought. The first of these two widely different questions may be dismissed, as one concerning which no difference of opinion is likely to arise. Touching the second, if the Professor does not mean what I have said he appears in some places to say, it is a pity that he should attempt to defend such a position as that chess, for instance, cannot be played unless the player "deals all the time with thought-words and word-thoughts." For the original learning of the game it was necessary that the powers of the various pieces should have been explained to him by means of words; but when this knowledge was thus gained, it was no longer needful that before making any particular move he should mentally state the powers of all the pieces concerned, or predicate to himself the various possibilities which the move might involve. All these things he does by his specially-formed associations alone, just as does a draught-player, who is concerned with a much simpler order of relations: in neither case is any demand made upon the *verbum mentale.*

Again, if the Professor does not mean to uphold the view that in no case can there be distinctively human thought without the immediate and direct assistance of words, it is a mistake in him to represent "the dependence of thought on language" as absolute.[1] The full powers of conceptual ideation which belong to any individual man may or may not all have been due to words as used by his ancestors, his contemporaries, and himself. But, however this may be, that these powers, when once attained, may afterwards continue operative without the use of words is not a matter of mere opinion based on one's own personal introspection, which no opponent can verify: it is a matter of objectively demonstrable fact, which no opponent can gainsay. For when a man is suddenly afflicted with aphasia he does not forthwith become as the thoughtless brute: he has lost all trace of words, but his reason may remain unimpaired.

June 4. GEORGE J. ROMANES.

[1] *e.g.* "I hope I have thus answered everything that has been or that can possibly be adduced against what I call the fundamental tenet that the science of language, and what ought to become the fundamental tenet of the science of thought, namely that language and thought, though distinguishable, are inseparable, that no one truly thinks who does not speak, and that no one truly speaks who does not think."—"Science of Thought," pp. 63–64.

I HAVE postponed offering you any remarks on Prof. Max Müller's "Science of Thought," until I had read the book through.

I think Prof. Müller is on the whole right, that language is necessary to thought, and is related to thought very much as organization to life. The question discussed by some of your correspondents, whether it is possible in particular cases to think without language, appears to me of little importance. I can believe that it is possible to think without words when the subjects of thought are visible things and their combinations, as in inventing machinery; but the intellectual power that invents machinery has been matured by the use of language.

But Prof. Müller has not answered, nor has he asked, the question, on what property or power of thought the production of language depends. He has shown most clearly the important truth that all names are abstract—that to invent a name which denotes an indefinite number of objects is a result of abstraction. But on what does the power of abstraction depend? I believe it depends on the power of directing thought at will. Prof. Müller lays stress on the distinction between percepts and concepts, though he thinks they are inseparable. I am inclined to differ from him, and to think that animals perceive as vividly as we do, but have only a rudimentary power of conception and thought. I think the power of directing thought at will is the distinctively human power, on which the power of forming concepts and language depends. JOSEPH JOHN MURPHY.

Belfast, June 19.

AFTER reading the correspondence published in NATURE (vol. xxxvi. pp. 28, 52, and 100) on this subject, it has occurred to me that the difficulties anthropologists find in Prof. Max Müller's theory are connected chiefly with his peculiar definitions.

In his letters to Mr. Galton, Prof. Müller narrows the domain of his theory to a considerable extent. By defining thought as the faculty of "addition and subtraction," and by taking language as composed of "word-thoughts" or "thought-words," Prof. Müller excludes from his theory all those processes which are preliminary to the formation of concepts. Thus narrowed, I do not see that his doctrine in any way touches the wider question, whether reasoning, as generally understood, is independent of language. If we keep to the terms of this theory, thoughts and words are undoubtedly inseparable. But this does not in the least imply that all thought is impossible without words.

When we enlarge the scope of our terms, it is at once evident that thoughts and words are not inseparable. It is all very well to join together "thought-word" and "word-thought." Yet the thought is something quite distinct from the mere sound which stands as a word for it. A concept is formed from sensations. Our thoughts are occupied with what we see, and feel, and hear, and this primarily. Thus it is that, in the wider sense of thinking, we can think in pictures. This is the mental experience which Prof. Tyndall so highly prizes. He likes to picture an imaginary process, not in words, not even by keeping words in the background, but in a mental presentation of the things themselves as they would affect his senses. Surely, then, if the mind can attend to its own reproduction of former sensations, and even form new arrangements of sensations for itself quite irrespective of word-signs, as Mr. Galton and most other thinkers have experienced, it is evident that thought and language are not inseparable.

All this is, of course, somewhat apart from Prof. Müller's restricted theory. But the question follows, how from these wider thoughts do we become possessed of the faculty of abstraction. Does not the one shade imperceptibly into the other? Prof. Müller answers no, and here I think he is at fault. It is at this point that anthropologists part company with him. If he be right, how do people learn? According to his theory, new thoughts when they arise, start into being under some general concept. I do not deny that they are placed under some general concept, but it seems to me that something entirely independent of the general concept has, for convenience, been placed under it, and this something must be called a thought. No doubt the thought is at first vague and indefinite, and only when it becomes definite does it require a name. But here one can plainly trace the genesis of a thought, and the adapation of a word as a symbol for it. The new concept and its sign do not arise simultaneously. There are two distinct growths, not one only, as Prof. Müller's theory presupposes. The connexion may be subtle and close, but the two elements can be easily separated. It avails nothing to say that until the thought is placed under a concept, it is not a thought. This is a mere question of definition, not of actual fact.

I would point out one other consideration. If Prof. Müller's theory were true for all kinds of thinking, development would be impossible. If man could not think without language, and could not have language without thinking, he would never have had either, except by a miracle. And scientific men will not accept the alternative. We can conceive shadowy thoughts gradually shaping to themselves a language for expression, and we can understand how each would improve the other, until by constant interaction, a higher process of thought was introduced. But we cannot conceive the sudden appearance of the faculty of abstraction together with its ready-made signs or words.

I have often wished that Prof. Müller would state distinctly how his theory accounts for the very first beginnings of language. I have not been able to discover any explanation of this point in his "Lectures on the Science of Language."

Clapham, June 6. ARTHUR EBBELS.

As poets have extraordinary inklings and *aperçus* on the most abstruse scientific questions, Wordsworth's opinion on this matter (quoted by De Quincy) is worth considering: Language is not the "*dress*" of thought, it is the "*incarnation.*" This is Shelley's *aperçu* of Darwinism. Man exists "but in the future and the past; being, not what he is, but what he has been and shall be."

How to "distil working ideas from the obscurest poems"—to use Lord Acton's words—is one of the secrets of genius.

A. GRENFELL.

THE interesting discussion between Mr. Francis Galton and Prof. Max Müller on this subject will doubtless raise many questions in the minds of those who have paid some attention to the habits of animals. I have been asking myself whether, if Prof. Max Müller is right in his conclusion—"Of course we all admit that without a name we cannot really know anything" (an *utterable* name, I presume), and "one fact remains, animals have no language"—animals must not, therefore, be held by him incapable of knowing anything. This would bring us to the question whether animals *know* in the same manner as men, or in some other manner which men do not understand. Now, I think—at least it is as strong a conviction as I am capable of entertaining—that animals not only know, but deal with the materials of knowledge—facts—in a manner quite indistinguishable from the manner in which I mentally handle them myself. Thus, I place an animal in circumstances which are quite unfamiliar to it, and from which it is urgently pressed to escape. There are two, or perhaps three, courses open to it; one being, to my mind, patently the most advantageous. It tries all of them, and selects that which I should have chosen myself, though it is much longer in coming to its conclusion. Here the animal has the same facts as the man to deal with, and, after consideration and examination, its judgment precisely corresponds with the man's. I cannot, then, find it possible to deny that the mental operations are identical in *kind;* but that they are not so in *degree* can be demonstrated by my importing into the situation an element foreign to the experience of the animal, when its failure is certain. It makes no difference whether the animal is under stress, or acting voluntarily. It may frequently be found to choose the method which most recommends itself to the man's judgment. Every student of animals is familiar with numbers of such cases. Indeed they are constantly being recorded in the columns of NATURE, and abound in all accepted works on animal intelligence. I am quite prepared to admit that where there are two or more courses open to it the animal will occasionally select that which presents the greatest difficulties, and labour most assiduously to overcome them, sometimes trying the remaining courses and returning to that which it first chose. Darwin gives a good example of the honey-bee ("Origin of Species," p. 225, edition 1872). But no one will be surprised at imperfect judgment or vacillation of will in an animal, when such are common among men.

Prof. Max Müller lays down the very distinct proposition that "animals have no language." I suppose *utterable* language is meant. Is this so? That their sign-language is both extensive and exact (and even understood to some extent as between widely different species) most naturalists, I apprehend, will entertain no doubt. But has any species an utterable language? What is to be the test of this? First there is the whole gamut of vocal expressions—which even we understand—conveying the ideas of pain, pleasure, anger, warning. What sportsman who has stalked extremely shy animals does not know the moment a bird or animal utters a certain note that he is discovered? If Prof. Max Müller will not admit this to be language, I, for one, must ask him what it is. It conveys to others a distinct idea, in general if not in special terms, and seems to me quite equivalent to "Oh, dear!" "This is nice" (expressed, I believe, in some African language by the reduplicated form *ñum-ñum*, the letter *n* having the same value as in the Spanish *mañana*), "Leave off," "Look out," "Come here," &c. Those who have heard animals calling to one another, particularly at night, and have carefully noted the modulations of their voices (why should there be modulations unless they have a definite value), will find it very hard to accept Prof. Max Müller's conclusion that "animals have no language." Every female mammal endowed with any kind of voice has the power of saying "Come here, my child," and it is an interesting fact beyond question that the knowledge of this call is feebly or not at all inherited, but must be impressed upon the young individual by experience. Further, the young brought up by an alien foster-mother pay no attention to the "Come here, my child," of the alien species. The clucking of the hen meets with no response from the ducklings she has reared, even when she paces frantically by the side of the pond imploring them not to commit suicide. But let us creep up under the banks of a sedgy pool at about this time of year. There swims a wild duck surrounded by her brood, dashing here and there at the rising *Phryganidæ*. Now let the frightful face of man peer through the sedges. A sharp "quack" from the duck, and her brood dive like stones, or plunge into the reeds. She, at least, knows what to say to them.

The already inordinate length of this letter precludes me from offering any instances of the communication of *specific* intelligence by means of the vocal organs of animals. I think it probable that we far under-rate the vocabulary of animals from deficient attention—and, I speak for myself, stupidity. Possibly Prof. Max Müller has not yet examined "Sally," the black chimpanzee. If not, he would surely be much interested. She is by no means garrulous, but, in spite of her poor vocal capacity, if he should still consider that she "cannot really know anything" on that account, I must have completely misinterpreted his letter to Mr. Galton. ARTHUR NICOLS.

Watford, June 3.

Two Friends.

THE remarks on the reasoning powers of animals (dogs in particular) given in your issue of June 9 (p. 124) induce me to relate an experience of my own. We possess a dog and a cat, both males, the former called Griffon here, much like a Skye terrier, the latter a splendid animal (a cross of the Angora). These two animals are bound to each other by the closest friendship, which began thus:—The dog came to us two years ago, quite a pup—about three months old. Soon after a small, wretched, half-starved kitten arrived at our door asking hospitality. The dog at once adopted it, let it eat out of the same dish, let it sleep on the same mat (and continues to do so still), in fact took entire charge of it. A black cat, a very vicious creature, and seemingly wild, haunted our garden, to the great destruction of birds' nests and to the excessive terror of the kitten. As the dog grew, it became the kitten's protector against the black cat, and has been so now for two years. If it was indoors and heard a cry of distress from our cat, you could not hold it from flying wildly to its rescue, forcing someone to open the door, or darting through a window. It has done this so long, and with such effect, that the black cat scarcely dares show its face in the garden, as the dog invariably attacks it with fury and drives it away, following it along the road to see if it is quite gone. I do not know if you will think this worthy of insertion, but I think it curious, and I can vouch for its truth. M. C.

La Tour de Peily, June 13.

The Use of Flowers by Birds.

As a curious incident enacted by sparrows has just come under my notice, which possesses some added interest in connexion with the two occurrences recorded by your correspondent

J. M. H.—viz. the employment by some finches of flowers in the formation of their nests (NATURE, vol. xvi. p. 83, and vol. xxxvi. pp. 101–2)—it may be worth while to submit a detailed consideration of the case.

The front of the house of a friend living at No 47 Highbury Hill is covered by an extensive growth of white jasmine which reaches beyond the first-floor windows. For several years house-sparrows have used the bushy branches of this shrub without causing special attention. This year, however, they have taken a new departure in nest-building. Not satisfied, apparently, with the hay, straw, and other ordinary materials of sparrow architecture, they have suddenly aspired to appropriating to their use the bright yellow flowers of laburnum, two trees of which are in full bloom a few yards from the first-floor window-sills below which they are carrying on their operations. Three nests were discovered twelve days ago, built close together in the jasmine, all of which had laburnum flowers strewn upon the top of ordinary nests; one nest contained two young birds just hatched, and the other two had each a couple of eggs. As they rather disturbed the lady occupant of the house, she had all three nests destroyed, the litter from them entirely filling a large foot-bath. But the three pairs of birds, as might be expected, only set to work rebuilding their nests in the same place, furnishing them with more laburnum than before. They were however again disturbed, and an obstacle (which in a previous year had proved effectual in stopping the building in another part of the house front) was set in the place of the nests, but still they did not desist; two pairs continued to add their materials on the top of it, with more laburnum than ever, replenishing the nest as constantly as it was removed, while the third pair rebuilt their nest under the sill of the next window, using laburnum also. Even entire sprays of the flowers were used, and the ground beneath the trees was so much strewn with fragments that my friend at first thought that boys had been pulling the trees. All the birds are now allowed to remain unmolested, and the yellow decoration is withered, without fresh being provided.

This unaccustomed action of the sparrows is apparently somewhat different from the operations described by your correspondent J. M. H., for the bright golden flowers enveloping the nests are so strangely conspicuous as to attract the attention of passers-by, and therefore cannot answer the protective purpose evident both in the case of the goldfinches with forget-me-nots and of the sparrows that used Alyssum. The only explanation I can suggest is that the birds have elevated their æsthetic taste to this "quite too too" extent of art cultus. It is highly interesting to note also that—in opposition to the notions of the obsolete school of naturalists, who believed only in blind instinct—the rage for collecting their favourite "yellow" is infectious with these little yearners for the intense, just as is the desire for "blue" that now and then breaks out (like a disease) amongst larger householders. The three pairs of birds seemed to vie with one another in their revelry of the chosen colour. It will be instructive to learn whether the fashion will last for many seasons: perhaps it will languish of satiety, and some other attraction of a less absorbing kind arise.

The fondness of birds (in this country at least), for the colour yellow is perhaps worth considering in this connexion. A large number of wild or cultivated plants might be enumerated that produce yellow flowers, which are either used as food or have their petals mauled by birds. There need be no doubt, I think, that the mutilation of such flowers is due to a playful fondness rather than to a dislike of the flowers. That birds evidently exercise the selective faculty in the choice of flowers is well illustrated by the fact, twice observed by my brother, that sparrows pull to pieces the yellow flowers only in mixed beds of pansies, and of crocuses, without injuring a single purple, mauve, or white flower of either kind. I have myself also witnessed the same selective operation performed by a sparrow on various crocuses growing in pots upon my window-sill, and I find many correspondents gave similar testimony to this fact in a series of letters which appeared in these pages in the year 1877 (vols. xvi. and xvii.). It may be questioned whether the education of their preference for the colour yellow is in any way connected with the fact that it is proper to the yolk of their eggs, and which they must be aware of; but since all good eggs contain that colour, while probably some birds do not like it and greatly prefer other colours, this suggestion may be no more valid as a theory than would be the argument that some people's taste for claret-colour is due to the analogous physiological accident of arterial coloration. The rich yellow colour, again, of the beaks, entire mouths, and "open sepulchres" of the newly-hatched nestlings affords their parents ample opportunities for the contemplation of colour, and there may be an unconscious mental absorption of the colour in consequence of this course of training. At any rate, canary yellow is very highly developed in many species of the *Fringillidæ*, and there is a strong tendency towards the development of the yellowish colour in the plumage of the males of several British finches, apparently through a greenish-brown tinge. It is also well developed amongst the weavers and the orioles, to which they are so nearly allied. That sparrows should thus use sprays of flowers is perhaps not so remarkable when we recall the close affinity they bear to *Ploceus* and other weaver-birds.

Doubtless the colour-sense in birds, as well as in insects, is a real factor in the evolution of the floral beauty that surrounds them, although the *modus operandi* is not always one that can be so readily traced. WILLIAM WHITE.

55 Highbury Hill, London, June 9.

Names for Electric Units of Self-Induction and Conductivity.

A NAME seems to be wanted for the practical unit of self-induction, viz. an ohm multiplied by a second; in other words, for a length approximately equal to an earth-quadrant. Profs. Ayrton and Perry call it a "secohm." Why not call it a "quad"? It would be a handy great length for many other purposes. For instance, the velocity of light in air would be 30 quads per second, in common glass 20 quads per second.

To avoid misunderstanding, it would have to be understood that the actual earth-quadrant passing through any given place is only approximately a quad, its real value having to be determined geodetically. A quad is to be understood as ten million metres *precisely*.

Another unit requiring a name is the unit of conductivity. Sir William Thomson has suggested the word "mho," but it has not been greedily assimilated. I make the small suggestion of omitting the *h*. True, the expressions 12 mo and 16 mo would at first excite only bookbinding ideas, but they would soon carry a fresh meaning to electricians. OLIVER J. LODGE.

June 13.

Units of Weight, Mass, and Force.

THE necessity for *names* for the units of velocity and acceleration is very clearly illustrated by a criticism of my "Dynamics for Beginners," which appears in the *Practical Engineer* of June 3. After objecting to the introduction of new names, and explaining that *a velo* stands for *a foot per second*, the writer proceeds:—"The second new name is 'celo,' and is meant for an acceleration of *one foot per second*, or unit acceleration; so that if a body is moving with a velocity which is being accelerated *at the rate of one foot per second*, it is said in the new language to possess one celo. In other words, a celo means *an acceleration of one foot per second*, or *of one velo*." The italics are mine. I cannot resist quoting also the following sentence, which occurs a little lower down in the same criticism:—"We think there is something ridiculous about the adoption of these names, which, while possessing the very questionable advantage of shortening the language of the subject by some two or three words, *serve to muddle the mind of the student, and to obscure the sense by wrapping it up in meaningless words.*"

Why is not the *Practical Engineer* consistent? He ought to state that just as a celo is unnecessary, for he considers it the same as a velo, in like manner a velo is unnecessary, for by the same line of argument it must be the same as a *foot*. The fact is that the names velo and celo are not *necessary* for scientific men, although I expect they will be found to be convenient. It is, I believe, generally admitted that some such words are greatly needed by teachers; for it is the clear mental differentiation of the ideas expressed by velo and celo, or the want of it, which often marks the distinction between a sound physicist and a muddler. JOHN B. LOCK.

Gonville and Caius College, June 4.

I AGREE with your correspondent, Mr. R. B. Hayward (NATURE, vol. xxxv. p. 604), in holding that names for the dynamical units are of less importance than a convenient notation for them. To invent names for the pound-foot-second units may be helpful to beginners; but it is a small matter compared with a notation which completely specifies the mode of dependence of each unit upon the pound, foot, and second; and it is still more so when compared with a general notation which will serve for any system of units.

The difference between names and notation is well seen in the case of chemistry. The notation for a substance expresses the manner in which the substance is made up of the elementary substances; while its name, however derived, serves merely as a distinguishing mark: and just as the chemical notation for a substance may be used as a name for the substance, so the notation for a physical unit may serve as a name for that unit.

In my work on "Physical Arithmetic," published by Macmillan and Co. in 1885, and reviewed in NATURE, vol. xxxi. p. 551, I have devised a notation which is the natural and legitimate extension of existing conventions both in language and in the mathematics; and I have made that notation the basis of a method for solving problems in applied arithmetic. If the Committee of the Association for the Improvement of Geometrical Teaching are considering the subject, I ask them to consider whether any notation more in harmony with existing conventions can be devised than the notation of that work.

As a specimen I append the general notation for the chief geometrical, kinematical, and dynamical units. The word *by* corresponds to $\times$, and the word *per* to $\div$, or / as now frequently used by physicists. The same method of notation applies to the thermal and electrical units. The notation for any special system is obtained by substituting the special names of the fundamental units **L**, **M**, **T**. The test of the value of a notation is the amount of facility it offers in reasoning; by referring to "Physical Arithmetic," anyone may see how this notation stands the test.

NOTATION FOR GENERAL UNITS.

Quantity.	Notation.	Dimensions.
	I. *Geometrical.*	
Length	**L**	l
Surface	**L** by **L** = **S**	l^2
Volume	**L** by **L** by **L** = **V**	l^3
Angle	**L** arc per **L** radius	l^0
Sine	**L** opposite per **L** along	l^0
Curvature	Radian per **L** arc	l^{-1}
	II. *Kinematical.*	
Time	**T**	t
Velocity	**L** per **T**	lt^{-1}
Acceleration	**L** per **T** per **T**	lt^{-2}
Angular velocity	**L** arc per **L** radius per **T**	t^{-1}
	III. *Dynamical.*	
Mass	**M**	m
Density	**M** per **V**	ml^{-3}
Mass-vector	**M** by **L**	ml
Momentum	**M** by **L** per **T**	mlt^{-1}
Force	**M** by **L** per **T** per **T** = **F**	mlt^{-2}
Pressure	**F** per **S**	$ml^{-1}t^{-2}$
Work	**F** by **L** = **W**	ml^2t^{-2}
Activity	**W** per **T**	ml^2t^{-3}

ALEXANDER MACFARLANE.

Austin, Texas, May 28.

The New Degrees at Cambridge.

A FEW years ago it pleased the dominant body in the University of Cambridge to institute a Doctorate of Science and of Letters. Candidates for these new degrees were required to be of a certain academical standing, and to submit the proofs of their qualifications to the respective Special Boards of Studies, who, after certain formalities, were empowered to forward their claims to the General Board of Studies for approval. By many well-meaning persons this step was thought to be a great encouragement to both letters and science. It was at the same time understood that the qualification for the Doctorate in Science was to be rather less than was required for admission to the Royal Society—a standard which all will admit is not too high. Whether any similar understanding was agreed upon as regards the Doctorate in Literature is uncertain. At first there was no particular desire shown among the best men of science and literature to aspire to the new distinction, and it is rumoured that a considerable amount of persuasion and friendly pressure had to be used to induce such men to submit to the infliction. But in time a few leading lights underwent the ordeal and were duly invested. The way being cleared, a good many others have followed, and as the Boards have not been too severe in judging the claims of candidates, the outbreak of "scarletina" has become rather general. However, no particular harm has ensued, and the coffers of the University have reaped the benefit—for the fee is not small.

But now there is another aspect to this business. The new Doctorate is inferior in rank to that of the old Faculties. The senior Doctor in Science or Letters must always yield precedence to the youngest Doctor in Divinity, Law, or Physics. So far, those who have sought the new degrees have known what their position would be; but of late the Council of Senate has taken upon itself to determine that when an honorary degree should be given to any distinguished man of science or letters he is not to have the higher degree of LL.D., but to be content with the lower rank. As a rule honorary degrees are almost invariably given to strangers—foreigners or colonists. They are not aware of this fine though real distinction; and thus this very day the Senate House at Cambridge has witnessed the time honoured and highly valued distinction of LL.D. being conferred on a number of excellent gentlemen, beginning with the Lord Mayor of London, while the new and inferior rank of Sc.D. is bestowed on one of the most distinguished biologists of the United States, whom the sister University is this week to recognize as a D.C.L.

It may be urged that proceedings like this are necessary to reflect the proper amount of dignity on the new "honour," and that in time it will be regarded as highly as the old one has been. But I submit that this is not fair to the innocent recipients, and, moreover, that the University should recognize the fact that its highest honours are not to be bestowed upon successful merchants, politicians, and persons of eminent social standing, while the greatest men of letters and science have to take up with the lower grade. OUTIS.

June 20.

"After-Glows" at Helensburgh.

I BEG to inclose a letter from Mr. L. P. Muirhead, with reference to the "after-glows" recently seen at Helensburgh, which you may think worthy of a place in NATURE.

ROBERT H. SCOTT.

Meteorological Office, 116 Victoria Street, London, S.W., June 8.

Rosemount, Helensburgh, June 4, 1887, 21*h*.

DEAR SIR,—I do not notice any remarks in any of the weather reports or in the press concerning the after-glows, and as they may be local only, I drop you a line. All have lasted about 45m.; the first of any note, on the 17th, commencing well down on the eastern, and finally fading away on the western, horizon, all through of a deep rosy red reflected from the under and western side of cirro-stratus. Again, on May 21, 23, 29, 30, 31, and June 1. The last was peculiar, not only as being the most lurid, the cloudscape being marvellously fantastic, but, dying away at 21h., it revived faintly at 21h. 18m. to 21h. 30m., and again from 22h. to 22h. 20m., of a decided rose colour on western side of roll-cumulus coming up from east-north-east. Thursday, Friday, and to-night there is no glow; overcast and oppressive just now; a little rain fell in forenoon. The glow reminds me, on a more intense scale, of that previous to January 26, 1884, and again on December 8 last year.

From May 21, until to-day, the weather has been genial and fine. Faithfully yours,

LEWIS P. MUIRHEAD.

R. H. Scott, Esq., Meteorological Office, London.

Zirconia.

SOMEHOW I overlooked for a few days the letter of Messrs. Hopkin and Williams, which necessitates a brief reply, since they have confused (I am sure from mere haste) two samples, one of which I never had, and a correspondence most of which took place after what I had recorded.

Briefly, these are the facts. I was informed by Mr. T. Bolas that I could obtain "pure zirconia" of Messrs. Hopkin and

Williams at a certain price. This seemed to me so low that I asked them about it, when they did inform me that the reason was its occurring as a by-product. Nothing whatever was then said, however, about being "impure"; on the contrary, they inclosed two small fragments, one of which they said they sold as "pure," and the other (at half price) as "impure." The last was a light yellow-brown colour, and I never meddled with it; of the other I purchased an ounce for trial. On finding so much silica and soda I wrote them reporting, and asking if the sample was reduced by the hyposulphite process, as Dr. Draper had mentioned the difficulty of getting a pure product in that way. They replied that hyposulphite was used, and that the "pure" sample might possibly contain soda, but they thought not silica; the other sample might contain soda, silica, and probably iron. I wrote again pointing out that oxyhydrogen illumination was the most likely use for the product, and asking if they could not purify it further at an enhanced price, when they declined, as they state.

The difference is, that all this took place *after* I had purchased and tested the sample, and reported to them upon it. I inclose you copy of their price list of 1886, still later, in which you will see that "zirconium oxide" still appears without qualification; and I also forward the original bottle and label which I received from them—the latter you will perceive is "pure zirconia." The correspondence, if sent you in full, will bear out all the details above.

At the same time I would say that I had not the least idea of impugning in any way Messrs. Hopkin and Williams. I simply pointed out, as reference will show, the *generally* unsatisfactory character of samples considered commercially "pure" (one never expects ordinary purchased articles "pure" in any other sense) for one special purpose, and I much regret that their letter necessitates this correction. LEWIS WRIGHT.

P.S.—I am sorry to add that my previous letter has not elicited any very satisfactory information, or real aid towards the desired object. I learn from Mr. Cottrell that Du Motay's cylinders were unquestionably more durable than any prepared since, even with the aid of Prof. Maskelyne. But I am as unable as ever to come across one, or to find exactly how the material was prepared, or what light it gave in comparison with limes.

THE JUBILEE.

II.

WE have already referred to some aspects of the Jubilee which have a special relation to science, and we shall soon have occasion to return to the subject. In the meantime we reprint from the *Times* an admirable passage which presents a striking confirmation of the opinions we have expressed as to the true place of science in the history of the past fifty years. The passage is from the "Jubilee Retrospect" which appeared in the *Times* on Tuesday last:—

"The keynote of the Victorian era is the development of scientific research, the concomitant growth of practical invention, and the expansion of industry which these have brought about. Other ages have been fruitful of profound scientific conceptions, or have been illustrated by great inventions and discoveries, but it would be difficult to point to any half-century in the history of the world in which equal progress in speculative science has been combined with anything approaching to the magnitude, variety, and importance of the applications of science to practical ends which distinguish the present reign. It is as true to-day as at any former period that nothing great can be done in pure science save by men who make the discovery of truth the sole aim of their efforts, and who prize no other reward. But it is no less true that abstract and applied science go hand in hand as they never did before, and that each owns enormous obligations to the other. For if the triumphs of the workshop have been achieved by means of the discoveries made in the laboratory, on the other hand the laboratory depends for every step of its advance upon the technical skill and hitherto unrivalled precision of the workshop. Physical science has reached a stage at which the verification of its hypotheses and the supply of new data for its speculations demand appliances of extraordinary excellence, and in many cases a collation of experience and experiment which nothing but the practical inventions of the age could render possible. It is doubtless to the co-ordination of the two forms of intellectual activity that we owe the rapidity of recent advance. An unprecedentedly large army of inquirers has simultaneously pushed the interrogation of nature in a thousand directions, and has attained unprecedented results. But beside them has been working an army larger, and equally keen, of men, eagerly seeking to utilize for practical ends every crumb of available information, and giving to scientific ideas a concrete application which often forms the starting-point for new processes of scientific induction.

"The fundamental conceptions of the material universe entertained by educated men have been revolutionized during the last fifty years. The simple atomic theory of the older chemistry has given place to a molecular theory, which itself has undergone considerable development. The outlines of the elements which the older chemistry accepted as an ultimate analysis are melting under the gaze of the spectroscopist, who across the haze of their wavering figures catches glimpses of a simple primal matter. The evolution of matter is, however, like the evolution of living forms, a philosophical conception which must always rest rather upon the general necessities of thought than upon actual experiment. The immutability of certain forms of matter in all the conditions that we can devise or have any experience of is as absolute as the persistence of specific types in the animal or vegetable kingdom. The most refractory substances have been vaporized in the electric arc, and the most attenuated gases have assumed the solid form under the combined influence of intense cold and enormous pressure. But we have made no nearer approach to actual evidence either of material evolution or of the complexity of the so-called elements than may be inferred from certain spectroscopic observations of the sun and some experiments tending to show that in some cases we have confounded two or more very similar elements under one name. Apart, however, from these abstruse speculations, the whole tendency of physical and chemical investigation has been to bridge the gulf formerly fixed between molar and molecular motion and between chemical and mechanical force. There is an obvious interdependence between this scientific movement and the doctrine of the conservation of energy, which is one of the main philosophical achievements of the epoch under discussion. According to that doctrine, the total energy of any body or system of bodies is a quantity as absolutely fixed and as incapable of suffering either increase or diminution as the matter of which these bodies are composed. Energy, like matter, may assume an endless variety of forms; but the force put forth by the locomotive is as indestructible as the particles which compose its framework or its fuel. But to balance our account we have to take cognizance not only of the forces of impact or pressure of which we have direct experience, and conceive ourselves to have tolerably full understanding, but also of the forces of attraction and repulsion in their various forms, concerning which we as yet know absolutely nothing beyond the fact of their existence as inferred from their effects. To refer the whole complex sum of these energies to a general law, and to deal with them on fundamental physical and mathematical principles, is the aim of the physical science of to-day. Notwithstanding all superficial resemblances, it stands differentiated from the science of all past ages by the clearness with which it apprehends the nature of this quest and the unrivalled range of the analytical methods it has brought to bear. In the domain of biology the theory of evolution, first placed upon a scientific basis by the genius of Darwin, is a product of the same great movement of philosophic thought which brought forth the molecular theory of matter and the doctrine of the conservation of energy. The idea of evolution itself was not new, but what was new was the proof that in the vast geological changes established by the labours of Lyell and other workers in the same field, in the visible tendency to variation in existing plants and animals, and in the evidence collected by Darwin's industry and observation of the power of the struggle for existence to exercise, in given conditions, a selective and protective influence upon occasional variations, we have all the data required for the construction of a coherent theory. Evolution has now definitely taken its place as a working scientific hypothesis, not, indeed, capable of explaining all the facts of biology, but consistent with these facts and furnishing—the most that a scientific hypothesis can ever do—the means of systematizing our knowledge in preparation for a further advance. The study of embryology is already modifying profoundly the interpretation put upon the evolutionary theory, and is probably paving the way for some new generalization. Mr. Herbert Spencer's application of the theory of evolution to the facts of social order is the expression, in the sphere of human thought and action, of the intellectual movement of which Darwin made himself the exponent in the field of biology.

"But striking as is the enlargement of the intellectual horizon during the last fifty years, the imagination is more powerfully impressed by the enormous extension of the applied knowledge which vivifies and transforms old industries, invents new ones, abridges the whole mass of social labour, annihilates the obstacles of time and space, destroys the enemies of the general well-being, and endows the whole population with conveniences, comforts, and luxuries which a century ago were beyond the reach of kings. It seems as if the tree of national effort, after long putting forth scanty leaves and rare blossoms, had suddenly borne a load of fruit. Knowledge, which had long lain dormant or had led only to slow and trivial change, seems suddenly to have acquired a new significance in the minds of men, and to have taken on a new and unprecedentedly rapid development. Physical science had made great advances between the age of Elizabeth and the close of the last century; but relays of swift horses represented at one period as at the other the most rapid attainable mode of travelling or of transmitting news. The power of steam had been practically utilized by Watt a hundred years ago, and the investigation of electrical phenomena had made great progress before the accession of Victoria, but the whole of the vast improvements in locomotion and the transmission of news which we now enjoy have been effected since that event. With the exception of one or two short lines, the whole railway system of the country is the creation of the last half-century, and its effect upon the fortunes of the nation can hardly be over-estimated. The England of to-day has, in fact, been rendered possible only by the railway system, which in turn has been fed by the industries it fostered, and depends for its very existence in the form we know upon the modern development of telegraphy and engineering. It is easy, but not particularly useful, to give statistics showing the growth of railway enterprise since George Stephenson began his task of developing steam communication. No figures can add to the impressiveness of the consideration that, whereas railways are now everywhere, fifty years ago they were practically nowhere. Our whole modern system of commerce has grown up around this efficient system of intercommunication, and depends absolutely upon rapid transit for its very existence. But the direct results of the application of steam to locomotion are probably trivial in comparison with its profound influence upon the social life and even the moral character of the nation. The population of the country, formerly attached to the soil on which it was born by necessities stronger than feudal custom, has been endowed with the power of easy, rapid, and comparatively cheap locomotion. For good and for evil the habits of mind belonging to an age characterized upon the whole by permanence of local relationships have given place to the habits proper to a time in which labour is nomadic, and all the relations of life in the remotest districts are profoundly affected by the attraction of distant centres of population. The immense increase of these centres, and the corresponding depopulation of rural districts, is one of the most obvious results, not, indeed, of railways alone, but of that industrial revolution in which they have played a central and indispensable part. That revolution may be defined as a great and sustained movement in the direction of economizing and organizing labour. Railways have powerfully promoted economy by reducing to a fraction of its former amount the time spent in the transport of goods and workmen, and they have no less powerfully promoted organization by equalizing conditions and combining a thousand isolated stores of industrial energy into one central reservoir. Nor must we leave out of sight the enormous effect they have produced by facilitating the transmission of correspondence and news. While the railways were yet in their cradle they were utilized for the carriage of the mails, but the whole postal system was so chaotic and inefficient that the public could have reaped but little advantage save for the drastic reforms advocated by Rowland Hill in 1837, and carried into effect, in spite of the opposition of the Post Office officials, in 1840. The establishment of the penny post, together with the novel rapidity and regularity of the service rendered possible by railway extension, is in itself a reform which in earlier ages would have sufficed to render a reign illustrious. It has been supplemented by a telegraph system which as far transcends the penny post as that surpasses the clumsy and costly system of the last century; and the telegraph is in turn yielding the palm to the telephone, in the use of which, however, this country, owing to the obstructiveness of the Post Office, is far behind America and some Continental States.

"The maritime supremacy of this country was fully established long before the accession of Victoria, and the marine steam-engine was familiar long before the locomotive. Patents for screw propellers were even taken out a century ago, although they were not successfully applied until 1837, when Ericsson attained a speed of ten miles an hour. In the following year the *Great Western* performed what was then the extraordinary feat of making the passage from Bristol to New York in eighteen days. Considerable success had thus been attained before the present reign in the application of steam to marine transport, but the advance that has since been made is not less remarkable than the improvement in land transport. The voyage to New York is now performed in six days, and ships are actually sailing between Liverpool and the Isle of Man at a speed equivalent to doing the New York passage in five. But the real measure of the revolution that has taken place must be sought in the supersession of sailing vessels by steamers for all the purposes of commerce, and the consequent multiplication of the resources of industry. At the beginning of the reign the tonnage of British steam-ships was considerably under 100,000 tons. It is now about 4,000,000. But just as the immense growth of railways has not prevented a large increase in the traffic of the canals, so has the increase of steam-shipping left room for an addition of 50 per cent. to the tonnage of British sailing-vessels. The increase of steam-tonnage taken alone gives but an imperfect idea of the progress that has been made. For by continual improvements in marine engines each ton of shipping is moved at a greatly increased rate and a greatly diminished cost; while, as regards a very large and important portion of our trade, the opening of the Suez Canal, to which we supply four-fifths of its traffic, has still further economized time and labour. In this connexion by far the most important achievement of recent years is the opening of the Canadian Pacific Railway, and the establishment of a line of steamers connecting its western terminus with India, China, and Japan. We thus gain a shortened route to the East, passing entirely over great ocean highways and British territory instead of through a land-locked sea and a narrow gut which accident or design may at any moment render impassable. In view of the expansion of commerce during the last half-century, and of the immense undeveloped resources of Canada, it would be rash to set any limits to the future possibilities of this great Imperial highway.

"The universal acceleration of locomotion and transit is the most extended and general application of science to the great modern purpose of economizing labour and time. Every department of industry can, however, show special applications for effecting the same result."

ATLANTIC WEATHER CHARTS.

THE Meteorological Council has recently issued the second part of the Synchronous Weather Charts for the North Atlantic and the adjacent continents, the folio just published embracing two charts for each day from November 8, 1882, to February 14, 1883. The first part was noticed in NATURE, vol. xxxv. p. 469, when we gave a somewhat detailed explanation of the charts and the observations upon which they were based. The second part embraces a very large portion of an English winter, and the conditions pictured over the Atlantic show that the weather over that ocean in winter is far more disturbed than it is during the summer months. The barometer in the winter ranges both higher and lower, and the changes of pressure are much more rapid and considerable. The movements of the travelling disturbances are also accelerated, and keep in a much lower latitude, the British Islands coming frequently under their full influence after they have passed over the warm and moist air of the North Atlantic. In the summer the barometer is above 30 inches over the greater part of the ocean, but the highest readings seldom exceed 30·3 inches, whilst the areas of low pressure, the readings at the centre of which are seldom especially low, ranging for the most part from 29·2 to 29·5, skirt to the north of the high-pressure area, and pass as a rule well to the northward of the United Kingdom. At times these low-pressure areas scarcely influence our weather. At other times, when from some cause the high-pressure area is situated in rather a lower latitude than usual, the low centres will have a more southerly route in their passage from west to east, and will occasion disturbed weather over our islands, but for want of sufficient difference of barometric pressure will but very seldom materially augment the strength of the wind. If, however, this southerly track of the disturbances is maintained for any length of time in the summer, it will have a very marked effect upon our weather, occasioning frequent and heavy rains; it was this which caused the entire failure of real summer weather in 1879. The winter charts show that the barometer often ranges as high as 30·5, 30·6, and 30·7 in Mid-Atlantic, whilst on the adjacent continents such readings are common, and in North America much higher readings occur—on February 1 the mercury reached 31·1 inches. The charts do not extend to Siberia, but it is notorious that excessively high readings are commonly experienced there during the winter months. The low-pressure areas which are principally limited to the ocean, and almost solely to the northern latitudes, frequently have the barometer at the centre below 29 inches, and occasionally below 28 inches. With these differences of barometric pressure there is ample material for the development and maintenance of storm systems; and the most cursory examination of the charts shows to how great an extent storm after storm rages almost daily in one part or another of the Atlantic, and frequently several storm areas exist at one and the same time. This second series of charts illustrates in the most unmistakable manner the behaviour of storms over the Atlantic: many a disturbance can be traced in its progress for days together. On November 13 a storm area was passing over the north of France, and was occasioning strong easterly gales in the south of England and the English Channel. This disturbance can be traced back day by day until November 3, when it was in the vicinity of the West Indies, where it was apparently bred. The severe storm which was blowing over the British Islands on November 19 was apparently formed over central North America on November 9, and, after travelling slowly over the Lake District, left the Gulf of St. Lawrence on November 14, and followed a north-easterly track, but, after passing over the south of Greenland, it took a more southerly course, the centre subsequently passing between Iceland and Scotland. A fairly good specimen of storm development is shown on the charts of February 7 and 8: on the 7th, a bend is shown in the isobars of 29·0 and 29·1 at about 300 or 400 miles to the west of Ireland, and this on the following day becomes a closed area with its complete wind circulation; the disturbance, however, dies out again on the 9th. A feature of very special interest in the charts is the size of some of the disturbances: this stands out clearly from the graphic manner of representation. There are many instances of a gale blowing simultaneously in America and Europe, due to the same storm area, and in these cases the area of low-barometer readings usually occupies the whole of the northern part of the Atlantic, whilst over the land, both in Europe and America, the barometric pressure ranges very high. On January 23, as the result of a single low-pressure area, a gale was blowing in Hudson's Bay, Labrador, and Newfoundland, and completely across the Atlantic to the North Sea and the north of Norway, the diameter of the area over which the wind was blowing with gale force, being as much as 3800 miles (nautical): the centre of the storm was situated off the south-west coast of Greenland, where the barometer was reading 28·2 inches, whilst in America and Europe the barometer reached 30·8 inches. An almost equally large disturbance is shown on February 10, the gale force extending quite across the Atlantic from Labrador and the Gulf of St. Lawrence to the Gulf of Bothnia, the diameter of the gale area being fully 3000 miles.

The equatorial doldrum is shown to be of less extent than the general charts which have been deduced from averages would lead one to suppose, and very frequently the north-east and south-east trades almost meet. Between longitudes 20° and 30° W., the position at which the trades meet in November is about 5° N., in December about 3° N., whilst in January and the early part of February the south-east trade only just blows north of the equator, and the doldrum is probably at this time at its most southern limit. The north-east trade is far more regular on the eastern side of the Atlantic than in mid-ocean or on the western side, and this is fully accounted for by the fact that the wind blows round the Atlantic high-pressure area in agreement with the ordinary anticyclonic circulation, so that on the eastern side of this high pressure which is also, as a rule, the eastern side of the Atlantic, the wind is northerly, whereas to the westward of this area of high barometer readings the winds are frequently from the southward. The northern margin of the trade varies considerably, and is almost entirely dependent on the position of the area of high barometer situated over the Atlantic; when this area is well to the northward the northerly winds hold from the chops of the Channel down the coast of Africa to about 5° N., so that a vessel may leave England and keep a steady northerly and north-easterly wind until close to the equator.

The winter charts also show that the differences of temperature are much larger over the Atlantic than they were in the summer or autumn series, and the isotherms of both air and sea run much closer together. On November 25 there is a difference of 30° in the sea temperature in the distance of 340 miles to the south east of Newfoundland, whilst on the eastern side of the Atlantic the same difference of temperature, 40° to 70°, spreads over 2360 miles. This disparity between the difference of temperature on the western and eastern sides of the Atlantic is quite common throughout the whole period of the charts, but not always to so large an extent. The charts of December 15 and 19 are other instances which show this difference, and on January 6 there is a difference of 30° (from 30° to 60° F.) in 120 miles off the south of Newfoundland, whilst on the eastern side there is only an equal difference of temperature (50° to 80°) in 3300 miles. The largest differences of temperature occur between latitude 40° and 45° N., and longitude 40° to 60° W., which is the area most affected by the meeting of the warm water of the Gulf Stream and the cold Polar current, and the weather which is given on each chart shows that there is almost constant rain in this position, and it is also the breeding-place of many a storm area, and storms when generated have a decided tendency to keep in the track of the Gulf Stream.

These synchronous charts will materially aid investigators in tracing the connexion between the weather in the British Islands and that over the Atlantic, and as it is not possible at present to know what is going on immediately to the westward of us, it is the more necessary to deduce, if possible, laws which regulate the changes from time to time. By the publication of these charts the Meteorological Council afford opportunity for testing many theories. Among these may be mentioned the theory of indraft of wind towards the centre of a cyclone, if this is not already pretty conclusively proved. Light is also thrown upon the question as to the position of rain with regard to the position and development of the general storm area, and upon many other inquiries of a similar nature. We hope that after the two remaining parts of the work have been completed the Council will see their way to undertake a thorough discussion of the material which the charts contain.

A REVIEW OF LIGHTHOUSE WORK AND ECONOMY IN THE UNITED KINGDOM DURING THE PAST FIFTY YEARS.[1]

II.

THE fifty years of the present reign have been distinguished with regard to lighthouse illumination by the development in this country of the beautiful dioptric system of Augustin Fresnel. In 1837, this system had been established in France fifteen years, but had only just been introduced into Britain, where the catoptric system was in full operation. Parabolic reflectors formed of facets of silvered glass were used in the Mersey lighthouses so far back as 1763, and at Kinnaird Head, in Scotland, in 1787. In 1804, perfected reflectors of silver plate rolled upon copper were used at Inchkeith, and similar reflectors have been ever since employed. To Teulère must be attributed the honour of the invention of these parabolic mirrors, in 1783. The Inchkeith Lighthouse is also notable as the first in Britain to receive a Fresnel apparatus (1835), through the exertions of Alan Stevenson, who placed the next one at the Isle of May (1836), and the third at the Start (1836). These lights were all of the first order, Start and Inchkeith being revolving, and Isle of May fixed. They were constructed by Messrs. Cookson, of Newcastle, who subsequently constructed at least a dozen others, mainly as regards the refracting portion.

The lenticular system, as received from Augustin Fresnel by Alan and Robert Stevenson, comprised four principal optical agents of glass, viz. the cylindrical refractor, the totally-reflecting prism, the refracting vertical prism, and the annular lens. These have been continued in use, with few modifications, until the present day, while his auxiliary elements, such as the small inclined lenses, the silvered metallic zones, and the plane silvered glass mirrors, have been abandoned. The first-order fixed light of Fresnel came well-nigh complete from his hands, and has remained unchanged in size and character, save as relates to the number of prisms above and below the lenses, which has been increased from 19 in all to 26, and as to the joints of the lenses, which have been made inclined instead of vertical, the latter improvement being due to Alan Stevenson, who also introduced a refractor of more truly cylindric form. It is in the apparatus of revolving sections that the most striking ameliorations have been effected. The French engineers added little between 1822 and 1852 to Fresnel's original work, a few combinations or modifications of his elements to produce flashes alternately with fixed light being nearly all. But between 1849 and 1852 the great improvement known as the holophotal system was elaborated by Mr. Thomas Stevenson. It is difficult to describe without drawings the various applications to both catadioptric and dioptric instruments of this principle, by which the light of maximum intensity, or the best utilization of all the rays, was attained. The first *catadioptric* holophote was employed at the North Harbour, Peterhead, in 1849. Better forms were realized in 1864. The first use of holophotal metallic mirrors above and below the annular lenses of a large revolving light was at Little Ross. These mirrors, which needed no small auxiliary Fresnel lenses, were, instead of being plane, like Fresnel mirrors, generated by a parabolic profile passing round a horizontal axis. The typical *dioptric* holophote is a central refracting lens of usually three elements, with a series of concentric holophotal totally-reflecting rings, forming an instrument of varying diameter and focal distance, condensing into a parallel beam all the front arc of the diverging sphere of rays. The holophote is perfected by a glass spherical mirror of totally-reflecting prisms so shaped and set as to return all the back hemisphere of incident rays through the flame, to be parallelized and sent out with the front hemisphere of rays. This spherical mirror in its most effective form was the invention, in 1861, of Mr. James Chance, who generated the double-reflecting prisms or zones round a vertical instead of a horizontal axis, separated them, and divided them into segments or panels, thus making it practicable to increase the radius of the mirror and apply it to the largest apparatus as a most useful adjunct. In this instrument the image of the flame is not reversed, and the light sent back is at least three-fourths of that received.

But the most important application of the holophotal system was to the dioptric revolving sea-light. The totally-reflecting zones above and below the refracting lenses were generated round a horizontal instead of a vertical axis, and made to work in complete unison with the lenses, the light being parallelized in every plane from top to bottom. The first holophotal sea-light was the North Ronaldshay, in 1851. Since that date every revolving light with prisms has been holophotal. It has been estimated that the modern plan gives light five or six times more intense than the original plan.

Another material addition to the resources of the lighthouse engineer has been contributed by Mr. Thomas Stevenson in the azimuthal condensing system. This is, briefly, an arrangement of the optical agents before described, and of some others specially devised, by which either one arc of the horizon is illuminated by a beam of the greatest attainable intensity while the rest is dark, or else two or more sectors are lighted with equal or with unequal intensity while the others are dark ; these distinctions being governed by the nautical requirements as to range and direction of the sea-coast, channel, or harbour

[1] Continued from p. 105.

where the light is established. The beams thus sent out may be white or coloured, the differences in coloured media themselves, or, as compared with white light, being equalized approximately by the instruments used. The condensing method has been applied more freely to the smaller than to the larger orders of apparatus during the past twenty-five years; and among the most beautiful illustrations of the system, designed not alone by Mr. Stevenson, but by Mr. Chance, Mr. Alan Brebner, and Dr. Hopkinson, may be cited the Buddonness, the Isle Oronsay, the Lochindaal, the Dartmouth, the Hoylake, and many apparatus for certain narrow seas in Australia. But the large lights of Orme's Head, Dungeness, Bidston, Longships, St. Tudwal's, Dublin Bay, and McArthur's Head, may also be selected as good examples of the condensing plan.

A third and very valuable improvement is the group-flashing system of Dr. John Hopkinson, F.R.S., by which a new series of characteristics has been added to revolving lights. This invention dates from 1874, and consists in so shaping and combining on unequal axes the panels of an apparatus that a double, triple, or fourfold flash may be produced, each flash of the group being of such duration and divided from another flash by such an interval of time that compass-bearings may easily be taken from the ship; while each group is separated from another group by a longer interval, the whole period being one of the usual periods of revolving lights, such as half a minute. Thus, while adequate power is maintained for each flash, an unmistakable distinction is established. This plan became rapidly popular. The Trinity House were the first to apply it, in 1875, to the catoptric floating light on the Royal Sovereign Shoals, near Hastings. The next applications were to a dioptric light for Mexico, and to the Little Basses light, Ceylon. It is now used all over the world. At the Casquets, in 1876, it enabled the Trinity Corporation to dispense with two of the three lights hitherto employed, and show from one tower a half-minute light in triple flashes, each lasting two seconds, each interval between them three seconds, and the long interval between the groups eighteen seconds. The great lights of Bull Point, Hartland Point, and Eddystone are other examples of double and triple group-flashing by optical combinations.

The use of colour in lighthouse practice has been gradually diminishing since 1837, and is now almost restricted to harbour-lights and ship-lights, with a few cases of fixed sea-lights where a danger is to be marked over a narrow sector. The loss by absorption in red and green, the only two colours available, being from 60 to 80 per cent.—a loss slightly redeemed in the case of red by a certain relative superiority to white in thick weather—it is obvious that colour must sooner or later disappear from the list of effective lighthouse agents. Meanwhile the power of a coloured beam (without regard to the illuminant) has been optically enhanced by one of two methods, superficial amplitude and azimuthal condensation.

Where a revolving light is to show, in alternate or other series, red and white beams, the power may be approximately equalized by assigning to the red a certain greater angular breadth in the panels of prisms and lenses than to the white. The Wolf Rock light (1869), the Flamborough Head (1872), the Hartland Point (1874), were so treated by Mr. James Chance, though with different arrangements of panels, the average proportion being 73 for the red, and 27 for the white. The coloured glass plates used were of a selected tint of "copper ruby." The second method, condensation, is mainly applicable, as before mentioned, by means of vertical prisms and other agents to lighting sectors of the horizon, or to securing perfect definition between two coloured arcs or between a white and a coloured arc. The Kingswear fourth-order light, Dartmouth (1865), designed by Mr. Chance, is an excellent example. In a seaward arc of 45° there is a central white beam of $9\frac{1}{2}°$ between a red beam of $17\frac{3}{4}°$ and a green beam of $17\frac{3}{4}°$. Ten vertical prisms were used, four condensing the lights on the border of the red and white, and four on the border of the green and white, while two augmented the central beam. The fairway channel to the harbour is indicated by the coloured light, and the bright beam constitutes a sea-light which is frequently observed at a distance of sixteen miles, though the lamp is inferior to the lamps of to-day.

The signal-lights of the port and starboard sides of a vessel are coloured in order that a marked contrast may be visible at a distance of at least two miles, and her course and evolutions plainly understood. But the great inferiority of green to red, and of both to white (the third signal carried by a steamer being a white light), combined with the imperfection of the optical apparatus and of the burner used, renders too many ship-lights lamentably untrustworthy at even this short range, and can only tend to multiply such terrible collisions as those with which we have become familiar during the past fifteen years. It might be impracticable, on account of weight or cost, to introduce condensing agents into side-lights generally, though Mr. Thomas Stevenson, ever foremost in the van of improvement, tried them on the small steamer *Pharos* in 1866; but there can be no sufficient reason for not adopting such lenses of true lighthouse types as are now made for the purpose in Birmingham and Paris, and in not fitting them with the incandescent electric light in two different degrees of power, so as to equalize nearly the red and green lights, and in not making them both equal in visibility to the white; thus securing an effective signal for the adequate protection of life and property at sea. The writer has long, but with small success, advocated this course. Public opinion, however, may yet be stimulated by some crowning disaster to insist on a reform so urgently needed, and so perfectly easy to realize.

In 1873 the first dioptric light established in England, Start Point, received its present apparatus in substitution for the old Fresnel lenses and concave mirrors. The new revolving light, the design of Mr. Chance, and which was repeated in 1874 at Cape Bon, Africa, and the South Stack Rock, Holyhead, was composed symmetrically of six sides of 60°, with the usual upper and lower prisms, the central lens having nine elements in circular settings. The panels are thus the widest in azimuth hitherto constructed, except some of those of Flamborough Head, which subtended $69\frac{1}{2}°$, or the four holophotal quadrants constituting the South Stack Low Light (1879), designed by Dr. Hopkinson, and the only existing light of the kind. By a subsidiary arrangement of totally-reflecting prisms and a holophote, a fixed red beam at Start Point was projected to a lower chamber in the tower, and thence sent out to mark the position of certain rocks. The Watling Island (Bahamas) second-order double-flashing light of 1885, designed by Dr. Hopkinson, is a unique specimen of holophotal circular settings, with the most recent improvements.

A remarkable variation of the usual elements of a dioptric sea-light dates from 1879 or 1880. Lower prisms for sea-lights had, at the suggestion of the writer in 1874, been suppressed on several occasions; and for port-lights, Messrs. Chance had dispensed with all prisms, and raised the lenses to a vertical angle of 80°. But now it was determined to produce a first-order apparatus with refractors only, extending the vertical angle to 92° from 56° or 57°, the old normal height. This was attained by Messrs. Chance by means of dense flint glass in the superior and inferior limits. The power of the lenses, always counting for 75 per cent. of that of the complete light, was thus considerably augmented, while the cost and bulk were reduced, though doubtless at the expense of symmetry. The first-order lights, Anvil Point (Dorset), the Eddystone, and the Minicoy (Indian Sea), were constructed on this principle at Birmingham (1880-83). In the case of the Eddystone, two apparatus exactly alike were employed by the Trinity House—one superposed on the other, and each lighted by its own lamp, the whole height of optical glass exceeding 12 feet. The plan of superposed lenses was first suggested, in 1859, by Mr. J. W. D. Brown, of Lewisham, and first practically set forth, in 1872, by Mr. John R. Wigham, an engineer of conspicuous ability, in connexion with his large gas flames for Irish lighthouses; and it has been since fully approved and adopted by the Trinity House. The great lights of Galley Head, Howth Bailey, and Rockabill attest the excellence of this arrangement of lenses, and the Eddystone biform (1881) is not less successful.

The enhancement of illuminating power through the amplification, vertical and horizontal, of lenticular panels has been described. But a more emphatic change, associated with the name of Stevenson, has recently been consummated. The radius or focal distance of Fresnel's first-order light is 920 millimetres. The Fresnel of our time proposed a radius of 1330, and such a lens has been already constructed in France. The name "hyper-radiant," given to it by Mr. Stevenson, seems hardly so accurately formed as "hyper-radial," which was independently suggested by the writer in 1885, although the new lens will be excellently adapted to the large flames of the day, at once utilizing their volume and not suffering from their heat. In the lights for the Bishop Rock and Round Island (Scilly) now (1887) being prepared by Messrs. Chance for the Trinity House, the apparatus will be of the hyper-radial type, and it will have a vertical angle of 80°, with glass all of the usual refractive index. There will be for each lighthouse a biform structure 15 feet high, the Bishop having lenses for white double flashes arranged in a pentagon of five groups, each lens subtending 36° horizontally, with an eight-wick burner; and the Round Island having lenses for red single flashes, each lens subtending 60° horizontally, with a ten-wick burner. Petroleum will be used in both cases. The latter apparatus would seem to mark the maximum limit of dimension, with regard to optical agents and to illuminants, compatible with the present conditions of lanterns and towers. Hyper-radial apparatus is also being prepared in Paris for the Tory Island and Bull Rock lights in Ireland.

But the true maximum of power or intensity for lighthouses must ever be sought in the electric light. This application of the branch of physical science that has perhaps more than any other distinguished the Victorian epoch had its experimental beginnings, under the auspices of Faraday, at Dungeness and the South Foreland. The apparatus used at Dungeness was of 150 millimetres radius. In 1881 the apparatus for Macquarie was constructed of 920 millimetres radius. Six large electric lights have been established in Britain since 1862, all the work of Messrs. Chance, and all of their design except the Isle of May, which was planned by Mr. Thomas Stevenson. The Souter Point light, revolving, of second and third order elements, dates from 1871; the South Foreland, High and Low, fixed, of the third order, from 1872; the Lizard fixed lights, of the third order, from 1877; and the Isle of May, which gives a fourfold flash, and is of first and second order radii, from 1886. In addition, there have been designed by Dr. Hopkinson, and made at Birmingham, the Macquarie (Sydney), a first-order revolving, the most powerful light in the world, and the Tino (Spezia), a second-order triple group-flashing light. It is needless to give details of these apparatus, which are throughout distinguished by skilful optical combinations and the utmost precision of workmanship. They have all been, with the exception of the Isle of May, the subject of elaborate papers and exhaustive discussion before the Institution of Civil Engineers. An apparatus of the second order is being prepared at Birmingham for the new electric light of St. Catherine's (Isle of Wight). It is composed of refractors only, extended to 97° of vertical angle, and with certain special arrangements for divergence. The carbons will be of 50 millimetres diameter and of a novel and perfect form.

There has been during the past fifty years, but especially since 1861, with regard to lighthouse characteristics, a selective process in operation by which the fittest have survived. Not only has the optical apparatus been perfected in curvature, finish, and adjustment to nautical conditions, and the intensity of light increased threefold, but the weaker forms of distinction have been suppressed, and the better forms retained and multiplied. Fixed lights for the most part have been discontinued, and, in this country at least, lights composed of fixed and revolving portions. Long periods in revolving lights have been altered to short periods, the uncertain aid of colour largely abandoned, the varieties of the group-flashing system invoked, and the quick contrasts of light and dark resorted to in occulting or intermittent apparatus, although the very ingenious but too complicated plan of Babbage, with its rhythmical longs and shorts, has not prevailed. The enhanced speed of steam-vessels, the multiplication of all kinds of vessels, the improvement of shore-lights, and the spread of commercial enterprise, by which new ports are opened and new coasts explored, have naturally effected these changes. And, *pari passu*, striking improvements in the mechanism of revolving carriages and of clockwork both with weights and springs, in occulting-cylinders and gun-metal framing of apparatus, have resulted from the combined efforts of our best lighthouse engineers.

The early rivalry between the catoptric and the dioptric systems has wholly ceased, the latter having, by the weight of its general and well-tried superiority, displaced the old system in all directions save in one or two revolving sea-lights of exceptional merit, like Beachy Head or St. Agnes, and save in all light-vessels where the excellent 21-inch reflectors, with the two-wick Douglass burners, often send out beams of 20,000 candles over the shoal-beset waters.

There were in the United Kingdom, in 1886, 202 sea-lights, of which 147 were dioptric and 55 catoptric, and, in addition, about 450 small lights of all kinds, making, with the 74 light-vessels, a total of about 730. Surely this is a noble growth of lighthouse illumination, even in the long period under review. It compares not unfavourably with the United States, the first country to adopt the lenticular system on a bold and comprehensive scale, or even with the country of Fresnel himself and of his brother Léonor, where the elucidations and experiments of Allard and of Reynaud, and the practical work of Lepaute, Sautter, Barbier and Fenestre, have done much to promote science and benefit humanity.

J. Kenward.

(*To be continued.*)

THE OBSERVATORIES AT OXFORD AND CAMBRIDGE.

The following is the Annual Report of the Rev. Prof. Pritchard, the Savilian Professor of Astronomy at Oxford, to the Board of Visitors of the University Observatory; read June 8, 1887:—

I. *Lectures.*—The statutable lectures have been given, and the Observatory and its instruments have been freely accessible to the students during every day of Term time. For next Term I offer a course of elementary lectures expressed as far as possible in untechnical language. I desire to add also two public lectures on the development of astronomy during the last century.

II. *Instruments.*—As a matter of practical convenience, portions of both the equatorial instruments have been within the last day or two placed in the hands of the opticians, with a view to modifications or repairs which shall render them applicable to the entirely new departure which is now in progress in respect of the processes and methods of practical astronomy. The De la Rue equatorial, which has long possessed an historical value, has been rehabilitated mainly at the expense of Dr. De la Rue in certain of its more delicate working parts, and this has been so advantageously completed that Dr. De la Rue has been induced to introduce still further renovations, whereby that instrument will be placed in a condition probably equal to that in which it first left its designer's hands.

The mounting of the large equatorial refractor, originally supplied at the expense of the University, is now required for some experimental inquiries suggested by the Photographic Committee of the Royal Society. Dr. De la Rue has supplied two mirrors of 15 inches aperture of different focal length, and these are to be mounted alternately on the tube of the refractor, together with a camera as arranged by Mr. Grubb. The expense of these valuable additions is borne by the Royal Society and by Dr. De la Rue. The delicacy of the projected inquiries necessitates the electrical control of the driving-clock.

The transit-circle recently presented to the University by Mr. Barclay has realized my expectations of its excellence. I find it to be thoroughly stable, and sufficient for all the purposes required, whether for University instruction or for accurate meridional observations. In the latter respect it completes the Observatory equipment. The electrical illumination of the circles and other necessary parts has proved entirely successful, and the general aspect of the instrument as it stands on its massive piers is such as to suggest confidence.

III. *Buildings.*—The fabric of the building and its complicated roofs and domes are in excellent substantial repair, and will require no outlay that I can foresee during the present year.

IV. *Astronomical Work.*—The somewhat hazardous enterprise of attempting for the first time in the history of astronomy to obtain the distance of the fixed stars from our earth by the aid of photography has been attended with success. The final results of the investigation have been placed in my hands only during the writing of this Report. The first observation was obtained on May 26 of last year, and the last was effected on May 31 of the present year. The intermediate computations were systematically continued during the interval. They involved the reduction of no less than 30,000 bisections of star images, on 330 photographic plates, procured on 89 nights. Eight independent determinations of the parallax of the two components of 61 Cygni resulted from all this work, and these happily indicate a substantial agreement between themselves, and afford other necessary proof of reliability.

By a happy coincidence, on the very day when the final results of these investigations were evolved, I had the pleasure of a visit from Her Majesty's Astronomer at the Cape of Good Hope, a practical observer whose experience in parallactic investigations is probably unrivalled. His remarks, after critical examination of the entire work, have encouraged and gratified me. Astronomical photography is hereby placed on a secure basis as an efficient and exact exponent of the highest form of astronomical science.

Simultaneously with these observations, similar work has been in progress for the determination of the parallax of μ Cassiopeiæ and Polaris. These observations will now be treated on a less laborious scale. Photographic plates of the Pleiades have also been taken with the view of obtaining the accurate relative positions of about one hundred stars therein. The necessary triangulations have been commenced.

I should say that the experimental investigations required by the Photographic Committee of the Royal Society originated in the necessity of ascertaining what are the limits of accurate field obtainable from mirrors of different focal lengths: the inquiry had distinct reference to the questions which were open for discussion at the recent Paris International Conference. I deeply regret that I was unable to fulfil my intention of taking part (as invited by Admiral Mouchez) in that important meeting.

V. *Finance.*—The funds granted by the University have been sufficient, notwithstanding the continuous activity, which requires a corresponding continuity of outlay. This grant, hitherto triennial, expires on December 31 next. If the Board of Visitors see fit to request the University to continue this grant for five years, it would assist me in undertaking, for the University, a share in the production of a photographic map of the heavens, a valuable and extended class of work, which under other circumstances I should not be justified in contemplating.

The details given above testify without further words of mine to the unwearied perseverance and intelligence of my two able assistants, Mr. Plummer and Mr. Jenkins.

Prof. J. C. Adams has just presented the Report of proceedings in the Cambridge Observatory, from May 27, 1886, to May 26, 1887. From this Report we take the following extracts:—

The total number of observations made with the transit-circle during this interval, for determinations of right ascension and north polar distance, is 2253.

These include 726 observations of clock stars made on 151 nights; 68 observations of Polaris at the upper transit involving 169 circle readings, and 61 observations at the lower transit involving 149 circle readings; 1331 observations of zone stars made on 88 nights; and 67 observations of stars compared with the minor planet Sappho.

For instrumental adjustment, the nadir point was observed 218 times, the bisections of the declination wires with their images being in every case made in two positions of the observer, on the north and south sides of the tube respectively; the level and collimation errors were each observed 217 times.

At the request of Mr. Bryant, F.R.A.S., the planet Sappho was compared with adjacent stars 70 times on 7 nights from January 12 to February 2, by means of the Northumberland equatorial and square bar micrometer for differences of right ascension and declination. Before the end of February all the compared stars were repeatedly observed with the meridian circle; and in addition to this 9 stars which had been compared elsewhere with Sappho.

State of the Reductions.—The true right ascensions are obtained up to February 17, 1887, and the true north polar distances to April 27, 1887.

The mean right ascensions and north polar distances for January 1, of the standard stars are calculated to the end of 1886, as are also nearly all the observations of stars made in the present year for comparison with Sappho. The mean R.A. and N.P.D. of the zone stars are similarly reduced up to the end of 1881. The right ascensions of zone stars are reduced to the epoch 1875 as far as March 16, 1878, and the north polar distances to March 1880.

The collection of the observations of the zone stars for the Catalogue has been commenced.

A fresh determination of the intervals of the right ascension wires from 73 observations of Polaris, from 1885 November 17 to 1886 July 6, was completed on July 12. As no change seems to have taken place in the wires, the results were combined with those previously obtained: so that the final determination rests on 145 observations of Polaris made from 1885 January 21 to 1886 July 6. These intervals were used till 1887 March 3, since which time another determination, from 78 observations of Polaris, from 1886 July 7 to 1887 April 27, has been used.

Sixty-five observed north polar distances of Polaris above the pole, deduced from observations made in 1886, with observed nadir point and assumed colatitude 37° 47′ 8″·4, and corrected for flexure and errors of division, give a north polar distance less than that given in the *Berliner Jahrbuch* by 0″·557: 68 observations below the pole, treated in the same way, give a polar distance greater than the Berlin one by precisely the same quantity. Thus our polar distance of Polaris for 1886 is exactly equal to the Berlin one, and the correction of assumed colatitude is + 0″·557; results very similar to those of previous years.

The observations of Polaris above the pole, direct and reflected, made by Miss Walker on 1886, April 8, May 3, 4, and 6, when corrected for errors of division and for refraction, give for the colatitude 37° 47′ 8″·853. The mean for eight years, given in the last Report, is 37° 47′ 8″·854.

579 observations of clock stars made by Mr. Graham in 1886, Mr. Todd in nearly every case reading the circle, give, as a mean value for reduction to the Berlin N.P.D. + 0″·353; or, if we take the means for each separate night as of equal weight, + 0″·319. These have not been corrected for errors of division and flexure, which, for the limits of the zone, 60°-65° N.P.D., have probably a mean value of - 0″·26 or thereabouts; this would have to be applied with an opposite sign to the above means: but the results for intervals of 1° show that the errors of division ought to be determined for each star, as they have been for Polaris and for the nadir point.

Meteorological Observations.—The meteorological observations continue to be communicated daily by telegraph to the Meteorological Office.

The sunshine recorder has been regularly employed, and the records are sent at intervals to the Office.

NOTES.

In the distribution of Jubilee honours the claims of science have not been forgotten. Among those who have been raised to the peerage we are glad to see the name of Sir William Armstrong, C.B., F.R.S. The honour of knighthood has been conferred upon Warington Smyth, Esq., F.R.S.; Dr. Garrod, F.R.S.; G. H. Macleod, Esq., Queen's Surgeon, Edinburgh; and J. Wright, Esq., C.B., late Civil Engineer to the Navy. Among the new Knights Commanders of the Bath are John Simon, Esq., M.D., C.B., F.R.S., late Medical Officer, Privy Council Office; and Capt. Douglas Galton, C.B., F.R.S.; Prof. W. H. Flower, F.R.S., British Museum, and Prof. Brown, Agricultural Department of the Privy Council, have been made Companions of the Bath.

The names of the following gentlemen have been added to the list of the Tyndall Dinner Committee:—The Duke of Northumberland, President of the Royal Institution; Sir W. G. Armstrong, F.R.S., ex-President of the Society of Mechanical Engineers; Dr. Haughton, F.R.S., President of the Royal Irish Academy; E. H. Carbutt, Esq., President of the Society of Mechanical Engineers; and G. B. Bruce, President of the Institution of Civil Engineers.

Mr. Harford J. Mackinder, M.A., has been elected Reader in Geography at the University of Oxford.

In the Report on the Oxford Observatory, which we print to-day, reference is made to important improvements effected, either wholly or in part, at the cost of Dr. De la Rue. We may add to what is there stated that Dr. De la Rue generously offers £500 to convert the Oxford 12¼-inch refractor into a Henry photographic telescope—practically, to buy a new object-glass.

The annual general meeting of the Marine Biological Association will be held to-morrow in the rooms of the Linnean Society. The Laboratory on the Citadel Hill, Plymouth, erected by the Association at a cost of £9000, will be opened for work in the summer, and the Council are anxious to co-operate in the foundation and management of laboratories on other parts of the British coast.

Miss Oldfield has presented to the Herbarium of the Royal Gardens, Kew, the botanical collections made in Australia by her late brother, Mr. Augustus Oldfield. This gentleman was, as stated by Mr. Bentham in the preface to "The Flora of Australia," an acute observer as well as "an intelligent collector." His series of *Eucalypti* are especially good, as he took great pains to obtain the various forms of foliage characteristic of each species, as well as the fruiting and flowering stages. Sir Joseph Hooker used his Tasmanian plants in his "Flora" of that colony. Mr. Oldfield "made large additions to the West Australian plants previously known." These collections were placed at Mr. Bentham's disposal for the purposes of his "Flora Australiensis."

The biennial Exhibition of Agriculture and Entomology in Paris will take place from August 27 next to September 29, at the Orangerie, one of the terraces of the Tuileries Gardens. The French Minister of Public Works is the President of the Society which organizes the display.

The Pilot Chart of the North Atlantic Ocean for June, issued by the Hydrographic Department of Washington, states that Capt. Lassan, of the Norwegian barque *Petty*, while in lat. 17° 38′ N., long. 46° 34′ W., on April 1, experienced three distinct shocks of earthquake, diminishing in force, and accompanied by strong eruption of air-bubbles, covering the surface during the continuance of the shocks. The ice-reports show large numbers of bergs north of lat. 42°, and between long. 47° and 53°.

On June 1, M. Hervé Mangon, President of the Council of the French Central Meteorological Office, read the ninth Annual Report of the work of the Office (see Nature, vol. xviii. p. 96). It shows satisfactory evidence of continued energy and progress in all departments of the service. No less than 154 telegraphic reports are received daily from Europe and Algeria, and 41 telegraphic summaries and weather forecasts are issued, including one to a London daily paper. The success claimed for the forecasts is 88 per cent., and for the warnings of storms 82 per cent., being a greater success than in any previous year. There are 153 climatological stations (including 12 observatories) taking not less than 3 observations daily, in addition to a large number of minor stations. The Office is also actively engaged in collecting observations made at sea, and received upwards of 500 log-books during the past year. This branch is encouraged by the presentation of medals, awarded by the Association Scientifique to the best observers. M. Hervé Mangon reviewed the work of the various observatories, and referred especially to the investigations of M. Renou, at St. Maur, who has just completed an inquiry into the rainfall for the last 200 years, and is finishing a work on the climate of Paris, on which he has been engaged for 40 years. Reference is also made to the reports now received daily from America and the Atlantic, of which our own Meteorological Office bears half the cost. The telegrams are regularly published in the French *Bulletin International.* The other half of the expense of these telegrams is borne by a lady whose name is not generally known. M. Hervé Mangon spoke at great length of the damage caused yearly by the inundations and mountain torrents, and of the advantage of planting the mountain declivities with trees or shrubs. One of the chief features of the past year is the completion of the Observatory of the Pic de l'Aigoual, in the department of Gard, which has been established in the interest of forest meteorology. A series of experiments is to be made on the influence of various kinds of soil and vegetation in storing the water caused by rainfall, and on the time necessary for its evaporation and percolation.

THE *Bollettino Mensuale* of the Italian Meteorological Society for May contains the report of the first annual meeting of the Council, held on April 14. The principal matters discussed were: the co-operation of the Italian Navigation Societies; the development of the service of medical meteorology at Naples and other towns; and the preparation of a map of the globe showing all the stations of the Society both at home and abroad. It was proposed to encourage observations of the temperature of the surface of the ground, and to publish the results of these and other observations already collected. The second annual meeting of the Council was fixed for the autumn.

THE Observatory at Batavia has just published vol. vii. of its series, containing the magnetic observations (only), from September 1883 to December 1885, together with the results from July 1882 to December 1885, prepared under the direction of Dr. van der Stok. The observations show a well-defined decrease of the declination in 1884–85, at the rate of nearly two minutes a year, and a decrease of the horizontal force at the rate of 0·00012 a year. The vertical force has continued to increase, and the dip shows a progressive value of about 7′·5 a year. It is intended in future to issue, yearly, a volume containing both the magnetic and the meteorological observations, but the publication of the meteorological observations for the years 1883–85, and the discussion of the results for the twenty years during which the observations have been made, are indefinitely delayed, owing to pressure of other work.

MR. CLEMENT L. WRAGGE, the newly-appointed Government Meteorologist of Queensland (see NATURE, vol. xxxv. p. 229), has published the meteorological synopsis of the Brisbane Observatory and rainfall reports for the colony for January to March 1887, and also his report of the inspection of the stations. The inspection disclosed the thorough disorganization of many of the stations. For instance, at Cooktown, a station of the first class, the spirit thermometer had the enormous error of 15°, owing to the volatilization of the alcohol. At Normanton, another first-class station, the shade thermometers were "exposed" in the sitting-room. It is unnecessary to multiply instances, and we merely quote Mr. Wragge's concluding remark that "the majority of the meteorological records and results already published are unreliable and valueless." We hope with him that the new system will gradually attain a position of excellence equalling that which obtains in this country.

WE have received a copy of the lecture delivered lately by Dr. Orme Masson, the Professor of Chemistry in the University of Melbourne, on the first occasion on which he addressed himself publicly to a Melbourne University audience. The subject is, "The Scope and Aims of Chemical Science, and its Place in the University." Dr. Masson has a clear, fresh, and vigorous style, and in this lecture he brings out with much force the part which chemistry has played in modern material progress, and its fitness to serve as an instrument of intellectual culture. He expresses a hope that there may always be at the University of Melbourne a small band of students devoting the bulk of their time for a few years to chemical research. The University, he says, will soon have "well-equipped laboratories, not only for the practical instruction of large classes of medical students and others, but for the accommodation of those specialists who go further in the work, requiring to be provided with the more elaborate paraphernalia of experimental science."

AT the opening meeting, on April 19, of the Royal Society of Tasmania for the session of 1887, Mr. R. M. Johnston read an interesting paper on the question, "How far can the general death-rate for all ages be relied upon as a comparative index of the health or sanitary condition of any community?" The object of the paper was to demonstrate that the general death-rate of any one place, although in itself due to a combination of many causes, may be taken as a fairly trustworthy local index to health and sanitary condition, but that it is a most faulty index as regards the comparative health and sanitary condition of different localities. The latter fact he attributed mainly to the extreme variability in the proportions of persons living in different places under the principal age groups.

MR. E. STANFORD, of Charing Cross, has just issued three volumes of his series of Tourists' Guides. They are Guides to Suffolk, Wiltshire, and the Wye and its neighbourhood; the first by Dr. J. E. Taylor, the second by Mr. R. N. Worth, and the third by Mr. G. Phillips Bevan. Each of the volumes has been carefully compiled, and is worthy of the useful and well-known series to which it belongs.

AN interesting collection of Indian antiquities is now being exhibited at the Albert Hall. It includes, among other objects, a large number of Palæolithic and Neolithic implements, remains from Indian grave-mounds of the prehistoric aborigines, copies of rude cave pictures and marks on rocks, and Buddhist sculptures and terra-cotta seals found among the ruins of Kusinagara. The objects exhibited form part of a collection made in India by Mr. A. C. Carlyle, late of the Archæological Survey of India.

THE remains of a cemetery belonging to the age of the Gauls have recently been discovered in Paris, in the old Faubourg St. Germain, at the corner of Rocroi and Bellechasse Streets. Fifty-two tombs have been found, with skeletons, most of which are skeletons of women and children. Only twelve are skeletons of men. Many weapons and implements have also been unearthed: swords, lances, shields, and bronze and iron instruments of all descriptions.

THE grasshopper plague is giving serious trouble in Algeria this year. The efforts made to destroy the eggs have proved useless. In one district 50,000 gallons have been collected and burned. This represents the destruction of 7,250,000,000 insects.

IT is observed in the French army that diseases of the heart are very common. In a recent study of this subject, certain military doctors show that they arise from the fatiguing duties imposed on recruits, at an age when, generally, the development of the body is not in harmony with that of the heart, being either in advance of it or behind it. In the latter case, there is hypertrophy of growth; in the former, insufficiency (the more common occurrence). An instance is given in which a regiment in garrison in the West, in 1880, had on an average twelve to fifteen men per thousand invalided annually (the normal figure for the French army), of which number two or three had hypertrophy of the heart. A colonel came to the regiment who had very faulty notions as to the amount of drill and fatigue the men could stand. By September 1883, the number of heart-invalids had risen steadily to twenty-two out of forty-five (*i.e.* about one in two).

A BRILLIANT discovery is announced in the current number of the *Berichte der Deut. Chem. Ges.* by Dr. Theodor Curtius, who has succeeded in preparing the long-sought-for hydride of nitrogen, $(NH_2)_2$, amidogen, diamide, or hydrazine, as it is variously termed. This remarkable body, which has hitherto baffled all attempts at isolation, is now shown to be a gas, perfectly stable up to a very high temperature, of a peculiar odour, differing from that of ammonia, exceedingly soluble in water, and of basic properties. In the course of his work upon the diazo-compounds of the fatty series, Dr. Curtius treated diazo acetic ether with hot, strong potash, and obtained the potassium salt of a new diazo-fatty acid, which on addition of mineral acids yielded yellow tabular crystals of the free diazo-acid. On digesting the yellow aqueous solution of this acid with very dilute sulphuric acid the colour disappeared without the usual evolution of nitrogen; and on cooling a magnificently crystalline substance separated out, which was shown by analysis to be no other than the sulphate of amidogen, $(NH_2)_2 . H_2SO_4$. These crystals remained unchanged at 250°, but on strongly heating over a flame melted with explosive evolution of gas and deposition of sulphur. On warming this salt with potash solution the free diamide, $(NH_2)_2$, was expelled as a gas which changed red litmus into blue, and rendered itself evident by its irritating odour. The gas fumed in contact with hydrochloric acid forming the hydrochloride, and on leading it into sulphuric acid re-formed the sulphate. It possessed energetic reducing properties, reducing Fehling's and ammoniacal silver solutions in the cold, gave a dense red precipitate with neutral copper sulphate, and formed crystalline compounds with aromatic aldehydes and ketones. It is very seldom that chemistry is enriched by the discovery of a new gas, and the intrinsic value of the isolation of amidogen to both organic and inorganic chemistry renders the communication of Dr. Curtius one of exceptional and of far more than passing interest.

MEASUREMENTS have lately been made by Messrs. A. von Ettingshausen and W. Nernst, upon the Hall effect manifested in different metals. They have found that tellurium far surpasses bismuth in its power, hence they think that the Hall effect is connected with the thermo-electric properties of the metals. The effect is least in tin. Taking this as unity, the effects in other metals are relatively as follows: platinum, 6; copper, 13; gold, 28; silver, 21; palladium, 29; cobalt, 115; iron, 285; nickel, 605; carbon, 4400; antimony, 4800; bismuth, 252,500: tellurium, 13,250,000. The sign of the effect is positive in the case of cobalt, iron, steel, antimony, and tellurium, also lead, zinc, and cadmium. It is negative in all the others.

THE mining engineer, M. Dahll, who has been examining the north of Norway on behalf of the Norwegian Government, states in his Report that all the rivers in the interior of Finnmarken, a district of fifty Norwegian square miles, carry gold. The metal is found in sand contained in little hollows, which by their shape prevent its being washed away by the water. The weight of the gold grains varies from 10 milligrammes to 1 gramme. Platinum is also found occasionally.

DURING the cutting of peat in a moss at Vevang, near the town of Christiansund, in the north-west of Norway, the workmen recently dug out a log of oak over 12 feet in length, and about 4 feet in diameter. It was found at a depth of 9 feet. The trunk and root of a great oak-tree were unearthed in the same moss some years ago, so we may conclude that there was once an oak forest in this spot. The remains of the oak were found below a layer in the bog in which remains of firs are often found.

IN the new number of the Proceedings of the Geologists' Association there is a paper by Dr. H. Hicks on the explorations which he, in conjunction with others, has carried on in the Ffynnon Beuno and Cae Gwyn Caverns in North Wales. He has no doubt whatever as to the accuracy of the conclusions presented by him in his previous papers on the subject. "I am," he says, "perfectly convinced by the evidence found during the exploration of these caverns that they must have been occupied by man and the animals before the climax of the Ice age; also that the thick stalagmite was formed some time during that age; that this was broken up by marine action during the submergence; and that the caverns were afterwards completely covered over by materials deposited from floating ice. There seems, therefore, to be every reason to suppose that man and the so-called Pleistocene animals arrived in this country in advance of the glacial conditions, and that their migrations were mainly from northern and north-western directions."

THE first number of the *Technology Quarterly* has been sent to us. This new American periodical is published by a Board of Editors chosen from the senior and junior classes of the Massachusetts Institute of Technology, representing, as far as possible, all the departments of the Institute. A large amount of original work is done in the Institute every year by advanced students, and it is thought by the editors that the *Quarterly* will be an acceptable journal if it contains nothing more than the results of the original investigations made in the chemical, physical, mining, mechanical, and biological laboratories, and also in the departments of civil engineering and architecture. But it is expected that the *alumni* of the Institute will be glad of this medium for recording their investigations and the results of their practical work. Among the contents of the present number are articles on "The Control of Rivers and the Prevention of Floods," "The Efficiency of Small Electromotors," "The Use of the Aneroid Barometer in Western Massachusetts by the U.S. Geological Survey," and "The Constitution of Benzol."

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented respectively by Mrs. Slatter and Mrs. Beeston; a Lesser White-nosed Monkey (*Cercopithecus petaurista*) from West Africa, presented by Miss Kate Wood; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Miss Dudding; a Common Squirrel (*Sciurus vulgaris*), British, presented by Mrs. Dick; a Virginian Deer (*Cariacus virginianus*) from North America, presented by Mr. Tom Jay; three Kestrels (*Tinnunculus alaudarius*), British, presented by Dr. J. W. Trentler; two Blue Titmice (*Parus cæruleus*), British, presented by Mrs. Francis L. Barlow; a Blue-eyed Cockatoo (*Cacatua ophthalmica*) from New Britain, presented by Mr. W. H. Fellows; four Horned Vipers (*Vipera cornuta*), three Dwarf Chameleons (*Chamæleon pumilus*), a Many-spotted Snake (*Coronella multimaculata*), a Rufescent Snake (*Leptodira rufescens*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Crowned Horned Lizard (*Phrynosoma coronatum*) from California, presented by Mr. Duff Gordon; a Pig-tailed Monkey (*Macacus nemestrinus*) from India, four Herons (*Ardea cinerea*), six Night Herons (*Nycticorax griseus*), European, deposited; a West African Python (*Python sebæ*) from West Africa, purchased; a Mesopotamian Fallow Deer (*Dama mesopotamica*), two Japanese Deer (*Cervus sika*), two Collared Fruit Bats (*Cynonycteris collaris*) born in the Gardens; two Yellow-legged Herring Gulls (*Larus cachinnans*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE GREAT SOUTHERN COMET, 1887 *a*.—Mr. Chandler continuing his researches on the orbit of this comet (referred to in last week's NATURE), gives, in No. 157 of the *Astronomical Journal*, the following elements deduced from the Cape and Adelaide observations published in the *Monthly Notices* for March last:—

$$T = 1887 \text{ January } 11{\cdot}230 \text{ G.M.T.}$$

$$\left.\begin{aligned}\omega &= 63^\circ\ 36'{\cdot}0\\ \Omega &= 337\ \ 42{\cdot}8\\ \iota &= 137\ \ \ 0{\cdot}0\end{aligned}\right\} \text{ True Equinox.}$$

$$\log q = 7{\cdot}73892$$

The observations on which this orbit depends differ widely from the estimates of position at Cordoba and Windsor, which Mr. Chandler had used in his previous computations; and the elements now found are in fair agreement with those deduced by Mr. Finlay from the Cape observations of January 22, 25, and 28 alone, which are published in the above-mentioned number of the *Monthly Notices*.

THE COMPANION OF SIRIUS.—Prof. A. Hall gives, as the mean results of his observations during the present year (*Astronomical Journal*, No. 157): Epoch 1887·238; position-angle, 24°·18; and distance, 6″·508.

A SHORT METHOD OF COMPUTING REFRACTIONS FOR ALL ZENITH DISTANCES.—In continuation of his paper in *Astronomische Nachrichten*, No. 2768 (NATURE, vol. xxxv. p. 329), the application of which was limited to zenith distances less than 45°, Mr. Schaeberle, of Ann Arbor, U.S.A., in No. 2788 of the same publication, gives his method for the computation of refractions, with Bessel's constants, for 45° to 77° of zenith distance, and for zenith distances greater than 77°, with an accuracy sufficient for practical purposes. Starting from Bessel's expression $r = \alpha\beta^A\gamma^\lambda \tan z$, Mr. Schaeberle finds that Δr (the quantity to be added to the mean refraction r_0) can be represented only by $\Delta r = r_0 F + \epsilon \frac{\Delta\gamma}{\gamma}$, between the limits $z = 45°$ and $z = 77°$. In this expression $F = \frac{\Delta\beta}{\beta} + \frac{\Delta\gamma}{\gamma}$ and $\epsilon = r_0 (\lambda - 1)$. For zenith distances greater than 77°, the final equation becomes $\Delta r = r_0 F + \epsilon \left(F - \frac{\sigma}{\gamma}\right)$, where $\sigma = 0·9 \frac{\Delta\beta}{\beta}$. The requisite quantities can evidently be easily tabulated, and the computer is thus provided with a very convenient method for calculating refractions which will not materially differ from those deduced directly from Bessel's Tables.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 JUNE 26—JULY 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 June 26.

Sun rises, 3h. 46m.; souths, 12h. 2m. 29·7s.; sets, 20h. 18m.; decl. on meridian, 23° 22′ N.: Sidereal Time at Sunset, 14h. 36m.

Moon (at First Quarter on June 28) rises, 9h. 35m.; souths, 16h. 38m.; sets, 23h. 29m.; decl. on meridian, 8° 53′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury ...	5 51	13 51	21 51	21 6 N.
Venus	7 39	15 11	22 43	16 40 N.
Mars	2 39	10 55	19 11	23 23 N.
Jupiter... ...	14 1	19 20	0 39*	8 51 S.
Saturn... ...	5 20	13 23	21 26	21 33 N.

* Indicates that the setting is that of the following morning.

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. ° °
27 ...	10 Virginis ...	6	23 36	near approach	199 —
July.					
1 ...	ξ^1 Libræ	6	0 52	near approach	201 —
1 ...	η Libræ	6	21 23	22 4	19 320

June.	h.	
29 ...	17 ...	Jupiter in conjunction with and 3° 40′ south of the Moon.
July.	h.	
1 ...	10 ...	Mercury at greatest elongation from the Sun, 26° east.
2 ...	9 ...	Sun at greatest distance from the Earth.

Meteor-Showers.

	R.A. °	Decl. °	
Near σ Herculis	253	47 N.	Swift meteors.
δ Cygni...	294	39 N.	Slow meteors.
ϵ Delphini	305	9 N.	
Between β and γ Cephei.	330	77 N.	

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	June 27,	23 54	*m*
			July 2,	23 33	*m*
R Piscium	1 24·8	2 18 N.	June 26,		*M*
S Ursæ Majoris ...	12 39·0	61 43 N.	July 1,		*m*
W Virginis	13 20·2	2 48 S.	,, 2,	22 0	*m*
U Ophiuchi... ...	17 10·8	1 20 N.	June 30,	23 25	*m*
U Sagittarii... ...	18 25·2	19 12 S.	,, 28,	0 0	*M*
β Lyræ...	18 45·9	33 14 N.	,, 30,	0 0	*M*
R Lyræ	18 51·9	43 48 N.	July 1,		*m*
S Vulpeculæ ...	19 43·8	27 0 N.	June 26,		*m*
η Aquilæ	19 46·7	0 43 N.	July 2,	0 0	*m*
S Sagittæ	19 50·9	16 20 N.	June 27,	23 0	*M*
T Aquarii	20 44·0	5 34 S.	,, 26,		*M*
W Cygni	21 31·8	44 52 N.	,, 29,		*m*
δ Cephei	22 25·0	57 50 N.	,, 28,	23 0	*M*

M signifies maximum; *m* minimum.

THE ZOOLOGICAL SOCIETY OF LONDON.

A GENERAL meeting of the Zoological Society of London took place on the afternoon of Thursday, the 16th inst. In celebration of the fiftieth anniversary of Her Majesty's reign the meeting was held on the lawn of the Society's Gardens, which was reserved for the occasion. A very large number of the members and their friends were present.

After the meeting there was a garden party, the visitors being received by the President, Prof. Flower, F.R.S., and the Secretary, Dr. P. L. Sclater, F.R.S. Among those present during the afternoon were the following:—The Queen of Hawaii and Princess Liliuokalani, His Highness the Thakore Sahib of Limbdi, His Highness the Prince Devawongse, the Maharajah of Bhurtpore, the Earl of Buckinghamshire, the Earl of Cawdor, Lord Wantage, the Earl of Lauderdale, the Earl of Kilmorey, the Earl of Wharncliffe, Lord Coleridge, Lord Walsingham, the Dowager Marchione s of Tweeddale, Lord and Lady Thring, Sir James Paget, Sir Harry Lumsden, Sir Richard Pollock, Sir Joseph Hooker, Prof. Huxley, Capt. Gouglas Dalton, and the following members of the Council of the Zoological Society:— Lord Abinger, Mr. W. T. Blanford, F.R.S., Mr. H. E. Dresser, F.R.S., Mr. C. Drummond, F.R.S., Colonel J. A Grant, F.R.S., Dr. A. C. L. Günther, F.R.S., Dr. E. Hamilton, F.R.S., Mr. E. W. H. Holdsworth, Dr. St. George Mivart, F.R.S., Prof. A. Newton, F.R.S., Mr. Henry Pollock, Mr. H. Saunders, F.L.S., Mr. J. Travers Smith, and Surgeon-General L. C. Stewart.

At the general meeting the President presented the silver medal of the Society to the Maharajah of Kuch-Behar. In doing so he said that His Highness had been good enough to present to the Society a fine specimen of an Indian rhinoceros.

The Maharajah of Kuch-Behar, in reply, said that he would be happy to supply specimens of such animals as the Society might desire to possess, so far as it was in his power to do so.

Prof. Flower then delivered the following address:—

Nowhere has the progress which the world has made during the fifty years of Her Majesty's reign, the completion of which we are now happily celebrating, been more strikingly manifested than in the advance of that so-called "natural knowledge" for the improvement of which our Royal Society was instituted more than two centuries ago. Although there have been, without doubt, immense strides in other directions— in morals, in art, in historical and literary criticism—I venture to say that none of these can be compared with the marvellous progress that has been made in scientific knowledge and scientific methods.

The tangible results that have followed the practical applications of mechanics, physics, and chemistry have so deeply affected the material interests of mankind, that the progress of these branches of knowledge may seem to put into the shade the wonderful changes that have taken place in the kindred sciences. Nevertheless, I think we may safely say that zoology, in a certain sense one of the oldest of human studies, has in these latter times undergone a new birth, which has not only changed the standpoint from which we view the special objects of our studies, but has also spread its influence far and wide, and profoundly modified our conceptions on many questions at first sight entirely remote from its sphere. The universal abandonment of the doctrine of fixity of species, which was an article of faith with almost every zoologist in 1837, has introduced new interests, as well, it must be confessed, as new difficulties, the extent of which we are only beginning to appreciate. The definite systems of classification and methods of nomenclature on which our fathers relied utterly fail before the wider field of vision which it is the privilege, as well as the embarrassment, of the present generation of zoologists to realize.

But it is no part of my intention, in the brief space of time for which I shall ask your patience, to attempt to give a history of the recent advances of zoological science in general, but only, as requested by your Council, to say a few words on the progress of the particular institution established for its cultivation in which we are personally interested, and the duration of which is so nearly cotemporaneous with that of Her Majesty's reign.

Before this Society was founded there was no distinct organization in the country devoted solely to collecting, recording, and discussing the facts upon which zoological science rests. The dignified parent of all our scientific Societies, the Royal, certainly undertook, as it does still, the discussion of many zoological subjects; but it could not be expected to treat them in any detail. The Linnean was a Society of great respectability, devoted solely to biological research, both zoological and botanical, already nearly forty years of age, and possessed of all the usual appurtenances of a scientific organization—meetings, library, and collections for reference. I cannot help thinking that if its leading Fellows had, at that time, displayed more energy, it might have kept in its hands the principal direction of the biological studies of the country, instead of allowing what has since proved so formidable a rival to spring up, and to absorb so large a portion of its useful functions. However, for reasons which it is perhaps not worth while to inquire into now, it did not supply all the needs of the lovers of zoology; and in the year 1826 an active and zealous band united together, and, as the Charter tells us, "subscribed and expended considerable sums of money for the purpose" of founding the Zoological Society of London.

The leading spirit of this band was Sir Stamford Raffles, then just returned from the administration of those Eastern islands of which the history, both natural and political, will ever be intimately associated with his name. He was chosen for the office of President, but his death, on July 4, 1826, deprived the Society, while yet in its infancy, of his valuable services even some years before it acquired its Charter of Incorporation. In this deed, dated March 27, 1829, Henry, Marquis of Lansdowne, is named as the first President of the chartered Society, Joseph Sabine as the first Treasurer, and Nicholas Aylward Vigors the first Secretary.

The Society appears to have acquired great popularity in a surprisingly short time. The first printed list of Members that I can discover (dated January 1, 1829) contains the names of 1294 ordinary Fellows and 40 honorary and corresponding Members. The list is an interesting one from the number of names it includes of persons eminent either in science, art, literature, politics, or social life: indeed, there were not many people of distinction in the country at that time who are not to be found in it.

A piece of ground in the Regent's Park having been obtained from the Government at little more than a nominal rent, the Gardens were laid out, and opened in 1828, during which year 98,605 visitors are recorded as having entered. In the following (the first complete) year there were as many as 189,913 visitors, and this number was increased in 1831 to 262,193.

While the menagerie of living animals was being formed in the Regent's Park, the Officers and Fellows of the Society were also engaged in establishing a Museum of preserved specimens, which soon assumed very considerable dimensions. A Catalogue printed as early as the year 1828 contains a classified list of 450 specimens of Mammalia alone; and it continued for many years to attract donations from travellers and collectors in all parts of the world, and became of great scientific importance, inasmuch as it contained very many types of species described for the first time in the publications of the Society. It was at first lodged in rooms in the Society's house in Bruton Street; but these becoming so crowded as to present the "confused air of a store rather than the appearance of an arranged museum," premises were taken in 1836 in Leicester Square; the same which were formerly occupied by the museum of John Hunter before its removal to the College of Surgeons. At this time the Museum is reported to have contained as many as 6720 specimens of vertebrated animals, and numerous additions were still being made both by dónations and by purchase. The rooms in Leicester Square being found inconvenient for the purpose, it was finally resolved, after considerable discussion of various sites, to transfer the collection to the Gardens in the Regent's Park; and in 1843 the building which is now occupied as a lecture-room on the upper floor and a store-room below was constructed and fitted up for its reception.

Although the Museum was at one time looked upon as a very important part of the Society's operations, being spoken of as "the centre of the Society's scientific usefulness" (Report of Council, 1837), and one upon which considerable sums of money were spent, it was afterwards a cause of embarrassment from the difficulty and expense of keeping it up in a state of efficiency; and when the Zoological Department of the British Museum acquired such a development as to fulfil all the objects proposed by the Society's collection, the uselessness of endeavouring to maintain a second and inferior zoological museum in the same city became apparent, and in 1856 it was, as I think very wisely, determined to part with the collection, the whole of the types being transferred to the National Museum, and the remaining specimens to other institutions where it was thought their value would be most appreciated.

Another enterprise in which the Fellows of the Society were much interested in its early days was the Farm at Kingston, the special object of which was thus defined :—"It will be useful in receiving animals which may require a greater range and more quiet than the Gardens at the Regent's Park can afford. It is absolutely necessary for the purpose of breeding and rearing young animals, and giving facilities for observations on matters of physiological interest and research, and, above all, in making attempts to naturalize such species as are hitherto rare or unknown in this country." The Farm, however, apparently not fulfilling the objects expected of it, and being a source of expense which the Society could not then well afford, was gradually allowed to fall into neglect, and finally abandoned in 1834.

The mention of this establishment, however, causes me to allude to one of the objects on which the Society laid considerable stress at its foundation, and which is defined in the Charter as "the introduction of new and curious subjects of the animal kingdom," but which, as may be gathered from the Annual Reports of the Council and from other documents, meant not only the temporary introduction of individuals for the purpose of satisfying curiosity about their external characters and structure, but also the permanent domestication of foreign animals which might become of value to man, either for their utility in adding to our food-supplies or for the pleasure they afforded by their beauty.

Abundant illustrations of the vanity of human expectations are afforded by the details of the hopes and disappointments recorded in the Reports of the Society relating to this subject. It is mentioned in the Report of the year 1832 that "the armadillo has three times produced young, and hopes are entertained of this animal, so valuable as an article of food, being naturalized in this country." More than fifty years have passed, and British-grown armadillo has not yet appeared upon the menu-cards of our dinner-tables. At one time the South-American curassows and guans were confidently looked upon as future rivals to our barn-door fowls and turkeys. Various species of pheasants and other game-birds from Northern India, collected and imported at great expense, were to add zest and variety to the battue of the English sportsman. The great success which for many years attended the breeding of giraffes in the Gardens not unnaturally led to the expectation that these beautiful creatures might become denizens of our parks, or at all events a source of continued profit to the Society; and it is possible that some who are here now may have been present at the feast for which an eland was sacrificed, amid loudly-uttered prognostications that the ready acclimatization of these animals would result, if not in superseding, at least in providing a change from, our monotonous round of mutton, beef, and pork. Unfortunately for these anticipations, no giraffe has been born in the Gardens during the last twenty years, and elands are still far too scarce to be killed for food of man in England.

It is well that these experiments should have been tried; it may be well, perhaps, that some of them should be tried again when favourable opportunities occur; but it is also well that we should recognize the almost insuperable difficulties that must attend the attempt to introduce a new animal able to compete in useful qualities with those which, as is the case with all our limited number of domestic animals, have gradually acquired the peculiarities making them valuable to man, by the accumulation of slight improvements through countless generations of ancestors. While all our pressing wants are so well supplied by the animals we already possess, it can no longer pay to begin again at the beginning with a new species. This appears to be the solution of the singular fact, scarcely sufficiently appreciated, that no addition of any practical importance has been made to our stock of truly domestic animals since the commencement of the historic period of man's life upon the earth.

I now turn to the history of one of the most important features of the Society, the scientific meetings. In the early days of the Society there was only one class of general meetings for business of all kinds; and the exhibition of specimens and the communication of notices on subjects of zoological interest formed part of the ordinary proceedings at those meetings. The great extent, however, of the general business was soon found to interfere with such an arrangement. The number of the elections and of the recommendations of candidates, the reports on the progress of the Society in its several establishments during each month, and other business, were found to require so much time as to leave little for scientific communications, and the Council saw with regret that these were frequently and necessarily postponed to matters of more pressing but less permanent interest. To obviate this inconvenience and to afford opportunities for the reception and discussion of communications upon zoological subjects, the Council had recourse to the institution of a "Committee of Science and Correspondence," composed of such Members of the Society as had principally applied themselves to science; at the meetings of which communications upon zoological subjects might be received and discussed, and occasional selections made for the purpose of publication.

The first meeting of the Committee took place on the evening of Tuesday, November 9, 1830, at the Society's house in Bruton Street, when a communication was received upon the anatomy of the urang utan by a young, and then unknown, naturalist, Richard Owen by name, the first of that long series of memoirs, extending over a period of more than fifty years, the publication of which in our Transactions has done so much to advance the knowledge of comparative anatomy and to give an illustrious place to their author in the annals of science.

Among the names of others who are mentioned as having taken part in the business of the Committee during the first year of its existence, either by their actual presence or by forwarding communications, are N. A. Vigors, W. Yarrell, J. E. Gray, J. Gould, E. T. Bennett, Andrew Smith, T. Bell, W. Martin, Joshua Brookes, W. Kirby, W. H. Sykes, Marshall Hall, W. Ogilby, John Richardson, and B. H. Hodgson, who, I am happy to say, is with us at the meeting to day.

The Committee continued in existence for two years, having met for the last time on December 11, 1832. The success of its meetings was so great that it was thought desirable to make an alteration in the by-laws, by which the meetings of the Committee were replaced by the "General Meetings of the Society for Scientific Business." The first of these meetings took place on Tuesday, January 8, 1833, and they have continued to be held on two Tuesdays in each month during the season to the present time. As long as the Society retained its house in Bruton Street, the meetings were held there. In 1843 the Society took another house, which it occupied for forty-one years, No. 11 Hanover Square; but its needs having outgrown the accommodation afforded there, it removed in 1844 to the far more spacious and commodious premises, in No. 3 of the same square, which we at present occupy. These meetings of the Society, which are open to all the Fellows and to friends introduced by them, have exercised a considerable influence upon the progress of zoological knowledge, not only by the reading and discussing of communications formally brought before them, but also by the interchange of ideas at the informal social gatherings over the coffee-table in the library afterwards, which have great value as affording a common meeting-ground and bond of union for all the working zoologists of the country, as well as of many visitors from foreign lands.

The more important scientific communications to these meetings have from the commencement been published in the form of quarto Transactions and octavo Proceedings, which constitute a series of inestimable importance both for the value of the material contained in them and for the excellence of the illustrations of new or rare forms of animal life with which they are embellished. In later times they have also formed a vehicle for communicating to the world the important results obtained from the dissection of animals which have died at the Gardens, and which, since the establishment of the office of Prosector in 1865, have been systematically used for this purpose.

In connexion with the scientific meetings mnst be mentioned the Library, the first formation of which is described in the Report of the Council for the year 1837, and which has been steadily growing ever since by donations of books, by exchange of publications with other learned Societies, and by judicious annual expenditure of money, to be one of the best-selected, well-arranged, and most accessible collections of works of reference that it is possible for the zoological student to enjoy. Its value has been greatly increased by the publication within the past month of an excellent Catalogue, which contains the titles of about 6560 publications.

The most recent addition to the functions that the Society has undertaken with a view to carry out the purposes of its foundation is the publication of an Annual Record of Zoological Literature, containing a summary of the work done by British and foreign naturalists in the various branches of zoology in each year, a publication of the utmost value to the working zoologist. Such a Record has been carried on for some years past by a voluntary association of naturalists, but, owing to the difficulties met with in obtaining sufficient support, it was in danger of being abandoned, until the Council, after the full consideration which the importance of the subject deserved, resolved to take it in hand as part of the operations of the Society.

The Society has, however, not only been mindful of advancing scientific knowledge—it has also endeavoured to spread some of this knowledge in a popular manner by means of lectures. In former years these were only given in an occasional manner; but the liberal bequest of Mr. Alfred Davis to the Society in 1870 has enabled the Council to undertake a more regular and systematic method of instruction; and the Fellows and others have had every summer for several years past the opportunity of hearing many of our most eminent naturalists and able expositors upon subjects which they have made especially their own. I must, however, confess that the interest taken by the Society generally in these lectures has not quite equalled the expectations that were raised when the question of establishing them was first brought before the notice of the Council.

Although, as will be seen by a consideration of the various subjects which I have already referred to, the Society has a wide sphere of operations and many methods by which the objects of its founders are carried out, it is undoubtedly the maintenance of the menagerie of living animals in the Gardens where we are now assembled, by which it is most known both to the public as well as to a large number of our Fellows. It will be well, therefore, before concluding, to add a few words upon some points of interest connected with the past history and present condition of this branch of the Society's operations, the one which is at the same time the largest source of its revenue and cause of its expenditure.

The collection and exhibition of rare and little-known living animals has long been a subject of interest and instruction in civilized communities, and in many countries either the State or the Sovereign has considered it as part of their duty or privilege to maintain a more or less perfect establishment of the kind.

Before the Zoological Society was formed, the "lions" at the Tower had been for centuries a national institution; and it may be interesting to those who derive pleasure in tracing the links between the present and the past, to be reminded that our collection is in some measure a lineal continuation of that time-honoured institution, as it appears from the Reports of the Council that in the year 1831 His Majesty King William the Fourth "was graciously pleased to present to the Society all the animals belonging to the Crown lately maintained at the Tower." It is also recorded that in the previous year His Majesty had made a munificent donation of the whole of the animals belonging to the Royal Menagerie kept in Windsor Park. This may perhaps be the place to mention that, in the Report read April 1837, the Council "had the gratification to call the special attention of the members to a donation from Her

Royal Highness the Princess Victoria," consisting of a pair of those pretty and interesting little animals the Stanley Musk-deer. During the fifty years that have elapsed since this first-recorded mark of interest in the Society on the part of her present Majesty, the Queen and her family have never failed to show their regard for its welfare whenever any opportunity has arisen, of which the acceptance of the Presidency by the late Prince Consort, on the death of the Earl of Derby in 1851, was one of the most signal instances. The advantages which the Society has received from the numerous donations to the Menagerie, and the constant kindly interest shown in its general progress by H.R.H. the Prince of Wales, are so continually before the observation of the Fellows, that I need scarcely do more than allude to them here, beyond stating that in no year of the Society's existence has the number of visitors to the Gardens, or the Society's income, been so great as in 1876, when the large collection of animals brought from India by His Royal Highness formed the special object of attraction.

Except for the collection, necessarily of limited extent, exhibited in the Tower, and a few others having their origin in commercial enterprise (as Mr. Crosse's menagerie at Exeter Change, and the various itinerant wild-beast shows), there were, before the foundation of the Society's Gardens, little means in the country of gaining knowledge of the strange forms of exotic animal life with which the world abounds. An extensive, well-arranged, and well-kept collection, where the circumstances of exhibition were more favourable than in the institutions just referred to, seemed then to fulfil a national need, as the rapidly acquired popularity of the Society already alluded to testifies. Indeed, when we consider the amount of enjoyment and instruction which has been afforded to the 24,572,405 visitors who are registered as having entered our Gardens from their first opening in 1828 to the end of last year, it is easy to realize what a loss the country would have sustained if they had not existed. There was a period, it is true, in which they fell rather low in popular favour, the record of 1847 showing both the smallest number of visitors and the lowest income of any year in the Society's existence. A new era of activity in the management of the Society's affairs was then happily inaugurated, which resulted in a prosperity which has continued ever since, with only slight fluctuations, arising from causes easy to be understood—a prosperity to which the scientific knowledge, zeal, and devotion to the affairs of the Society of our present Secretary, ably seconded in all matters of detail by the Resident Superintendent, have greatly contributed.

One of the greatest improvements which have been gradually effected in the Gardens in recent years is the erection of larger, more commodious, and more substantial buildings for the accommodation of the animals than those that existed before. A few examples will suffice to illustrate the successive steps that have been taken in this direction. The primary habitation of the lions and other large feline animals was the building on the north side of the canal, which many of us may remember as a Reptile-house, and which has been lately restored as a dwelling-place for the smaller Carnivora. The Council Reports of the period frequently speak of the bad accommodation it afforded to the inmates, the consequent injury to their health, and the disagreeable effects on visitors from the closeness of the atmosphere. In September 1843, the terrace, with its double row of cages beneath, was completed; and the Report of the following spring, speaking of this as "one of the most important works ever undertaken at the Gardens," congratulates the Society upon the fact that the anticipations of the increased health of this interesting portion of the collection, resulting from a free exposure to the external air, and total absence of artificial heat, have been fully realized. The effects of more air and greater exercise were indeed said to have become visible almost immediately. Animals which were emaciated and sickly before their removal became plump and sleek in a fortnight after, and the appetites of all were so materially increased that they began to kill and eat each other. This, however, led to an immediate increase in their allowance of food, since which time, it is stated, no further accidents of the kind have occurred. As this structure, looked upon at that period as so great an improvement upon its predecessors, still remains, though adapted for other inmates, we all have an opportunity of contrasting the size of its dens and the provision it affords generally for the health and comfort of the animals and the convenience of visitors, with those of the magnificent building which superseded it in 1876.

In the Report of the year 1840 it is stated that the only work of considerable magnitude undertaken since the last anniversary was the erection of the "New Monkey-house," and the Council speak with great satisfaction of the substantial nature of the structure and the superior accommodation which its internal arrangements are calculated to afford to its inmates.

Many of us may remember this building, which stood on the space now cleared in the centre of the Gardens. Twenty-four years after its erection, in their Report dated April 1864, we find the Council speaking of it as "what is at present perhaps the most defective portion of the Society's Garden establishment," and the erection of a second "New Monkey-house" was determined upon. This is the present light and comparatively airy and spacious building, the superiority of which over the old one in every respect is incontestable.

Up to the year 1848 the only attempt which had been made to familiarize the visitors with the structure and habits of animals of the class Reptilia was by the occasional display of a pair of pythons, which were kept closely covered in a box of limited dimensions in one of the smaller Carnivora-houses. In 1849 the building which had been rendered vacant by the removal of the lions to the new terrace was fitted up with cases with plate-glass fronts on a plan entirely novel in this country, and which for many years afforded an instructive exhibition of the forms, colours, and movements of many species of serpents, lizards, and crocodiles. This house was a vast improvement upon anything of the kind ever seen before; but the contrast between it and the present handsome and spacious building so recently erected in the south-eastern corner of the grounds affords another illustration of the great progress we are making.

If time allowed I might also refer to the Elephant-house, completed in 1870, to the Insect-house, opened in 1881, and to various others of less importance.

The erection of these houses has necessarily been a very costly undertaking; in fact, since what may be called the reconstruction of the permanent buildings of the Gardens, which commenced in the year 1860, more than £50,000 has been expended upon them. It is only in years of great prosperity, when the Society's income has considerably exceeded its necessarily large permanent expenditure, that works such as these can be undertaken.

Much as has been done in this direction, we must all admit that there is still more required. The buildings of to-day will, we may even hope, some day seem to our successors what the former ones appear to us. The old idea of keeping animals in small cramped cages and dens, inherited from the Tower and the travelling wild-beast shows, still lingers in many places. We have a responsibility to our captive animals, brought from their native wilds, to minister to our pleasure and instruction, beyond that of merely supplying them with food and shelter. The more their comfort can be studied, the roomier their place of captivity, the more they are surrounded by conditions reproducing those of their native haunts, the happier they will be, and the more enjoyment and instruction we shall obtain when looking at them. Many of our newest improvements are markedly in this direction. I may especially mention the new inclosure for wild sheep near the Lion-house in the South Garden, with its picturesque rock-work and fall of water, and the large aviary for herons and similar birds just completed on what used to be called the Water-Fowls' Lawn.

All such improvements can, however, only be carried out by the continued aid of the public, either by becoming permanently attached to the Society as Fellows or by visiting the Gardens. I trust that this brief record of the principal events of the Society's history will show that such support is not undeserved by those who have had the management of its affairs.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—In the Natural Sciences Tripos, Part I., the following women students were placed in the first class: E. E. Field, A. J. Flavell, and M. M. Smith, all of Newnham College.

In Part II. the following men were placed in the first class in alphabetical order, the subject for which they were so placed being named:—Adie, Trinity, and Couldridge, Emmanuel (Chemistry); Durham, Christ's, and Edgeworth, Caius (Physiology);

Lake, St. John's (Geology); Melsome, Queens' (Physiology); Rendle, St. John's (Botany); Turpin, St. John's (Chemistry). No women were placed in the first class.

Mr. Lake, of St. John's, whose name appears in the above list, has been elected to the first Harkness Scholarship for Geology and Palæontology.

Dr. William Hunter, M.D., F.R.S.Edin., has been elected the first John Lucas Walker Student in Pathology.

The degree of Doctor in Science has been conferred on Mr. James Ward, of Trinity College, and Prof. F. O. Bower, of Trinity College and Glasgow University.

In consideration of this year being the two hundredth anniversary of the publication of Newton's "Principia," the Chancellor's Medal is to be given for an English poem on Isaac Newton.

The botanical teachers in the University have made a pressing appeal for the erection of a class-room for practical microscopical botany.

The Examiners for the Mathematical Tripos, Part II., have issued the following class list:—

Class I. Division 1: C. W. C. Barlow, and Bryan, Peterhouse; Dixon, Trinity; Fletcher, St. John's; Platts, Trinity. Division 2: Coates, Queens'; F. W. Hill, St. John's. Division 3: Clark, Pembroke; H. G. Dawson, Christ's.

Class II. Division 1: Askwith, Trinity. Division 2: Johnston, Peterhouse; McAulay, Caius; Nicolls, Peterhouse. Division 3: Tate, St. John's.

Class III. Division 1: Dickinson, Trinity.

The appointment of a Demonstrator of Pathology has been approved.

The proposals regarding the teaching of geography and the appointment of a University Lecturer in Geography have been confirmed.

The modified proposals to build new plant-houses in the Botanic Garden have been approved. A small research laboratory is to be built in connexion with them.

At the annual election at St. John's College, on June 18, the following awards in Natural Science and Mathematics were made:—

Foundation Scholarships:—Science: Rendle, £50; d'Albuquerque, £60; Groom, £50—Mathematics: Norris, £40; Varley, £50; H. H. Harris, £50; Rudd, £40. Scholarships prolonged or increased in value:—Science: Rolleston, £80; Shore, £60; Seward, £40; Harris, W., £50; Lake, £80—Mathematics: Fletcher, £80; Hill, £60; Tate, £40; Orr, £80; Sampson, £80; Baker, £100; Flux, £100.

Exhibitions:—Science: Grabham, d'Albuquerque, Baily, Hankin, Shaw—Mathematics: Orr, Sampson, Carlisle, Millard, Cooke, Humphries, Shawcross, Palmer. Proper Sizarships:—Science: Kellett—Mathematics: Box, Brown, Lawrenson; Shawcross, Palmer. Hughes Prizes:—Science: Lake; Mathematics: Baker and Flux, equal. Wright Prizes:—Science: Turpin, d'Albuquerque; Mathematics: Orr, Cooke. Hockin Prize (for Physics, and in particular Electricity): Turpin. Herschel Prize (for Astronomy): Flux. Hutchinson Studentship (for Sanskrit): Strong.

Among the distinguished persons upon whom honorary degrees were conferred on June 20 was Prof. Asa Gray, Professor of Natural History and Keeper of the University Herbarium and Botanical Library, Harvard University, author of the "Elements of Botany" (1836), the "Botanical Text-Book" (1842, ed. 6, 1880), "Darwiniana" (1876), "Flora of North America" (1878), &c., &c. We append the text of the speech delivered by the Public Orator, Dr. Sandys, in presenting him for the degree:—

Iuvat tandem pervenire ad historiae naturalis professorem Harvardianum, botanicorum transmarinorum facile principem. Annorum quinquaginta intra spatium de scientia sua pulcherrima quot libros, eruditione quam ampla, genere scribendi quam admirabili composuit. Quotiens oceanum transiit ut Europae herbaria diligentius perscrutaretur, virosque in sua provincia primarios melius cognosceret. In aliorum laboribus examinandis, recensendis, nonnunquam leviter corrigendis, iudicem quam perspicacem, quam candidum, quam urbanum sese praebuit. Quanta alacritate olim inter populares suos occidentales Darwini nostri solem orientem primus omnium salutavit, arbitratus idem doctrinam illam de formarum variarum origine causam aliquam primam postulare, et fidei de numine quodam, quod omnia creaverit gubernetque, esse consentaneum. Viro tanto utinam contingat ut opus illud ingens quod Americae Borealis Florae accuratius describendae olim dedicavit, ad exitum felicem aliquando perducat. Illum interim, qui scientiam tam pulchram suis laboribus, sua vita, tam diu illustravit, usque canam ad senectutem, ut poeta noster ait, 'vitae innocentis candidum florem gerens,'—illum, inquam, his saltem laudis flosculis, hac saltem honoris corolla, libenter coronamus.

Plurimos in annos Academiae coronam illustriorem reddat Florae sacerdos venerabilis, Asa Gray.

SCIENTIFIC SERIALS.

The *Journal of Botany* for May contains the following articles:—Angolan Scitamineæ, by Mr. H. N. Ridley.—Forms and allies of *Ranunculus Flammula*, by Mr. Chas. Bailey.—Notes on British Characeæ for 1886, by Messrs. H. and J. Groves.—The progress of botany in Japan, by Mr. F. V. Dickins.—Conclusion of the Rev. Mr. Purchas's list of plants for South Derbyshire.

In the number for June Mr. E. M. Holmes describes and figures two species of seaweed new to Britain, *Ectocarpus simplex* and *E. insignis*.—There are also papers on Queensland ferns, by Baron von Müller and Mr. J. G. Baker; on the genus *Potamogeton*, by Mr. A. Fryer; on plants of Northern Scotland, by Mr. F. J. Hanbury and Rev. E. S. Marshall; on Chinese ferns, by Mr. J. G. Baker; and on Australian species of *Potamogeton*, by Mr. A. Bennett.

SOCIETIES AND ACADEMIES.

London.

Royal Society, June 16.—"Abstract of Investigations upon Rabies." By G. F. Dowdeswell.

The first experiments made by inoculations with the saliva of rabid street dogs, during the outbreak of the disease in 1885, all failed to produce infection, thus confirming the reputed uncertainty of the result of the bite of a rabid animal.

Subsequently, adopting the methods recently described by M. Pasteur, it was found:—

(1) That the virus of rabies in the lower animals and of hydrophobia in man resides in the cerebro-spinal substance and in the peripheral nerves, and is not confined to the salivary secretion, as previously believed, nor is even as constantly present or as actively virulent in it as it is in the nervous tissues.

(2) That inoculation of a portion of the nervous tissue from a rabid animal upon the brain of another by trephining produces infective rabies or lyssa, much more certainly, and with a far shorter incubation period, than by subcutaneous inoculation of the same substance; but that the disease is identically the same in both cases.

(3) That the virulence of "street rabies" is usually increased and ultimately becomes remarkably constant by passing through a series of rabbits, in which animals the symptoms are somewhat different from those in others, and which are generally regarded as typical, being essentially paralytic, but that paresis to some extent is always present in this disease in dogs and others of the lower animals, and that there is no constant distinction between the so-termed "dumb" and "furious" rabies in the latter animal, the difference consisting in the preponderance of the paralytic or other symptoms.

(4) That the tissues of an infected animal do not themselves usually become infective till towards the close of the incubation period.

(5) That of a large number of drugs that were tried, both germicides and those which act specifically upon the cerebro-spinal system, including those most esteemed for the treatment of rabies and hydrophobia, none have any material effect in modifying the result of infection in the rabbit.

(6) Lastly, that with respect to the methods of protection against infection by a series of inoculations with modified virus, as advocated and practised by M. Pasteur, these are unsuccessful with the rabbit, and that his recent "rapid" or "intensive" method of inoculation is liable itself to produce infection; and that with the dog the natural refractoriness of this animal to infection with rabies by any method of inoculation, is so great, that it is exceedingly difficult to determine the effect of any remedial or prophylactic measures upon it; and that with man the statistics of the treatment must determine its effects.

Physical Society, June 11.—Mr. Shelford Bidwell, F.R.S., Vice-President, in the chair.—A number of Puluj and other vacuum-tubes were exhibited by Dr. Warren De la Rue. The Puluj tubes consisted of a phosphorescent lamp, and radiometers with phosphorescent vanes and mica disks painted with phosphorescent substances. The other tubes contained different phosphorescent minerals, such as magnesium carbonate, calcium silicate, and Iceland spar. When illumined by a large induction-coil, beautiful colour-effects were produced.—The following papers were then read:—Note on beams fixed at the ends, by Profs. Ayrton and Perry. This paper contains a simple method of solving problems relating to horizontal beams with vertical loads, and fixed at both ends. The curve of bending-moment for the given distribution of load is first plotted, supposing the beam "*supported*" at the ends, and the constant c, by which the ordinates of this curve exceed those of the true curve, is determined from the condition that the angle between the end sections must be nought. If M is the bending-moment at a section, I the amount of inertia of the section about its neutral line, and E Young's modulus of elasticity for the material, then $\frac{M}{EI}$ is the curvature of the beam at that section. If O O′ is a short length of the beam, the angle between the originally parallel sections at O and O′ is $\frac{M}{EI} \cdot OO'$. Hence, if the beam be divided into a great number of parts, and the values of M and I determined at the middle of each, then

$$\Sigma \frac{M}{I} \cdot OO' = 0 \quad . \quad . \quad . \quad . \quad . \quad (1)$$

since E is supposed constant. But $M = m - c$, where m is the bending-moment at the same section, supposing the ends "supported";

$$\therefore \Sigma \frac{m - c}{I} = 0,$$

or,

$$\Sigma \frac{m}{I} = \Sigma \frac{c}{I},$$

$$\therefore c = \frac{\Sigma \frac{n}{I}}{\Sigma \frac{1}{I}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The following rule results: Knowing m and I at every point, divide the beam into any number n of equal parts, find $\frac{m}{I}$ at the middle of each part, and take their sum; this gives the numerator of (2). Find $\frac{1}{I}$ at the middle of each part, their sum gives the denominator of (2). From this c is determined. Diminish all the ordinates of the m diagram by c, and we have the diagram of bending-moment for a beam fixed at both ends, with any assumed distribution of load and variation of cross-section. Particular cases are worked out in full. Numerous drawings made by students of Finsbury Technical College were exhibited, showing applications of the method to different distributions of loading.—Note on Messrs. Vaschy and Touanne's method of comparing mutual induction with capacity, by Prof. G. C. Foster. In November last the author described a method of comparing the mutual induction of two coils with the capacity of a condenser. Since then he has found that a very similar method was used by Messrs. Vaschy and Touanne in July 1886, and published in the *Electrician* the following month. The formulæ are identical, and the difference consists in interchanging the galvanometer and the variable resistance ρ. Messrs. Vaschy and Touanne's arrangement has the advantage that the resistance of the secondary coil need not be known. Prof. Foster's method had been used by one of his students (Mr. Draper) about two years ago, but priority in publication belongs to Messrs. Vaschy and Touanne.—Prof. Perry asked the meeting for suggestions to explain why a strip of steel twisted about its longitudinal axis at a red heat, and allowed to cool, tends to untwist when under tension, and for a formula to calculate the amount.—A note on magnetic resistance by Profs. Ayrton and Perry was postponed.

Geological Society, June 8.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—A revision of the Echinoidea from the Australian Tertiaries, by Prof. P. Martin Duncan, F.R.S. After calling attention to a previous paper by himself published in the Society's Journal for 1877, and to additions to the fauna made by Prof. R. Tate and Prof. McCoy, the author proceeded to give notes on the characters, relations, and nomenclature of 29 species of Echinoidea. A few notes were added on the relations between this fauna and that now inhabiting the Australian seas, also on the connexions with the Tertiary Echinoidea of New Zealand, Sind, &c.—On the lower part of the Upper Cretaceous series in West Suffolk and Norfolk, by Mr. A. J. Jukes-Brown, and Mr. W. Hill. The district described in this paper is that of West Suffolk and Norfolk, and is one which has never been thoroughly examined; for no one has yet attempted to trace the beds and zonal divisions which are found at Cambridge through the tract of country which lies between Newmarket and Hunstanton. Until this was done the Hunstanton section could not be correlated definitely with that of the neighbourhood of Cambridge. It was the authors' endeavour to accomplish this, and the following is an outline of the results obtained by them. The paper was divided into six parts: (1) stratigraphical, (2) palæontological, (3) microscopical, (4) chemical analyses, (5) faults and alteration of strike, (6) summary and inferences. In the four first parts separate lines of argument were followed, and each led to the same set of conclusions. The chief interest of the paper probably centres in the gault, and its relations to the chalk marl and the red chalk. Quite recently the very existence of gault in Norfolk has been disputed, but the authors think the facts they adduce and the fossils they have found will decide that point. The gault at Stoke Ferry is about 60 feet thick, and in the outlier at Muzzle Farm *Ammonites interruptus* occurs plentifully in the form of clay-casts with the inner whorls phosphatized. At Roydon a boring was made which showed the gault to be about 20 feet thick, the lower part being a dark blue clay, above which were two bands of limestone inclosing a layer of red marl, and the upper 10 feet were soft gray marl; the limestones contained *Amm. rostratus*, *Amm. lautus*, *Inoceramus sulcatus*, and *Inoc. concentricus* (?), while the marls above contained *Belemnites minimus* in abundance. At Dersingham another boring was made which proved the gray marl (2 feet) to overlie hard yellow marl, passing down into red marl which rests on Carstone. The gray marl thins out northward, and as the red marl occupies the position of the red chalk, the authors believe them to be on the same horizon, an inference confirmed by the presence of gault *Ammonites* in the red chalk. Another point of importance is the increasingly calcareous nature of the gault as it is followed northward through Norfolk. This was regarded as evidence of passing away from the land supplying inorganic matter, and approaching what was then a deeper part of the sea; this inference is borne out by the microscopical evidence. As regards the chalk marl, it also becomes more calcareous: at Stoke it is still over 70 feet thick, and its base is a glauconitic marl which can be traced to Shouldham and Marham, but beyond this the base is a hard chalk or limestone, which is conspicuous near Grimston and Roydon, and passes, as the authors believe, into the so-called "sponge bed" at Hunstanton. The Totternhoe stone is traced through Norfolk, but is thin at Hunstanton (2 feet); its existence, however, enables the limits of the chalk marl to be defined, with the result that some 13 feet of the hard chalk at Hunstanton must be referred to that subdivision. The gray chalk also thins northward, and from 90 feet near Cambridge is reduced to about 30 at Hunstanton. The Belemnite-marls are traceable in Norfolk, but either thin out or are replaced by hard white chalk near Heacham. The Melbourn rock is continuous, and maintains similar characters throughout. The total diminution in the thickness of lower chalk is from 170 feet at Newmarket to 55 feet at Hunstanton, viz. 115 feet. An endeavour was made to estimate the amount and extent of gault removed by erosion from Arlesey and Stoke Ferry.—On some occurrences of Piedmontite-schist in Japan, by Mr. B. Kotô. Communicated by Mr. Frank Rutley.

Mathematical Society, June 9.—Sir James Cockle, F.R.S., President, in the chair.—The President announced that the Council had awarded the second De Morgan Medal to Prof. Sylvester, F.R.S.—The following communications were made:—Note on the linear covariants of a binary quintic, by A. Buchheim.—The motion of a sphere in a viscous liquid, by A. B. Basset (the method of solution was by definite integrals analogous to Fourier's solution of equations determining the propagation of heat).—On the reversion of series in connexion with reciprocants, by Capt. Macmahon, R.A.—Explanation of illustrations accompanying a preliminary note on diameters of cubics, by J. J. Walker, F.R.S.

PARIS.

Academy of Sciences, June 13.—M. Janssen in the chair.—On the life and labours of M. Laguerre, Member of the Section for Geometry, by M. Poincaré. A brief sketch is given of the important discoveries made, especially in pure geometry, by this distinguished mathematician, who was born at Bar-le-Duc on April 9, 1834, and died there on August 14, 1886.—General method for the determination of the constant of aberration, by M. M. Lœwy. By means of the table published in the *Comptes rendus* for May 23, the author has determined the two azimuths relative to the horizontal direction of the terrestrial movement. The solution of this problem affords a good illustration of the easy application of the new method, as well as the high degree of accuracy of which it is capable.—Note on the earthy phosphates, by M. Berthelot. Some practical remarks are offered in connexion with M. Joly's recent communication on the earthy phosphates. While confirming the numerical data of previous thermo-chemical studies, they extend and in some respects modify their application.—Note on the residuums resulting from the action of the acids on the alloys of the metals in association with platina, by M. H. Debray. In a previous communication it was shown that the common metals, such as tin, zinc, lead, alloyed with a small quantity of the metals of platina, when heated with an acid capable of dissolving the common metal yield either the metal of platina in the crystalline state, or perfectly distinct alloys, or, lastly, residuums containing a considerable portion of water and oxygen. Here it is shown that these residuums even contain nitrogen when the acid employed is nitric acid.—Figures in relief representing the successive attitudes of a pigeon on the wing; disposition of these figures on a zootrope, by M. Marey. By the method already described and applied to other birds, the author here represents the flight of a pigeon in eleven successive attitudes taken at equidistant phases in a single revolution of the wing. The zootrope on which these phases are reproduced is an instrument derived from Plateau's phenakistiscope, which reflects the continuous flight of a bird. The large number of the images and the slow rotation of the instrument reproduce the apparent movements so gradually that the eye is easily able to follow them in all their shifting phases. The bronze figures are painted on a white ground, the illusion being completed by appropriate tints imparted to the bill, feet, and eyes.—"The Pygmies of the Ancients in the light of Modern Science," by M. A. de Quatrefages. On presenting to the Academy the work bearing the above title, the author remarks that, although now found only in scattered groups everywhere oppressed or encroached upon by larger and stronger races, the dwarf Negrito peoples existed in compact bodies forming the bulk of the population in many parts of Africa, Southern Asia, and the Eastern Archipelago. The Akkas, discovered by Schweinfurth south of the Monbuttu country, formerly reached as far north as the parallel of Khartoum, and were known by this name to the ancient Egyptians, Mariette having found it inscribed under a pygmy sculptured on a monument dating from the old empire. The Negritoes of Malaysia and Melanesia, characterized by their low stature and a relative degree of trachycephaly, are quite distinct from the Papuans of the same region, and this distinction is now generally recognized by anthropologists. The Asiatic pygmies described by the ancients are represented by these eastern Negritoes, just as the African pygmies of Herodotus and Pliny were the ancestors of the Negrilloes still surviving in many parts of Africa. In stature the modern pygmies range from 1·507 (various tribes in the Malay Peninsula) down to 1·300 metre (the Batwas recently discovered by Dr. Wolff in the Congo Basin).—Observations of the Borrelly planet made at the Observatory of Algiers, by M. Trépied.—Observations of the new planet, No. 267, discovered at Nice on May 27, by M. Charlois.—On a new form of electrometer, by M. J. Carpentier. The apparatus here described has been prepared especially with a view to industrial appliances. It is distinguished by its exceptional qualities of aperiodicity, by which its readings are rendered perfectly sure and rapid.—Researches on the trimetallic phosphates, by M. A. Joly. Here are studied the sodico-strontianic and sodico-barytic phosphates and arseniates, which are specially interesting owing to the readiness with which they are formed in the crystalline state with a considerable liberation of heat, and under conditions analogous to those yielding the ammoniaco-magnesian phosphate.—On the metallic vanadates, by M. A. Ditte. Having already prepared a number of vanadates by dry process, the author here shows that many metallic vanada such as those of magnesia, lime, nickel, cobalt, zinc, cop lead, and silver, may also be produced by the wet process. crystallized vanadates thus obtained present, like the oth compositions analogous to those of the alkaline vanadates.—the hydrochlorates of chlorides, by M. Engel. This paper d more especially with the hydrochlorate of perchloride of iro On the composition of different butters, by M. E. Ducl The experiments made by the author with butters from var parts of France show that, contrary to the generally accep opinion, the quality of this article does not depend so much the method of preparation as on the breed of cattle and t food, the character of the pastures—that is to say, the geolog constitution of the soil—the influence of the seasons, the ag the milk, &c.

BOOKS, PAMPHLETS, and SERIALS RECEIVE

Journal of the Chemical Society, June (Gurney and Jackson).—Proc ings of the Society for Psychical Research, May (Trübner).—Journal of Royal Microscopical Society, June (Williams and Norgate).—Bulletin d Société Impériale des Naturalistes de Moscou, No. 2 (Moscou).—Beiblä zu den Annalen der Physik und Chemie, 1887, No. 5 (Barth, Leipzig).—cords of the Geological Survey of India, vol. xx. Part 2.—The True Sou of the Mississippi: P. Giles.—A Century of Electricity: T. C. Mender (Macmillan).—Atlas de la Description Physique de la République Argen Deux. Section, Mammifères: Dr. H. Burmeister and E. Daireaux (Bu Aires).—Metal Plate Work: C. T. Millis (Spon).—Animal Biology: C Morgan (Rivingtons).—My Hundred Swiss Flowers: M. A. Pratten (Al —Dinocerata, an Extinct Order of Gigantic Mammals: Prof. O. C. M (Washington).—Introductory Text-book of Physical Geography, Edition: D. Page (Blackwood).—On Light (NATURE Series): Prof. G Stokes (Macmillan).—Manchester Microscopical Society, Transactions Annual Report, 1886.—Geodätische Arbeiten, v. Heft; Vandstandsobse tioner, iv. Heft (Kristiania).—The Nature of Fever: Dr. D. MacAl (Macmillan).—Proceedings of the American Academy of Arts and Scien New Series, vol. xiv., Part 1 (Boston).—Natural History Transaction Northumberland, Durham, and Newcastle-upon-Tyne, vol. ix., Pa (Williams and Norgate).—Bulletin de la Société Impériale des Naturali de Moscou, 1886, No. 3 (Moscou).

CONTENTS. PA

THURSDAY, JUNE 30, 1887.

FORESTRY.

School of Forestry in Germany, with Addenda relating to a desiderated British National School of Forestry. By John Croumbie Brown, LL.D. (Edinburgh: Oliver and Boyd; London: Simpkin, Marshall, and Co., 1887.)

"SILVER and gold have I not; but what I have I am prepared to give." This is what the author tells us towards the end of the present volume, and there can be no doubt that he has fully acted up to his promise. He has now presented the public with what appears to be the fifteenth volume on subjects of forestry, and he offers to publish some thirty additional volumes if the necessary inducement is held out. Surely Dr. Brown must be extremely philanthropic, or else the publishing of books is considerably cheaper than we have so far believed it to be. These works, published and unpublished, deal with forest subjects in almost every known country of the earth, and we wonder how Dr. Brown has managed to collect all the information. The above-mentioned offer seems to have been made in succession to a variety of bodies, but none of them have availed themselves of it, and the world at large must, for the present, be satisfied with the information contained in the fifteen volumes which have so far passed through the press. That, however, extends over a considerable range, including information regarding forests and schools of forestry in Germany, France, Spain, Norway, Russia, and the Cape; on modern forest economy; the effects of forests on humidity of climate; hydrology of South Africa, &c., &c. Now, it appears to us either that Dr. Brown's works are deficient in interest, or that his countrymen are very ungrateful in not availing themselves of his handsome offer. If we follow the dictates of common-sense, we must, it seems, decide in favour of the former alternative.

We hear occasionally of a Parliamentary Committee which considers "whether, by the establishment of a forest school, or otherwise, our woodlands could be rendered more remunerative"; or a feeble effort is made to start a National British Forest School in Edinburgh; or a languishing controversy turns up, whether the junior officers of the Indian Forest Department should be educated in France, Germany, or at home. But on the whole these matters do not excite much curiosity or interest. Parliamentary Committees on the subject die away without making any proposals beyond suggesting the re-appointment of a similar Committee in the next Parliament, which event may come to pass if members have no bigger game to hunt; Edinburgh is still without its forest school, and a forestry branch has actually been added to the Royal Indian Engineering College at Cooper's Hill, for the education of Indian forest officers, without many people being aware of the fact. The explanation of all this indifference is that even the perseverance of Dr. Brown has not yet succeeded in convincing Englishmen of the importance of afforestation. The mere fact that it is of importance in various Continental countries and in several British dependencies is not sufficient to show that the same holds good in these islands, and it will be as well to say something more on this subject.

Forests are, in the economy of Nature and of man, of direct and indirect value: the former through their products, and the latter through their influence upon climate, the regulation of the water-supply, the healthiness of a country, and allied phenomena. These islands are rich in iron ore, coal, and peat, wherewith to produce more iron than is required by the country, and to render the question of firewood of very subordinate importance. What is more, they are so situated that the importation of wood and other forest-produce is comparatively easy and cheap, owing to their sea-bound position, and a multitude of railways and other means of communication scattered over the country. At any rate, we have received, so far, as much timber as we require, and at a lower rate than it has been possible to produce it at home. Whether this state of things will last for ever is a different question; but it rests with us to initiate measures in our dependencies (such as Canada) which will secure us against a timber famine as long as the British Navy rules the sea. After all, the whole question turns on this point, and the decline of the British Navy would raise other issues of such immense importance, that the question of the future timber-supply of this country may well be added without producing a nightmare in even the most imaginative mind.

Again, in respect of the indirect effects of forests, Englishmen may rest assured that the absence of woodlands will not ruin their country. The climate and rainfall of these islands are principally governed by their geographical position. Strong moist air-currents come to us direct from the sea, and, compared with their effects, those of forests, even if 20 per cent. of the total area of the United Kingdom were covered with them, would be found comparatively small. Nor need we cry for forests on account of the general regulation of moisture; because, thanks to an ample rainfall and a comparatively moist state of the atmosphere, our waste lands are generally covered with heath, mosses, and other growth, which act as powerful retainers of moisture. To add a crop of trees to these would make comparatively little difference, especially as afforestation would, in many cases, have to be accompanied by the draining of the soil.

In some respects, however, an increase of our woodlands might be highly beneficial. They would afford protection not only to cattle and birds (the latter being the great destroyers of noxious insects), but also to agricultural lands which are at present exposed to strong sea breezes. A judicious distribution of woodlands along the coasts (especially the western) of these islands would no doubt be followed by beneficial results in this respect. Again, our waste lands (occupying upwards of 40,000 square miles, equal to 34 per cent of the total area), might be made more remunerative than they are at present, and their afforestation would provide a very considerable amount of work, not only by the creation and subsequent management and working of the forests, but also by the springing up of a variety of industries dependent on the existence and sustained yield of woodlands. We take this opportunity to recommend the subject to the careful attention of those who are about to legislate once more on the Irish land question. Experience has shown that the climate of a considerable portion of Ireland (especially in the coast districts) is not sufficiently favourable to produce crops which will permanently support the cultivator and yield large rents to the owners of the soil. In such cases the afforestation of surplus lands (that is to say, lands not required for agricultural operations) might prove a useful auxiliary in the solution of the Irish land question, by providing additional work which would enable the small cultivator to earn a day's wages whenever his presence was not required on his holding. Instead of sitting idle during a good portion of the winter, he could appreciably augment his income (and capacity to pay rent), without being obliged to leave his home in search of distant work

However, we must return to Dr. Brown. Our author has in the book under notice placed before the few who may be interested in the question, a fair account of how forest schools are arranged in Germany, the country where forest science has attained its highest development. The arrangement of studies at the several schools is given in considerable detail, and the book shows the high standard of education of German forest officers. Some of the schools are independent institutions situated in or near extensive forests, while others form part of Universities or technical Colleges. In the former case the education takes generally a more practical turn, while under the latter arrangement a higher standard of general education is likely to be secured. Dr. Brown is in favour of attaching the desired British forest school to a University or other similar educational establishment. In our opinion the decision on this point should depend on the class of men whom it is desired to educate. The ordinary foresters required by British landowners for the management of their woodlands are men who could not be enrolled as members of a University College; and their education must be of a more simple description, with a strong practical tendency. But men who are to join the general administration of India should attain a high standard of education, and a forest school for their benefit might well be attached to a University or to a high-class College. Unless such men are fit to take their proper place amongst the rest of the governing staff of the country, they will not be able to do justice to the work which will be intrusted to them on their arrival in India.

Both wants cannot be met by one set of men. The employment of men who have merely had a practical training might be disastrous to the Indian forests. On the other hand, British landowners would decline to receive men who, in consequence of a College education, would be above the ordinary work of a British forester, not to mention the fact that such men would expect higher rates of pay than the owners of woodlands would be willing to give them. In short, the course of studies to be followed by each of these two classes of men must be arranged on different lines. In either case, however, a tract of well-managed woodlands should be situated close to the seat of the school. To do without such a training-ground would be equivalent to training medical men without a hospital ready at hand. On this point we are decidedly at issue with Dr. Brown, who, in declaring such a school-forest unnecessary, has, in our opinion, only proved that he has failed to grasp the essential requirements of a forester's traini[ng.] At the same time, the reader of Dr. Brown's books can[not] help wondering at the marvellous industry employed [by] the venerable author in the compilation of his vari[ous] works on Continental forest schools. Such energy [and] devotion are deserving of a better reward than they [are] likely to meet with, owing to the apathy on forest qu[es]tions existing in this country. Sw

OBSERVATIONS AT GODTHAAB.

Observations Internationales Polaires, 1882–83. "Ex[pé]dition Danoise: Observations faites a Godthaab s[ous] la direction de Adam Paulsen." (Copenhague, 188[6].)

THIS part of the publications of the Meteorolog[ical] Institute of Denmark deals with the meteorologi[cal,] tidal, and other observations made in 1882–83 by [the] Danish Arctic Expedition at Godthaab (64° 11′ N. l[at.,] 51° 44′ W. long.). The pages devoted to meteorol[ogy] present us with detailed tabular views of the ho[urly] observations of atmospheric pressure, temperature, [and] humidity, and the direction and velocity of the w[ind.] These are prefaced by an interesting and full discuss[ion] of the atmospheric pressure (illustrated with 186 press[ure] and wind charts of Greenland), which includes a valua[ble] comparison of the observations of that year with thos[e of] all the stations since 1866.

In summer the mean lowest temperature, 38°·2, occ[urs] at 2 a.m. and the highest, 43°·1, at 2 p.m., the daily ra[nge] being thus 4°·9. On the other hand, the mean d[aily] range of temperature is extremely small in winter, ow[ing] to the proximity of Godthaab to the Arctic circle. Thus [the] highest and lowest hourly temperatures were respectiv[ely] in December, 19°·5 at 10 a.m. and 18°·0 at 10 p.m.; [in] January, 15°·1 at 5 p.m. and 14°·1 at 1 a.m.: and [in] February 4°·4 at 6 p.m. and 3°·7 at 3 p.m. Thus [in] February the mean warmest and coldest hour of the [day] show a difference of only 0°·7, and are only three ho[urs] apart from each other. Quite different is the amount [of] the daily range of temperature deduced from the maxi[ma] and minima of the month; in February the mean of the highest was 9°·0 and of the lowest 0°·3, the differe[nce] being 8°·7. In these months the changes of temperat[ure] are but little influenced by the sun, being almost al[to]gether occasioned by the passage of the cyclones a[nd] anticyclones. It is this practical elimination of the in[flu]ence of the sun which gives a peculiar value to [the] temperature and hygrometric observations of such s[ta]tions when any serious attempt is made to assign to th[ese] important elements the parts they play in the history [of] storms. The mean annual temperature calculated fr[om] the twenty-four hourly observations is only about o[ne-]tenth of a degree lower than that from the daily maxi[ma] and minima. In all the months the agreement is clo[se,] the greatest difference being 0°·5 in February a[nd] August; and in seven of the months the difference [does] not exceed 0°·1.

The results giving the variations in the hourly veloc[ity] of the wind are interesting as showing that such diur[nal] variation as may exist will require a number of ye[ars'] observations to show it clearly, this periodicity be[ing] masked in an unusual degree by the high winds wh[ich] accompany the low-pressure systems of Greenland.

With eighteen years' observations (1866–83) at Ivigtut, Godthaab, and Jacobshavn, and nine years' (1875–83) at Upernivik, we can now present, with an approximate correctness not hitherto attainable, the distribution of pressure over Greenland during the months of the year. The following mean pressures, at 32° and sea-level, give the highest and lowest, with the months of their occurrence:—

	Highest. Inches.	Lowest. Inches.	Year. Inches.
Ivigtut	29·827 in May.	29·398 in January.	29·666
Godthaab ...	·847 ,,	·445 ,,	·684
Jacobshavn..	·898 ,,	·565 ,,	·749
Upernivik ...	·981 in April.	·603 ,,	·753

Thus in Greenland pressure increases with latitude. The difference between the extreme south and north is in January 0·205 inch, and in spring 0·154 inch; but the difference in summer is small, being in July only 0·008 inch. The above mean of January at Ivigtut 29·398 inches, and for the same month at Stykkisholm in the north-west of Iceland 29·396 inches, are, so far as known, the lowest mean monthly pressures anywhere yet observed in the northern hemisphere; and it is interesting to note that it is in the region immediately to the south and south-west of these places that a very large number of our European storms have their origin.

Attention is forcibly drawn by the 186 charts of pressure and wind to the remarkable fact that the depression areas of Greenland appear to travel from north to south. An extension of the area charted would doubtless show that while in many cases these areas travel northwards yet in a considerable number of cases this direction is more apparent than real. It is, however, abundantly evident that Greenland exerts an important influence on our Atlantic storms that remains still to be investigated.

The most elaborate part of the paper is the discussion of the diurnal curves of pressure from the hourly observations. The curve for the year exhibits the two usual maxima at 8 a.m. and 8 p.m., and the two minima at 2 a.m. and 1 p.m., the morning minimum and the evening maximum being respectively the greater, and these features of the curves are, with one exception, reproduced in the curves for the months. The results will be made to tell their story more clearly if we eliminate the more prominent irregularities attaching to means of one year only, by bloxaming the hourly means, by taking for the hourly means of each month means calculated from that month, the month immediately preceding, and that immediately following it. In these new mean hourly values the morning greatly exceeds the evening maximum in February, March, and April, whereas in every other month the reverse holds good, and that in a very pronounced degree. On the other hand, the morning greatly exceeds the afternoon minimum in each month of the year. From the relations the results show to those for places in similar situations in lower latitudes, we may conclude that unusual care has been taken in securing for the barometer a position where it was subject to only a very small daily change of temperature. It is absolutely necessary that this condition should be attended to, if observations are to be of any use at all in the discussion of the important question of the horary variations of pressure in high latitudes. Since the variations dealt with seldom exceed 0·010 inch, and are generally much less, it is evident that the inquiry is for these regions a refined one; hence it is essential that the attached thermometer should represent the temperature of the whole instrument to within 1° F. It is the neglect of this point that vitiates several series of horary barometric observations in the Arctic regions.

Over the open sea in high latitudes during the summer months, where the sun either does not set, or only for a brief interval, the diurnal curve of pressure differs essentially from the above. The observations made by the *Challenger* Expedition in the Antarctic Ocean, and those made by the Norwegian Expedition in the north of the Atlantic, show only one maximum and one minimum in the day, the maximum occurring during the day and the minimum during the night. This peculiar curve is restricted to the open sea of high latitudes. Director Paulsen is inclined to the opinion that the diurnal variation of pressure at Godthaab is caused not so much by local variations of temperature and humidity as by transmissions from lower latitudes of their diurnal variations of pressure. In this opinion we to some extent concur, it being probable that some of the more prominent features of these daily curves of pressure are the results of vast quasi-tidal movements communicated through the higher regions of the atmosphere, in which the space traversed by the individual aërial molecules is not necessarily great.

OUR BOOK SHELF.

Essays and Addresses. By the Rev. James M. Wilson, M.A. (London: Macmillan and Co., 1887.)

IN these "Essays and Addresses" Mr. Wilson deals chiefly with problems connected with religion and morality, and his main object seems to be to show that theological and ethical principles, properly interpreted, are supported, instead of being contradicted, by scientific ideas. The book is evidently the result of much independent reflection. Mr. Wilson tries to grapple with no intellectual difficulty which he has not thoroughly examined, and in all his statements of scientific doctrine he is scrupulously exact. He refers to science in so many aspects that much of what he has to say may be studied with interest even by readers who do not feel that his arguments with regard to such subjects as "Miracles" and "Christian Evidences" are perfectly conclusive.

Introductory Text-book of Physical Geography. By the late David Page, LL.D., F.G.S. Twelfth Edition. (Edinburgh and London: W. Blackwood and Sons, 1887.)

THIS book was originally published about twenty-five years ago, and has done good service in many schools and colleges. After the author's death it was brought up to date by Dr. Charles Lapworth, who, besides making a number of minor corrections and additions, contributed a summary of those results of the *Challenger* Expedition which had reference to the depths, deposits, and temperature of the ocean; an account of British storms; a description of the biological regions of the earth; and a short sketch of Prof. Huxley's arrangement of the human family. In the present edition Dr. Lapworth has again sought to bring the work abreast of scientific knowledge, introducing new matter relating to geology and petrography, meteorology and climatology, and the distribution of animals and plants. On the latter subject he has obtained from Prof. D'Arcy Thompson an excellent summary of recent biological research and theory. The value of the book has also been increased by the insertion of several new maps illustrative of the astronomical and meteorological sections.

Longman's New Geographical Reader. Standard VII. (London: Longmans, Green, and Co., 1887.)

THIS "Reader" contains lessons on the ocean, currents, tides, the planetary system, and phases of the moon. The subjects are of more scientific interest than those treated in most books on geography, and are arranged in a progressive and readable form.

The book is divided into sixty lessons, each being followed by a list of some of the words contained in it, with their meanings.

In the chapters on the ocean the subjects are well selected, and the various depths and currents are illustrated by maps. In the lesson on the tides the differential action of the sun and moon on the water of the earth should have been mentioned. The diagram illustrating neap tides has one bad point, the sun being shown as shining on a part of the moon which is turned away from it.

In the diagram on page 231, which represents the sun as seen in full daylight from the surface of the moon, the sun is shown with its corona. The fact of the sun being seen from the moon, which has no atmosphere, would not make the corona visible, but would only tend to intensify the light of the sun and the corona proportionally. It is a pity that this illustration should have been put in without any explanation whatever.

The chapters on the inhabitants of the sea and methods of catching them are very interesting; also the voyages to the Arctic and Antarctic regions. An appendix is added which contains a summary of the whole 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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The New Degrees at Cambridge.

THE letter of "Outis" in yesterday's number of NATURE (p. 175) is likely, I fear, to convey a false impression as to the new degrees of Doctor of Science and Doctor of Letters, which, by the way, were instituted, for good or for evil, by the Commissioners, and not by any "dominant body in the University." It is true that Doctors in the new faculty take precedence after Doctors in Medicine just as Doctors in Medicine take precedence after Doctors in Law, and Doctors in Law after Doctors in Divinity, but this distinction is only of importance when a procession has to be marshalled; to all intents and purposes the academic rank of all Doctors is the same.

If it be true, as I believe it is, that the standard for admission to the regular degree of Doctor in Science is only "rather less than that required for admission to the Royal Society," and that the standard for Doctor in Letters is much the same, it follows that the standard for such degrees is much higher than that for any other Doctorate in the University, while that for Doctor in Law is notoriously the lowest of all.

Since the new degrees were instituted the Council has usually offered the new degrees to those persons selected as recipients of honorary degrees whose claims were essentially scientific or literary, while it has continued to give the honorary LL.D. to persons whose distinction was of a less academic kind. This may have been wise or unwise, but the Council had certainly no idea that in what they were doing they were offering to men of science an honour of a lower grade than that to which they had been accustomed. It is true that, fearing perhaps that the less familiar title might at first be not so well understood outside the University, they began by offering to the recipients the choice of the degree of LL.D. or of Litt.D. or Sc.D., as the case might be; but I believe that in all cases those who had the choice preferred the literary or scientific degree.

No doubt these degrees, like that of LL.D., have been and will continue to be given to men of very different degrees of eminence. It is not every year that the University has the opportunity of enrolling among its honorary graduates a man like Asa Gray; but I think that, even if he is excluded, the roll of our honorary Doctors in Science and Letters need not fear comparison with that of the honorary Doctors in Law who have received their degrees within the same period.

Cambridge, June 24. C. T.

Weight, Mass, and Force.

THE position taken up by "P.G.T." and some others in the discussion on the proper use of the words "weight" and "mass" is similar to that assumed by an astronomer coming forward to tell us that we have been calling the stars by their wrong names.

The following extract from an American technical journal is submitted to the consideration of "P. G. T.," Mr. Hayward, and Mr. Alfred Lodge, in order that they should point out for our benefit where they consider the dynamical language is erroneous, and that they should translate it into the terminology necessary in their opinion to make it correct by using the mathematical terminology of poundals, dynes, moms, poundems, &c.

"DESCRIPTION OF THE STRONG LOCOMOTIVE."

American Journal of Railway Appliances, March 15, 1887.

"The weight of the engine in working order is 137,000 lbs., of which 90,000 are on the drivers, 27,000 on the front truck, 20,000 on the back truck. The weight of the tender loaded is 75,000 lbs. The boiler carries 160 lbs. of steam, which pressure is easily maintained when the engine is pulling the heaviest and fastest trains over the 96-feet grades across the mountains.

"The engine having 20-inch by 24-inch cylinders, and 62-inch drivers, the traction force is, according to the well-known formula, $20^2 \times 24 \div 62 = 154{\cdot}8$ lbs. for each lb. of mean effective pressure in the pistons. The resistance of modern rolling-stock as deduced from the most recent experiments both in this country and in Europe is from 12 lbs. per ton of train including engine and tender at speeds of 30 miles an hour to 15 lbs. at 50 miles an hour, above which point the resistance increases in a much greater ratio.

"Let us suppose then that the engine is hauling a train at 30 miles an hour on the level, cutting off at 10 inches of the stroke. From indicator cards taken from the engine under these conditions we find that a mean effective pressure of 100 lbs. is maintained in the cylinders. The traction force exerted will thus be $154{\cdot}8 \times 100 = 15480$ lbs.; and taking the resistance at 12 lbs. per ton, we find the maximum load the engine can pull is $15480 \div 12 = 1290$ tons, and subtracting from this the weight of the engine and tender there remains a weight for the train $1290 - 106 = 1184$ tons, or the equivalent of no less than 59 20-ton cars.

"Now suppose the engine is running up a grade of 96 feet to the mile (1 in 55) at the same speed and cutting off at the same point as before. The resistance to gravity on a 96-feet grade is $2240 \div 55 = 41$ lbs. per ton, and this added to the 12 lbs. resistance on the level gives $41 + 12 = 53$ lbs. per ton for the train going up the grade. Under these conditions the load hauled would be $15480 \div 53 = 292$ tons; or, subtracting the engine and tender, $292 - 106 = 186$ tons, the equivalent of $9\frac{1}{2}$ 20-ton cars.

"Turning now to the question of adhesion, we find that taking the coefficient of adhesion at one-fifth, we have a weight of 18000 lbs., one-fifth the weight on the drivers, as against the 15480 lbs. of traction. We need hardly say that the average coefficient of adhesion is usually higher, one-fourth being generally taken in calculations relating to the performance of locomotives. Under this condition the weight available for adhesion would be 22222 lbs., or one-fourth the weight on the drivers, and the mean effective pressure in the cylinders would have to amount to $22222 \div 154{\cdot}8 = 143{\cdot}6$ lbs. per square inch before the wheels would begin to slip or the use of sand become necessary. At the speed of 30 miles an hour and 100 M.E.P. (mean effective pressure) this engine would exert about 124[illegible] H.P.," &c.

This account is written by someone to whom the dynamical problem is a reality and no theoretical abstraction: he employs throughout the gravitation measure of force, and to an engineer there is no ambiguity in his use of the words pound and ton sometimes in the sense of mass or weight and sometimes in the sense of force.

Prof. A. B. W. Kennedy, in his "Mechanics of Machinery," pp. vii. and 222, has called attention to the same ambiguity of language, and points out that the word pound is used in two senses (three senses when pound-sterling is to be distinguished), which he proposes to distinguish as weight-pound and force-pound; not, observe, mass-pound and weight-pound as the mathematician would have us call them.

An article in the current number of *Wiedemann's Annalen*, No. 6, 1887, by A. Oberbeck, "Ueber die Bezeichnung der absoluten Maass-systeme," shows that a similar controversy on dynamical terminology is now going on in Germany.

The mathematical definition that "the weight of a body is the force with which the earth attracts the body" must disappear and be replaced by "the attraction of the earth on a pound, a ton, or a kilogramme, is called the force of a pound, a ton, or a kilogramme;" these are gravitational units of force, derived originally from statical considerations, but used in practice for dynamical problems also; but inasmuch as the magnitude of these units depends on the local value of *g*, they are unsuitable for astronomical or electrical purposes, and are now replaced in such cases by the absolute units of force, the poundals, dynes, &c.

The defect of modern dynamical teaching is the unreality of its applications; it is too much the "dynamics of a particle." Were the student accustomed to examples taken from the magnificent problems presented by the latest industrial developments, he would become accustomed to the use of gravitational and absolute units of force, and recognize their respective advantages. A. G. GREENHILL.

Woolwich, May 30.

Upper Cloud Movements in the Equatorial Regions of the Atlantic.

RECENT communications to your paper have given the motion of the upper clouds from the eastward in the equatorial regions of the Atlantic. My observations (the result of having passed through these regions sixteen times in sailing-ships) give the motion of the upper clouds from the westward; and the motion of the intermediate cloud layers, consisting of the high low-level stratiform clouds (cirro-cumulus and such like), from a point somewhat to the north of east on the north side of the equator. Intermediate clouds are rare in the equatorial regions south of the Line. The high low-level clouds are constantly being confounded with the true high clouds.

There is another source of error in noting upper cloud movements; little attention has been paid to a movement of propagation. So marked is this at times, that they are propagated over the sky quicker than they are moving, this movement being frequently at right angles to the direction of motion.

DAVID WILSON-BARKER.

The Shadow of Adam's Peak.

THE shadow of Adam's Peak, to which Mr. Abbay refers in his letter to NATURE, vol. xxxvi. p. 152, is certainly not the kind of shadow that I witnessed, but that which is only seen in the clearest weather and without the intervention of mist. This is mentioned in one of the last paragraphs of my paper.

Nevertheless, I cannot think that mirage has anything to do with that shadow. When the Observatory was first established on Pike's Peak, the observers used to see the shadow of the mountain rising against the sky on the far distant horizon. At first they thought this very curious, but soon found that the appearance was always there in very fine weather.

Further observation showed conclusively that the appearance was simply the ordinary earth shadow of sunrise projected so clearly against the sky, that an irregularity such as a sharp peak could be distinguished on the edge of the generally circular shadow.

I do not think that mirage has anything to do with this anti-crepuscular shadow, but no doubt there are abundant thermometric observations in America for anyone who wishes to investigate the subject further. RALPH ABERCROMBY.

21 Chapel Street, London, June 17

Temperature and Pressure.

I HAVE to thank Mr. S. A. Hill for replying to my letter, and it is most interesting to know that the same connexion between temperature and pressure exists in India as in Jamaica (NATURE, vol. xxxv. pp. 437, 606).

No doubt, as Mr. Hill observes, different localities will have different values of the coefficients λ and μ in the equation—

$$\delta T = \lambda \delta P + \mu (\delta P)^2;$$

indeed, we must expect very different values; but still, by putting $\delta P = 0$ in the equation for *minimum* temperatures, each locality should give the same limiting temperature, which we may regard as the temperature of space.

It is of course to such concordance that we must look for the determination of the temperature of space, so defined, rather than to extreme care in the taking of observations in any one particular part of the world.

With reference to Mr. Hill's remarks about extrapolation, it is hardly necessary for me to point out that astronomical refraction is caused by the whole terrestrial atmosphere, and that some law between temperature and pressure must be adopted before refraction-tables can be constructed; Mr. W. H. M. Christie, the Astronomer-Royal, has, in the Memoirs R.A.S., vol. xlv. p. 177, recently pointed out how errors may arise from this source.

Indeed, errors must arise from this source. In Jamaica the values of λ and μ are not the same for mean and minimum temperatures, or, roughly speaking, for day and night; neither is it to be expected that they will be the same anywhere else. But enough has been said to indicate the importance of the problem, and the steps which should be taken for its solution.

Jamaica, June 6. MAXWELL HALL.

British Association Sectional Procedure.

IN reference to Prof. Thompson's letter (June 16, p. 151), will you allow me to say that in 1884 I went from the meeting of the Association of American Microscopists at Rochester, N.Y., to that of the British Association at Montreal. At the former the proceedings commenced daily at 9 a.m., closing about noon, and another short session was held in the afternoon. The middle of the day was thus left at liberty for Committee work, sight-seeing, or rest, and the greater amount of work got through in the day as compared with the usual plan at our Association was very striking. ALFRED W. BENNETT.

6 Park Village East, Regent's Park, June 18.

Mirage.

THIS afternoon, shortly after 4.30 p.m., my attention was drawn to an extraordinary and wonderfully perfect "mirage." My house, situated almost on the extreme point of Hartlepool, near the Heugh Lighthouse, overlooks with a south aspect the Hartlepool or Tees Bay, Redcar, Saltburn, and in clear weather a beautiful high coast-line stretching from Saltburn to Staithes. When first seen, all the houses of Redcar, some seven miles distant, and lying almost at sea-level, were enormously elongated to at least six or seven times their ordinary height, and looked like high square towers with intensified colouring. I could not however determine (with the aid of an opera glass) whether the phenomenon was a simple elongation or whether the upper part of the "mirage" was an inverted image of the houses. I am inclined to think that the lower two-thirds was an elongation of the buildings, while the upper third was an inverted image.

During the height of the mirage a dark misty stratum of air, bounded by a distinct upper margin parallel with the horizon, and decreasing in density towards it, stretched from the western end of Redcar through an arc of almost 90° seawards. I estimated the height of this stratum at 35′ to 36′ of arc. After some 10 minutes the "mirage" gradually dwindled over Redcar, but remained distinctly visible for a short time longer over Saltburn, the coast-line, and out to sea. At Saltburn, about 11½ miles distant, some of the buildings were duplicated, a white house being visible as two spots widely separated. The normal coast-line south of Saltburn was obscured by the haze, but a beautifully clear "mirage" of it was visible, taking as its horizon the upper margin of the misty air stratum, the horizon being thus bodily raised through some 36′ of arc. Out at sea in an almost easterly direction a smoking steamer was faintly visible with an inverted image, masthead touching masthead, and a small schooner under sail, some 8 or 10 miles away, exhibited an inverted image of its topsails. In the direction of Saltburn a tug-boat (paddle steamer) steaming within perhaps 2 miles of the coast was quite normal in appearance. The smoke rising from it drifting eastward appeared normal for a short time, but suddenly expanded on rising to about ten times its thickness, and then tailed off again at its topmost eastward corner to the normal thickness. A large steamship lying about half-way across the bay showed no signs of being affected by the "mirage."

These phenomena disappeared in about 20 minutes, and were followed by a haze which obscured the distance.

The weather had been very hot and sultry all day, with about 70° in the shade, and a gentle south-east by east breeze, and a perfectly clear sky.

Estimating the height of the Redcar houses at 50 feet and the distance at 7 miles, they would occupy 5′ of arc; and taking the thickness of the air stratum producing the "mirage" at 35′ of arc, the elongation of the buildings would be seven times their ordinary height.

Though I have been informed that "mirages" were visible on the previous hot days, the phenomenon is on the whole of rare occurrence here, and has never been witnessed by myself before. CHAS. O. TRECHMANN.

Hartlepool, June 18.

P.S.—My point of view would be about 20 feet above sea-level.

A Suggestion for Anthropologists.

So far as the undersigned has seen, all reviewers of the "Précis d'Anthropologie," lately issued by Profs. Hovelacque and Hervé, of Paris, have noted with no little interest the attitude of the work towards the problem of the origin of man. Rejecting on the one hand the doctrine of the monogenesis of the human family in the way of a purely natural evolution out of lower forms of life, and on the other hand discrediting the polygenesis of men by special creation in different centres of distribution, these eminent anthropologists present, as the probable truth, a compromise hypothesis, which they call *transformisme polygénique*. According to this view, men were evolved from the lower animals, but in more than one original centre, and from more than one original pair. A French reviewer has well intimated the significance of this new teaching by observing that it marks a distinct schism in the ranks of the Darwinistic anthropologists, and inaugurates debates and investigations from which most important new light may be expected.

In this connexion it has occurred to me that if I were an anthropologist, and especially one of Darwinian principles, I should be exceedingly eager to institute investigations looking to the establishment or overthrow of a still different conception of the matter—one not yet studied with anything like the thoroughness which it deserves. I might call the hypothesis to which I allude the hypothesis of *transformisme bigénique*. Being neither its inventor nor a believer in it, I can the more freely call the attention of believers in transformism to its decided richness of promise. Indeed, if there is any middle ground of truth between the anthropology of Darwin and the anthropology of Agassiz, it can nowhere so hopefully be sought as precisely here. The hypothesis to which I refer is that according to which the human family consists of the descendants of two primitive human races—the one white and originating at the North Pole, the other dark and originating at the South Pole.

The only work in which I have ever found this view suggested is one published in Sweden about the year 1842, and two years later in an English translation, in London, under the following title: "The Theogony of the Hindus; with their Systems of Philosophy and Cosmogony. An Essay, by Count M. Björnstjerna." I may be allowed to add that all I have published respecting the north polar origin of the race was already in its third edition before I had seen, or had any knowledge of, this work.

For the further satisfaction of those readers to whom the work may not be easily accessible, it may be stated that the idea is by no means elaborated and formally presented as a scientific solution of the problem of the origin of man. On the contrary, it is so transient and incidental a suggestion on the part of the Count that the substance of all he says is found in two sentences on page 177 of the English translation, as follows:—"As, according to the nature of the thing [*i.e.* owing to the secular cooling of the earth], both the polar regions must have been prepared equally early for the reception of mankind, it is possible that the appearance of man took place at the same time in both regions; perhaps the white race in the countries about the North Pole, and the black race in those about the South Pole. A number of difficult problems might hence be solved." How singular it would be if this passing remark of a Swedish Count, writing upon the mythology of the Hindus, and more than a generation ago, should prove to be the watchword of the most advanced school of scientific anthropologists at the opening of the twentieth century. WILLIAM F. WARREN.

Bad Gastein, Austria, June 20.

Snow in Central Germany.

IN a note in NATURE of May 12, p. 42, it was stated that the quantity of snow which fell in Central Germany from December 19 to 23, between 50° and 52°·5 N. latitude, and between 7° and 18° E. longitude, weighed no less than 10,000,000 tons. I think there is a mistake in the calculation. Supposing that the snow was equivalent only to a stratum of water of 5 centimetres in height, its weight would be not 10,000,000, but 10,000,000,000 tons. OTTO KNOPF.

Berlin, June 21.

Meteor.

AT about 7.45 p.m. on June 19 a brilliant meteor was seen in broad daylight from this place. At a rough estimate it followed the meridian of Antares for about 30°, and disappeared near the meridian of that star. H. KING.

Chithurst, Petersfield.

Medicine in McGill University.

IN a criticism of my "Outlines of Lectures on Physiology" which appeared in NATURE for May 12, you say:—"Pathology or the application of physiology to disease, is hardly touched upon in this book. It is a most unfortunate omission unless both pathology and therapeutics are taught more systematically than with us." About three years ago "Institutes of Medicine" (then including physiology proper, histology, and pathology) was divided, and now these departments are each taught separately, and each is provided with its own laboratory. A systematic course of lectures and demonstrations in pathology is given, with instruction and practice in making autopsies (after Virchow). Therapeutics is taught from the physiological point of view, and also has its own laboratory. So that it only becomes necessary to make such reference to pathology, &c., in the lectures on physiology as suffices to indicate that the subject does bear on the study of disease, and thus interest the student in it from its bearing on his life-work.

It may be interesting to English readers to learn that very recently two of Montreal's citizens have given one million dollars to erect and endow a "Royal Victoria Hospital" in commemoration of the Queen's Jubilee. This hospital is to be located close to McGill University.

I make these statements simply in justice to the Medical Faculty. T. WESLEY MILLS.

Physiological Laboratory, McGill University,
Montreal, May 28.

The University of Tokio.

IN vol. xxxv. of NATURE, p. 401, it was stated that the recent amalgamation of the Engineering College and the University of Tokio occasioned the "total elimination of Europeans from the teaching staff, their place being taken by Japanese." Justice to the new University requires the correction of this statement, which is not only misleading, but erroneous. It is true that two well-known foreign Professors vacated their posts—one immediately after the amalgamation and the other within six months thereafter. Their place, however (for they taught the same subject), is soon to be filled by an engineer who is expected shortly from England. But giving full allowance to this temporary vacancy, any person who will take the trouble to compare the number of foreign Professors in the two establishments before the incorporation with the number after will find that "the total elimination" amounts to "one." Since the publication of the Calendar, again, no fewer than six have been added to the list of European Professors in the University. S. SEKIYA.

Imperial University, Tokio, April 22.

SCIENCE FOR ARTISTS.

ON many former occasions, furnished by the opening of the Royal Academy, the Grosvenor, and other exhibitions of pictures, we have made some remarks upon them from a specially scientific point of view, endeavouring in this way to note if any progress has been made in the treatment of natural phenomena by artists. This has also sometimes been accompanied by minute criticisms of certain pictures in which such phenomena have been admirably portrayed, or, on the other hand, travestied. Our remarks naturally have referred more to landscapes than to the other classes of pictures exhibited, first, because we have to deal chiefly with physical phenomena, and secondly, because, in representing the human form, artists have now for many years received such complete instruction that there is little chance of any gross error being committed. Why we think it worth while to write these articles at all is that, so far as we can find out, in no scheme of art instruction does the study of natural phenomena find any place, and one of our objects is to show that such instruction ought to be given to artists side by side with their anatomical work, in order to prevent them from making grotesque blunders which destroy all the artistic beauty of a picture, however well painted, in the eyes of the initiated. It happens very curiously that the various scientific points which are raised by the pictures exhibited vary from year to year. This alone would indicate the many points of contact between science and art, beyond those which we usually recognize. This year we think the questions raised by the pictures exhibited in the Royal Academy to which we now confine ourselves, for it really comes to that, are smaller in number than they have been for some time past, and are more restricted in scope. This arises, we believe, in a great measure from the very distinct improvement in the landscape pictures generally. The air has more atmosphere about it, the skies and clouds are truer in colour and form, the play of light upon water, the forms of waves, and many such points as these, to which reference might be made, have received better and more careful treatment. The most wonderful play of colour in Nature is brought before us at sunrise and sunset, and the only wonder is that artists do not pay more attention to the magnificent pictorial effects which are provided by these natural displays. This wonder, however, no doubt is greatly reduced when we come to consider the enormous difficulty of the problem. In the first place, there is no book, as far as we know, containing any statement in regard to sunset colours which will help an artist who wishes to paint them. Again, the play of colour in cloud and sky, in the objects illuminated by the fading and coloured light, varies incessantly; while, perhaps worst of all, the artist himself has to choose his colours from a palette which is illuminated by a light the colours of which are constantly changing as the sun gets lower and lower. In spite, however, of these enormous difficulties, artists have succeeded in producing sunset pictures of great beauty, nor are they absent in the present Academy Exhibition. No. 52, "Sunset after a Shower," is a case in point. Sunsets are not always so exactly alike as the painter of that picture paints them, as if pictures represented the different states of an etching, but the picture in question has many beauties about it, and, as all good pictures should do, it raises a question. In it we are supposed to be looking very nearly towards the place of sunset, and the sunset is a distinctly coloured one, as is evident by the colours on the clouds, and the very carefully painted zone of the sky getting warmer and warmer as the horizon is approached. The light in fact is so warm that the blue has disappeared from it, and almost the green. Under these circumstances there is no green light, or very little of it, to be reflected from the leaves and trees, which are not green in themselves of course, but only have the capacity of reflecting green light. We venture to think, then, that the trees in this picture are too green, and certainly greener than they would be ever likely to appear when they were backed by a sunset sky. In No. 682, by the same artist, the greenery of the trees would have been more in place, because in that picture the sunset colours are much less warm, albeit they are beautifully true to Nature, being caused by a different meteorological condition. We do not see in this year's Academy any distinct attempt to give us that glorious contrast one sometimes sees at sunset between brilliantly illuminated clouds, running through all the composite colours which are possible between red and yellow, backed by a "daffodil sky," as Tennyson calls it, or even one approaching an olive green. The nearest approach to such a green sky as this last we find in No. 990, which the artist funnily styles "Beneath Blue Skies." Surely the sky in this picture is green, and not blue.

On a former occasion, some years ago now, we ventured to put together a few notes regarding the hints that artists might glean, if they chose, in two or three hours' reading from any elementary books on astronomy about the moon. We are sorry to say that the moons in this year's Academy show that such advice still holds good, for in most cases the moons are wofully wrong. Funnily enough, there is a difference between the moons now and the moons of ten years ago. They were then far too large, now as a rule they err in an opposite direction. It may perhaps be imagined that the artist has no available means of drawing the moon to anything like the correct scale. This really is not so. If the artist has made up his mind that his picture shall contain, say, some 60° in the horizontal scale, so that six such pictures would enable him to give a complete panorama of the landscape around him from the place he has chosen to paint from, we have—provided the 60° are properly estimated—a perfect method of drawing the moon to scale, for the reason that as the diameter of the moon is about half a degree, so the diameter shown should be 1/120 of the length of the picture. We are inclined to believe that a moon of this size would really look rather too small, although, of course, it would be scientifically correct, for the reason that we have been fed upon large moons in pictures all our lives, and it is a part of our artistic education at the present moment to expect to see a large moon on canvas, and there is something uncanny about a small one, even if it is perfectly correctly painted. This artistic treatment of the moon will of course lose all its objectionable characters as the years roll by, and we shall not expect to see one thing in a picture and another thing, which the picture is supposed to represent, in the heavens.

In pictures 118 and 231 the moons would have been truer to nature if they had been slightly larger; but the worst of it is that this is not the only defect about them. Thus, dealing with 118, it is obvious that the sun is setting to the right of the picture; the moon, therefore, cannot have been full. If the artist thought he was drawing a gibbous moon, then he should have shown the imperfect edge of it away from the sun, and not below it as he has done. The fault in No. 231 is that the warm tone of the picture generally, indicates that we are somewhere about the hour of sunset; the sun has evidently not yet set, the luminosity of the picture shows that. Now we cannot get a full moon as high as the artist has represented his until after the sun has set, for a very simple reason which is known to everybody. We should like also to suggest to another artist that when he gets his picture, No. 137, back from the Academy, he should carefully take out the moon, because its presence shows that the sun is not setting, and if the sun is not setting, then the colours on the clouds are false; without the moon the picture would be entirely satisfactory to a keen observer of natural phenomena, assuming the sunset hour to be approaching, for the colours of the clouds are most beautiful. There can be no doubt, we think, that the study of cloud-forms is now not so neglected as it used to be. Some of the forms of clouds in many of the pictures this year indicate a very close attention given to them and to their floating changes by the artists; witness Nos. 459, 945, and 28.

The forms of water, too, and the illuminations of a water surface, are admirably represented in many pictures this year. The tracery of the water surface could scarcely be more exquisitely shown than in No. 118, the picture we have already referred to as possessing a most unfortunate moon. The little roller stretching nearly across 630 seems absolutely approaching us, while the view of the opening sea with the solitary sail in No. 659 positively makes one feel as if one were on a Cunarder, revelling in the fresh free air.

Although we have pointed out much that we hold to be very excellent in the way of sky and sea colour, we cannot help thinking that the chalkiness is on the increase: we never remember to have seen before so many seas and skies resembling chalk and water. This is an effect really very rarely observed in the sky, and not often in the sea, excepting close in shore, and in the absorptive properties of water we have a very good reason why it should not be so. Let the reader look at 223, 254, 274, 353, and 419.

There is no help for it, we must say a few words about rainbows. Why should an artist who has never seen a rainbow, or, if he has seen one, been so careless as not to take the trouble to look at it or to observe the order of its colours, why, we say, should he take the trouble to paint it? why does he not leave it alone? As most people know, the colours in the rainbow are regulated by a definite law—that is to say, the order of colours is the same. If we have red on one side of the bow, then we have to pass through orange, yellow, green, and blue, till we reach the violet on the other side. But in the two rainbows portrayed this year in the Academy, this order is not at all followed. One of the artists preferred to put the green between the yellow and the red, and the other thinks he has found a more excellent way. The result is that these gentlemen present us, under the guise of a rainbow, with a phenomenon which no mortal has ever seen or ever will see.

It is a matter, we should have thought, of general knowledge that a rainbow is caused by the action of raindrops upon the light which impinges upon them. In the annexed woodcut, we may imagine the beam S I a sunbeam entering a drop of water, a section of which is shown, A I I′. The beam is refracted on its entrance into the drop at I, is reflected at A, and is again refracted on emerging from the drop at I′. The light which originally entered the drop in the direction S I leaves it in the direction I′ M, and the eye, to see the raindrop, must be along the line I′ M. The observer, therefore, must obviously have his back to the sun, and the bow will appear to be at some distance above the horizon. If the sunlight could not be broken up into various colours by refraction, the bow would not appear coloured; but as refraction, which has two chances of working, does break up white light into its constituent colours, the emerging beam I′ M is coloured, and the order of colours must necessarily be the same as that which is observed by means of an ordinary prism or lustre. We begin with the warmest colours, the reds and oranges, outside the bow, the inside of the bow being formed of the cooler colours, blue and violet. This rainbow, which is the one observed under ordinary conditions, is formed by means of the rays of light falling on the outer portions of the drops, and is called the primary bow; as we see, the light suffers two refractions and one reflection in the drop.

There is another rainbow seen, when the conditions are entirely favourable, outside the former or primary one. This is called the secondary bow; it depends for its existence upon the light which falls nearer the front surface of the drop, a condition of things shown in the next woodcut, in which S I represents the light which is refracted at I, reflected internally at A, reflected internally

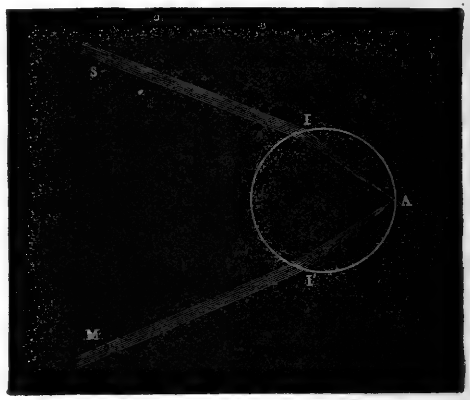


FIG. 1.—Path of light in producing primary bow.

again at B, and refracted at I′, and sent along the path I M to the eye. The eye, which receives it therefore in the line I′ M, receives it after two refractions and two internal reflections.

In this secondary bow the order of colours is reversed. We get the warm colours, the reds and the yellows, inside, and the blue and violet outside, so that when the primary and secondary bows are both seen, the two reds are together, and the two blues as far apart as they can be.

When we talk of the spectrum colours in the rainbow, it must be always understood that the appearance is not that assumed by an absolutely pure spectrum, for the reason

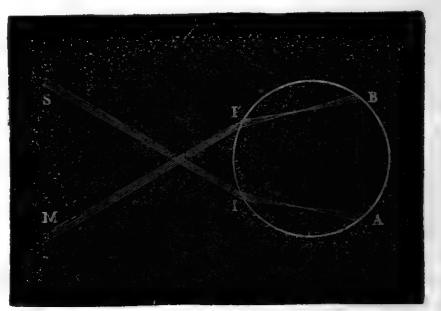


FIG. 2.—Path of light in producing secondary bow.

that the sun, the light of which is in question, has a disk of visible magnitude, and that the ray of light coming from each point of the sun is competent to produce a rainbow. Hence the rainbow is a very composite thing, and really consists of millions of spectra overlapping. The more the overlapping is intensified, the more the pure colours disappear, and hence it is that the rainbows seen when the sun is behind a cloud are very often seen without any trace of colour. The more the light proceeds from a large surface, the dimmer and less clear will the rainbow become, until at last it fades into invisibility.

An artist in painting a rainbow, then, has to consider that the colours can never be absolutely pure, and that their order is absolutely invariable; and the first thing that one must expect in a picture painted by an artist of intelligence is that this order shall not be interrupted or travestied.

In 624 a very interesting question of perspective is raised. If we imagine a room with an atmosphere very thickly laden with dust, and imagine further this room to be illuminated by a window with an ordinary window-sash: if the sun shines into the room, the sashes will cast their shadows on to the dust-laden air. As the sun is 93,000,000 of miles away, or thereabouts, the shadows of all the horizontal sashes and of all the vertical sashes will be parallel. If on the other hand, instead of letting the sun throw shadows of the sashes in this way, we imagine a strong light not far away from the window to do it, the shadows of the two series of sashes will be no longer parallel, they will diverge, and the nearer the light is to the window the more they will diverge. It is of course quite understood that in the room itself the parallel shadows cast by the sun could not appear to be truly parallel, for the reason that one part of the shadow must be nearer the eye than the other, but we cannot help thinking that the difference between this condition and the one which holds when the source of light is close to us, has not been sufficiently taken into account by the artist, so that a sun 93 yards off instead of 93,000,000 of miles would have been very much more likely to produce the effect shown in the picture.

A REVIEW OF LIGHTHOUSE WORK AND ECONOMY IN THE UNITED KINGDOM DURING THE PAST FIFTY YEARS.[1]

III.

THE growth of improvement in lighthouse towers, lanterns, and apparatus has been glanced at. The source of light, or lamps and their aliment, must now be considered. It is probable that the *phari* of antiquity were open wood fires of great size on the summit of high towers or headlands. "*Ignes*" and "*flammis*" are terms used by Pliny and others, and Statius compares the *pharos* to the moon, not to a star as a modern poet would rather do. Yet Lucan speaks of "*lampada*," and Pliny fears that the flames might be mistaken for a constellation. But in these times oil could hardly have been used, as no form of lamp known could be applied with success. For 2000 years the illuminant was mainly wood or coal. The Cordouan, in 1610, was kindled with oak logs. Coal fires were burnt at Harwich in the end of the eighteenth century. The Lizard was a coal fire in 1812. St. Bees ceased to be one only in 1822. The Isle of May remained a coal light for 181 years. It is now the single specimen of the electric light in Scotland. Sperm oil was not used before 1730, and then but on a small scale until the burners of Argand in 1783 and the reflectors of Teulère in the same year changed the character of lighthouse illumination. The Eddystone in 1759 threw its first beams over the waters from ten pounds of tallow candles, for which, in 1811, wax was substituted.

But in 1837 sperm oil was the general aliment for our catoptric lights. In that year the oxy-oil lamp was proposed by Mr. Gurney. The principle of this light, known as the Bude, was a stream of oxygen injected into an oil flame, and it has since been tried with gas flames. It was followed by the Drummond lime-light, and by ignited platinum wire and various pyrotechnic mixtures. The Bude and Drummond lights were tried by the Trinity House without successful result. In 1845 a Parliamentary inquiry on oils led to the choice of rape-seed as a substitute for spermaceti, and in 1860 vegetable oil was being used everywhere, with perhaps a little gas for small lights. The single lamp of the dioptric system was then in England and Ireland the "fountain," and in Scotland the mechanical or clockwork lamp, as used in France, both having four concentric wicks. It was with this lamp that Fresnel established his first light at the Tour de Cordouan in 1822. So far as can be ascertained, the electric spark was first practically suggested for a lighthouse in 1852 or 1853 (Holmes), or in 1854 (Watson), as will be later referred to. In 1860, Prof. G. B. Airy wrote to the Royal Commissioners on Lights:—"At present the great excellence of a lighthouse is, or may be, the optician's part. The great defect and waste is in the source of light." Coal gas had been introduced in 1837 at the inner pier light of Troon (Ayrshire), and in 1847 it was used in the Hartlepool Heugh dioptric sea-light. From 1865 to 1867, gas was proposed for lighthouses in Ireland, but not officially adopted. In the same period, mineral oil, which had become familiar to English people in domestic lighting, and had been used in French lighthouses in lamps of a single wick and apparatus not larger than the fourth order, was much discussed as a suitable illuminant for sea-lights. After a long course of official experiment and inquiry, the unreserved use of mineral oil was authorized for lighthouses on land, and the Flamborough Head was the first Trinity light to receive the new illuminant (1872). One name is here worthy of distinction. Capt. H. H. Doty may justly be regarded as the chief demonstrator of the "promise and potency" of mineral oil. He also constructed a burner with multiple wicks which produced steady and brilliant flames. This burner is not, however, novel in its elements or combinations, and other petroleum burners of equal and superior merit have since been introduced. It is not on the Doty burner itself that his reputation is best founded, but on his strenuous and intelligent advocacy of mineral oil, and on his practical application of it to a multiple burner. It is gratifying to know that his services have been for this reason recognized by grants of money from the Governments of England and France.

Since 1872 the use of petroleum has been more and more extended, and it is now a trusted and perfectly safe illuminant. Until recently the variety known as "Young's lighthouse oil" was exclusively adopted by the Trinity House, its flashing-point being not lower than 142°, its specific gravity 0·81. Later varieties of it have a flashing-point of 154°. This fluid does not rise to the level of the top of the burner, but is confined to a certain distance below, whence the cotton wicks are charged with it by capillarity, and it is the vapour or gas that is ignited. The absence of overflow leaves the tips of the burner dry and unrefreshed, and therefore subject more or less to rapid deterioration. But in the heavy mineral oil lately recommended by the Trinity House, the specific gravity is between 0·828 and 0·832 (at 60° F.), and the flashing-point is not lower than 250°. This oil, therefore, may probably be allowed to overflow the burner like colza. There is also a very useful variety, under the name of "mineral sperm," which was first introduced by the writer into harbour and ships' signal lights. The flashing-point has reached 270°.

The saving of expense in using mineral oil in a lighthouse may be understood thus. A six-wick burner of the best Trinity type consumes, when at full power, 79·4 fluid ounces, or half a gallon hourly. In a year of about 4000 hours this would cost perhaps £70. Vegetable oil in the same quantity would cost perhaps £250. There would be no appreciable difference in intensity of light, but much in favour of mineral oil in the facility of service and in the smaller consumption of wicks. *Pari passu* with the adoption of this illuminant has been the improvement in the pressure and pump lamps and their burners effected by the Trinity House and by Messrs.

[1] Continued from p. 181.

Chance. It is, above all, to Sir James Douglass that credit is due in this field. For at least eighteen years he has worked unweariedly, and in the interest of the public service alone, at the perfecting of the burners which bear his name, whether for colza, paraffin, or gas; and some striking developments have been attained by him. For instance, the typical four-wick vegetable-oil burner of Fresnel had an intensity of 230 candle-units, or 23·6 units per square inch of sectional area. The Trinity four-wick has an intensity of 415 candle-units, or 44·3 per square inch. The Trinity six-wick, perhaps the most serviceable and complete burner ever constructed, equals 730 candles, or 40·1 per square inch. The Trinity seven-wick equals 1100 candles, or 46·9 per square inch, and the Trinity nine-wick equals 1785 candles, or 49·8 per square inch. These burners are all for vegetable or mineral oil. The Trinity six-ring gas-burner equals 853 candles, or 44·4 per square inch; and, which appears to be the most powerful combination ever attained, the Trinity ten-ring gas-burner reaches 2619 candles, or 50·9 per square inch of sectional area. The admirable expanding gas-burners of Mr. Wigham are hardly less powerful. They are formed of concentric circles of jets from 28 to 108 in number, disposed so as to suit optical apparatus of several degrees of size, and weather of every degree of clearness. To this gentleman must be accorded the same pre-eminence in the skilful use of gas for lighthouses as to Capt. Doty for the skilful use of mineral oil. His ingenious combinations and contrivances, not only in regard to power, but to distinctiveness of character, are seen to great advantage in Galley Head and other notable Irish lights. It has been urged against gas flames of the largest dimensions on this system, that a portion of light escapes lenticular action, yet this very ex-focality has been found to have a useful side, for it tends to produce a glare or glow in the heavens, visible to mariners when the tower is beneath the horizon, and, in some circumstances, positively useful to them. (The electric light produces a similar effect, though in a different manner.) A more serious objection to large gas flames, especially when used in triform or quadriform series, appears to be the excessive heat, which is capable of injuring the delicate optical glass, and is hardly favourable to the keepers. It is probable that the hyper-radial apparatus just introduced may, both as relates to condensation of light and to mitigation of heat, be well suited to gas-burners of these striking magnitudes. Of the thirty-two dioptric sea-lights in Ireland, about one-fourth are successfully endowed with the gas-illuminant. Of the sixty-five in England, the Haisboro' is the only case. There is no gas in the fifty-one Scottish sea-lights, except Ailsa Craig, which has oil-gas. It should be added that the compressed mineral-oil gas of Messrs. Pintsch, and the petroleum spirit of Herr Lindberg, for the automatic lighting of buoys, have been, since 1878, tried in this country with great success.

The third illuminant, electricity, has been known in England for about thirty-five years. As generated in the magneto-machines of Prof. Holmes between 1853 and 1862, and as tried experimentally in the lighthouses of Dungeness and South Foreland, it was very small in dimension and very uncertain in character. Several forms of the light were suggested during this period, such as the voltaic arc of Watson and the mercurial electric lamp of Way. With the more effective alternating current machines, and with the larger carbons, of later years, the arc grew in power and dimension. At the present time carbons of from 25 to 40 millimetres are available, with an intensity in the focus of a light of ten times that of the most powerful gas or oil burner. The arc is thus become a most valuable resource, not merely for its unsurpassable power, but also for its focal adaptability to the usual dioptric apparatus, and to special optical combinations dictated by nautical circumstances. It is most flexible in its application. It radiates no harmful heat It has the high merit of not exacting any abnormal dimensions of apparatus, lantern, or tower. Lastly, being the most powerful in all its gradations relatively to other illuminants, it is the cheapest of all lights if the cost of establishment and maintenance be computed in terms of the units of the beam transmitted, which is the only strictly logical and practical way of treating it. For these reasons it has been chosen in France as the best illuminant for a large number of coast-lights, and it is making rapid way in Europe and America. It may therefore be safely asserted that the electric light, when it shall have been freed from its last disabilities, and shall have attained its utmost development, will, in the not distant future, be the prevailing illuminant of our own lighthouses and of the other chief lighthouses of the world.

In illustration of the power of the electric arc with suitable optical treatment, I may mention that the direct beams of the Tino light, near Spezia, were observed on April 20, 1885, by Prof. Noceti, from the hill S. Giorgio, behind Savona, at an elevation of 2733 feet, and a distance of 73 statute miles, the atmosphere being clear and under moonlight. The beams of the arc were notably brighter than those of the *lanterna* at Genoa, at one-third of the distance. Frequent observations are reported of the Macquarie light in New South Wales, at ranges of 60, 65, and 70 nautical miles, by means of reflections on the sky while the light itself is below the horizon.

The relative merits of gas, oil, and electricity, were established in the prolonged official experiments at the South Foreland in 1884-85. It has been proved that there is no appreciable *qualitative* difference between mineral oil and coal-gas as light-giving agents; and that such differences as appeared were rather *quantitative*, arising from the number and dimensions of the burners, and the modes of their collocation or superposition. It has been proved also that the electric arc (in addition to its superiority in clear weather, which was never in question) has an absolute superiority in thick weather to both gas and oil—"the greatest penetrative power in fog." Much public controversy has been excited by the Report in which conclusions like these are embodied. The fairness and impartiality of persons concerned in the investigation have even been impugned, and objection has been taken to the manner in which the electric light was presented to the observers, and to the refusal of the Trinity House to accept certain maximum arrangements called "double-triform" and "double quadriform." But to anyone reading the Report of the Trinity House (1885) with no bias toward a particular interest or a pre-conceived theory, it must appear that the inquiry was as exhaustive as it was prolonged, and that it is impossible that such names as those connected with it—names eminent in science, in engineering, and in the nautical and official world—should be associated with any other desire than the desire to shed light on a vexed technical question, and to achieve honourably and thoroughly a great public work. With regard to the exclusion of maximum combinations from the Foreland programme, it was obviously sufficient to compare gas and oil in their respective primary burners, multiplied or combined in such a way as, while insuring equal terms or nearly so, to reproduce the actual or allowable conditions of a lighthouse; and nothing would seem to be gained by augmenting the rival elements *pari gradu* to ampler and ampler bulk regardless of all else. The inter-relation of the numbers one, two, three, is not affected, or very slightly so, by raising them to two, four, six, or to four, eight, twelve. And although the highest power of the initial flame or the emerging beam were reached according to the opinion of the moment, the next day might suggest a still higher power, until it became clear that we might as well revert to the old beacon-fire on the headland, for indeed with unlimited tar-barrels or profuse pine-logs a light *could* be kindled exceeding everything yet achieved by gas, oil, or electricity, and visible not only on the horizon, but across half the midnight sky.

Phonic signals as auxiliary to luminous signals have engaged the attention of our lighthouse authorities from dates previous to 1837, and almost continuously from 1848 to 1875. The early instruments were the bell, the gun, and the gong, with sometimes an explosive such as a rocket. Between 1848 and 1850, Mr. G. Wells produced his patent "fog screamer" (by atmospheric pressure), which, however, did not meet the approval of the Trinity House, who, in 1853, considered that a good-sized bell struck sharply by machinery surpassed any mode yet tried. During the next ten years experiments on fog-signals were carried on in France, and in 1864 there was an important investigation by the Government of the United States. In 1872 a Committee of the Trinity House visited that country and Canada, and tested the merits of the new sound instruments in use, chiefly the Daboll horn actuated by air, and the siren actuated by steam. The Canadian and American steam-whistles and the New York siren, together with air-whistles, air-trumpets, and some guns, were next employed in the most complete scientific and practical inquiry ever held into the laws that regulate the passage of sound through the atmosphere, and into the mechanical agents most suitable to be adopted. The experiments were conducted by the Trinity House at the South Foreland in 1873-74, under the superintendence of Dr. Tyndall, who was able to demonstrate that fog or heavy rain is permeable by sound to a degree never before understood, and that optical transparency might be combined, even as cause and effect, with acoustical opacity or turbidity, and *vice versâ*. These results attracted much attention, and although Prof. Tyndall's inductions as to homogeneity or non-homogeneity of the atmosphere have been to some extent questioned, the large body of facts on which they rest has been still further enlarged and confirmed. It follows that a fog dense enough to quench all light may permit sound to be transmitted with unimpaired distinctness; and where the sound, either by alternations of pitch or of interval, is made a substitute for the characteristics of a light-signal, a very valuable secondary set of signals is realized. Of the instruments tried at the South Foreland the siren was found to be the most effective in almost all circumstances. This instrument was the work of Mr. Brown, of New York, to whom a simple and powerful form of caloric engine is also due. It consists of a trumpet about 17 feet long, increasing from 5 to 27 inches in diameter, having two disks in it, one fixed and one rotating, with radial slits in them. The rotation is from 2000 to 2500 times in a minute, steam at from 70 to 80 lbs. pressure being supplied. A note of varying pitch and intensity, audible at distances from 3 to 10 miles is thus generated. The siren in another form was improved by Dove and by Helmholtz, and previously by Cagniard de la Tour, who gave it this name, presumably on the *lucus a non lucendo* principle It is now employed at many first-class land lighthouses where space exists for the needful steam or caloric motor. Truly for the help not the harm of the mariner, in the words of the poet, "*Siren assuetos effudit in æquore cantus.*" It is possible that the recent disaster off Dieppe might have been averted if the *Victoria*, moving doubtfully through the fog, could have heard the steam-trumpet on Cap d'Ailly, which seems to have been disabled at the critical time. Both the range of the siren and the facilities for working it have of late been enhanced by the methods of Mr. Charles Ingrey, who, by employing air compressed by engines of 40 horse-power, the air-pressure being 80 lbs. per square inch, has, in the case of the Ailsa Craig establishment, produced from two sirens a sound audible, it is said, at a distance of 20 miles in calm weather. One of these instruments gives single blasts of 5 seconds duration, the other a high, low, and high note in series, each of 2 seconds, with intervals of 2 seconds between them, followed by 170 seconds of silence. This is the phonic analogue of a single-flashing and of a group-flashing light respectively. The compressed air is conveyed from a considerable distance to the siren-house, and Mr. Ingrey is confident that he could work the instrument from an engine placed 10 miles away. After the South Foreland experiments of 1874, the Trinity House proceeded to improve the gun as a sound-signalling agent. It now ranks as second to the siren alone. Gun-cotton is proved to be a more effective charge than powder, and it has been supplied with the gun to a few lightships; but the siren is for principal stations, and the gong, or bell, or an explosive mixture, for others.

The details so far given, though necessarily incomplete, illustrate the notable progress in lighthouse design and construction attained in this country since the accession of our Queen, and not less do they show the increasing number of the lights established on our shores. . Along with France and the United States—and due honour must be accorded to the eminent men representing them—Great Britain has proceeded steadily in the path of investigation and experiment. And here the labours of the celebrated Royal Commission of 1859-60 on lights, buoys, and beacons should not be overlooked. This Commission collected from all maritime countries and from the leading authorities in official life, in engineering and nautical science, in mathematics and physics, a vast body of evidence which to the careful student will not prove the *rudis indigestaque moles* it at first sight appears. Some of the recommendations of the Commission have been fully carried into effect during the last quarter of a century, *e.g.* the proper adjustment of optical agents to the flame and to the sea horizon, the development of lamps and burners, the provision of reflectors, testing of foci, &c. The conclusions also of the Commissioners on the complicated and anomalous system of lighthouse government formulated by the Merchant Shipping Act of 1854 have never been impugned; and the expediency of a central Lighthouse Board as suggested by them, and as indeed had been suggested by the Parliamentary Committee of 1834, has become more and more evident down to the present day.

But while Great Britain has, in common with France and the United States, pursued this path of inquiry and reform, she has distanced these countries altogether in the results of research and the realisation of improvement. The splendid gift of the dioptric system was made to the world by the genius of Fresnel, yet little has been added to it by his countrymen. The most solid and important additions and applications are the work of Scottish and English engineers, whether in the optics or the mechanics of lighthouses, whether in oil, gas, or electricity. And the gift of Fresnel has thus been returned enhanced three-fold to France and to the world. How it has been received is apparent by this one indication: the yearly statistics of our Admiralty comprise forty lighthouse notices issued to mariners in 1862, and 311 issued in 1886, the subjects of these notices being mainly new lights, and the new lights being mainly on the most modern lenticular system.

NOTE.—Since the above was written the small circle of men associated with lighthouse illumination has been broken by the death of its most distinguished member, Thomas Stevenson, who, during the whole of the half century under review, did more than any other to multiply for engineers the resources of his science, and to diminish for all the world the manifold perils of the sea.

The extraordinary power of the electric light has been referred to in connexion with the apparatus of Isola del Tino. In a recent communication to the *Standard* from "C. P. S." from Via Reggio, further testimony is given to this power in clear weather, but a far more important and controversial point is conclusively dealt with. The writer says:—"Again, though dimmed by heavy rain and thick fog, as it has been during the last few nights, the triple flash is always clear and unmistakable, and then produces, through the quasi-opaque atmosphere, and at a distance of thirty miles, the effect of the blurred disk of the moon on a small scale. This remarkable penetration power of the Tino light is conclusive proof, not only of how admirably it is designed and suited to its essential purpose as a guiding light under the peculiar atmospheric conditions of the Mediterranean, but also how hazardous it would be to dip—viz. to divert such a light, as has been suggested by some—from the horizon to the nearer sea in foggy weather, forsooth according to the *bene placito* of the man in charge, on the presumption that in such weather the luminous rays could not reach the horizon, and would therefore be wasted. This presumption is wholly fallacious in the Mediterranean, for in the Bay of Spezia, owing to its proximity to the Apennines, the rainfall is much greater than in other parts of the Tyrrhenean Sea, and banks of land fog can often be seen hanging over the bay and Tino, when the horizon as far as Leghorn, Gorgona, and even Corsica, is perfectly clear."

June 1887. J. KENWARD.

REPORT OF THE BOARD OF TRADE ON WEIGHTS AND MEASURES.

ONE of the many official Reports which are laid annually before Parliament, but which unfortunately are not so carefully read by the general public as they might be, is a Report by the Board of Trade on their proceedings and business under the Weights and Measures Act of 1878. In Report No. 9, Sess. II., 1886, there is something of scientific interest to which we would invite the attention of our readers.

The only two units from which all Imperial measures and weights are derived are, as is well known, the yard and the pound, and material standards of these two units are deposited with the Board of Trade. The Act provides that an accurate copy of each of these standards is to be made, and an account of the verification of a new copy of the yard measure is given in the present Report. The results of the comparisons of the new yard, P.C. VI., with the Imperial standard yard No. 1, show that it differs little from the original standard:—

$$\text{P.C. VI.} = \text{No. } 1 - 0{\cdot}0000034 \text{ inch. } t = 62^\circ \text{ F.}$$

There are no two primary standards between which our present scientific methods cannot measure some difference, but the above two standards would appear to be as nearly alike as it is possible to make them.

In determining the rate of expansion by heat of the new standard yard (which is a bronze bar 36 inches long), it was found that with a rising temperature, the new bar expanded 0·000356 inch for 1° F., but with a falling temperature it contracted at a lesser rate, 0·000343 inch for 1° F. It is not stated whether this curious difference in the rate of expansion, as determined when the temperature is alternately rising and falling, is owing to the march of the mercurial thermometers or to other causes. Some doubt has arisen as to the rate of expansion of the metal (bronze) of which the Imperial standard was made. The thermometric expansions stated in the Report of the Astronomer-Royal on the construction of the Imperial standard (*Phil. Trans. Roy. Soc.*, part iii., 1857, p. 61) do not agree with those stated by Colonel Clarke and Sir Henry James in their Report on the comparisons of standards of length (1886). The actual rate of expansion of the Imperial yard was, in fact, not determined by the Standards Committee in 1857, but was assumed to be the same as that of other similar bars of the same metal. The more recent experience, however, is that no two bronze bars expand by heat at precisely the same rate, although they may be of the same dimensions, form, and material. The co-efficient of expansion of an alloy is slightly affected by differences in the age of the alloy, by its being subjected to extreme variations of heat and cold, and by peculiarities in molecular condition.

With reference to the metric system, we are glad to see that during 1886 standards of metric weight and measure were verified for certain authorities for use in scientific research or otherwise. It would, however, appear that in Japan, in the competition for commercial acceptance, the British system is at present outstripping the metric system.

The legal equivalent of the metre is 39·37079 inches, but, as some doubt has been expressed as to the scientific accuracy of this equivalent, Prof. W. A. Rogers, of Colby University, has undertaken to construct for the Standards Department a subdivided standard yard and metre, on which the precise length of the two standards shall be marked off, so that an exact inter-comparison of the two standards may, as far as practicable, hereafter be made at London and at the Bureau International des Poids et Mesures, at Paris.

In this Report we have for the first time complete and trustworthy information as to the standard weights and measures in use in China and Japan, Her Majesty's ministers at Pekin and Tokio having obtained the information from the native Governments and through the different local consuls.

During the year 1886 the Standards Department was specially engaged in the re-verification of the accuracy of its own standards, and in the issue to local authorities of some suggestions with reference to the duties of inspectors of weights and measures. Amongst the re-verifications particularly referred to we notice a memorandum on the re-verification of the gas-measuring standards, a memorandum which shows the several conditions necessary for the accurate measurement of gas used for lighting purposes. The accuracy of the unit of volume, the cubic foot, is made to depend on the Imperial standard pound, and not on linear measurement. Experience has hitherto shown that the determination of the weight of a cubic foot of distilled water may best be made by means of a round vessel which holds a quantity of water equal to 62·321 lb. avoirdupois at 62° F., rather than by a vessel of rectangular shape, which might be made to measure one foot in each of its dimensions. In this memorandum reference also is made to a slight difference in the methods of determining the zero, or freezing-points, of thermometers. It is, for instance, uncertain how long a thermometer should remain in melting ice or snow before its precise freezing-point is noted. At the Bureau of Weights and Measures at Paris, for instance, the thermometer is only left in the pounded ice just long enough for it to reach the maximum of depression. With thermometers made of hard glass it is stated to be desirable not to hurry the observations—the thermometers remaining long enough to allow the use of a micrometer, and several observations to be taken; but with other kinds of glass it is found to be desirable to be as quick as possible. This practice is also stated to be adopted at the Standards Offices at Berlin and Washington. On the other hand, at Kew the freezing-point of a thermometer is not observed until the instrument has been completely buried, both bulb and stem, up to 32° F., or 0° C., in well-pounded block ice for a period of not less than a quarter of an hour, care being always taken in cold weather to insure the whole of the ice being in a melting condition during the experiment by pouring a small quantity of tepid water over it from time to time.

Unlike the determinations of the rate of expansion of gas, there has not been made any determination of the rate of expansion of water which in exact experiments might be accepted as conclusive, and hence, in determining the weight of a volume of water at any given temperature, the Standards Department have been advised to adopt a mean result from several selected determinations, as those of Despretz, Kopp, and Pierre.

In many technical works, measurements of gas are erroneously referred to the temperature of 60° F., and not to the legal temperature of 62° F., at which temperature alone the standard foot contains 12 inches; the standard gallon 10 lbs. weight of water; and the standard pound 7000 grains. It would appear that an error originally committed in certain hydrometer tables in taking 60° F. instead of 62° F., has been followed by many chemical authorities.

The weight of a cubic foot of ordinary air has been still taken by the Standards Department after the determinations of Regnault, as corrected by Moritz, Broch, and Agamemnone. The amount of carbonic acid present in ordinary air has been taken, after the inquiries of Parkes and Angus-Smith, at 6 volumes in every 10,000 volumes of air. If double the quantity, 12 volumes, is present, as is sometimes the case in common rooms, it will make a difference of about 0·18 grain in the weight of the cubic foot of air. Ordinary air is still taken, after Regnault, as being two-thirds saturated with moisture.

In calculating the true weight of any given volume of air or of gas we may, of course, on very rare occasions have to allow for the accelerative effect of the force of gravity at the latitude of the place where the air or gas is weighed, as well as for the height of the place above sea-level. The normal latitude adopted in all such experiments is that of 45° at sea-level.

For instance, the weight in grains of a cubic foot of ordinary air ($t = 62°$ F., $B = 30$ in.) at London (lat. = 51° 29′ 53″), at Edinburgh (lat. = 55° 57′ 20″), and at Dublin (lat. = 53° 20′ 38″), has been taken as follows:—

	Dry air.	Air two-thirds saturated.	Moist air saturation = 1.
	Gr.	*Gr.*	*Gr.*
Edinburgh	534·42	531·92	530·68
Dublin	534·30	531·81	530·56
London	534·21	531·72	530·48

From time to time, on the application of local authorities, suggestions are issued by the Board of Trade as to the best modes of testing the weights, measures, and measuring instruments used for commercial purposes. In this country the local inspectors are not bound to follow official instructions as they are in other countries, but are free to carry out their technical work in such a way as the Justices and Town Councils may approve. It is therefore only by the issue of such official suggestions that uniformity of local practice can be at all reached, and some amount of co-ordination and local effectiveness thereby secured. It is perhaps to be regretted that there is in this country no central authority like the Normal Aichungs Kommission of Germany or Austria to give force and life to the whole local system; not that we would have the local officers in this country drilled to the dull sameness of official uniformity in the way they are drilled by some Continental Governments, but the absence of a proper scientific training by our local inspectors often leads to complaints from traders and manufacturers. By these official suggestions the Board of Trade endeavour, therefore, to educate local officers in their technical work and to keep them so far in touch with modern progress.

The present Report contains a paper on the testing of weighing-machines, which should be of really practical use to the local officers, for it is the first time that any instructions have been published as to the mode of testing such machines.

Amongst other appendixes to this Report we find papers relating to the well-known model apparatus, designed by Sir F. Abel, for testing the flashing-point of petroleum; abstracts of returns from local officers; notes on the sale of coal by weight and the sale of intoxicants by measure—with reference to which it would appear that there is more petty fraud than ever amongst traders; and a note on the average current weight of the sovereign. In the latter note reference is made to a number of weighings of gold coin, which have been recently made at the leading Banks in London. The results of these weighings show that most of the gold coinage in circulation has really ceased to be legal tender as to weight. Nearly all the coins which were weighed were found to be slightly below the least current weight allowed by the Coinage Act. If the present law, which requires receivers of light gold coins to cut or deface them, were really obeyed, then it would appear from this note that six sovereigns out of every seven ought strictly to be cut or defaced. This seems to be a worse state of things than when Prof. Jevons made his well-known report on the metallic currency of the United Kingdom in 1868.

THE GERMAN METEOROLOGICAL OFFICE.

A HISTORY of the Royal Prussian Meteorological Institute from the time of its establishment in 1847 until its re-organisation in 1885, by Dr. G. Hellmann, has just been published in the year-book of the Institution, "Ergebnisse der Meteorologischen Beobachtungen im Jahre 1885" (Berlin, 1887, 246 pages, large 4to, with plates). Dr. Hellmann is well-known to students of meteorology by many very valuable articles, and especially by his laborious compilation of a "Repertorium der Deutschen Meteorologie," containing a list of the articles, inventions, and observations in the domain of Meteorology and Terrestrial Magnetism in Germany from the earliest times down to the year 1881 (Leipzig, 1883, 995 pages, large 8vo). The kingdom of Prussia was relatively late in organising a regular system of observations, Baden and Bavaria in Southern Germany having established well-appointed services before the end of the last century; and Würtemberg followed with its system in 1821-2. The want of trustworthy data for Northern Germany was much felt by Baron A. von Humboldt at the time of the construction of his first isothermal charts in 1817, and the establishment of the service in Prussia was due to the urgent representations which he made to the present Emperor. In 1847 a system of 25-30 stations was established under Dr. Mahlmann, and observations were taken at the hours of 6, 2, and 10; these hours have been generally adhered to both in Germany and Austria down to the present time. Before commencing operations, all the stations were duly inspected, and suitable observers selected, mostly from teachers in the upper schools. While neither instruments nor remuneration are provided for such observers in this country, in the Prussian system an annual allowance, varying from about £7 10*s*. upwards, according to circumstances, is made to many of the observers, together with an outfit of instruments. The result of these arrangements has been that probably in no other system upon the globe have so many useful works been published by the various observers, upon whom generally devolved the task of working up their own observations. Dr. Mahlmann having died suddenly on one of his tours of inspection, his work was taken up in April 1849 by the late eminent Prof. H. W. Dove, of the University of Berlin, and his first care was to revise the observations hitherto taken and to publish them in a first Report of the Observations taken in 1848-9. The publication of this Report induced several other states to join the Prussian system, many of the observers now taking up the work without remuneration, and this active co-operation enabled Dove to publish for 1855, and for subsequent years, a summary of observations for each month of the year for Northern Germany, and in 1858 a first sketch of the climatological conditions based upon ten years' observations. Prior to this publication these conditions were almost unknown for Prussia. Some of the stations were inspected yearly by Dove, but strange to say, it is stated that not a single Report of these inspections is to be found in the archives of the Institute. Among the numerous treatises by Prof. Dove, that best known is his work on the "Law of Storms," which was translated and adopted in this country. After Dove's death, in 1879, the Institute introduced the French measures in its publications, and adopted generally the recommendations of the various International Congresses, to which innovations Dove himself had always been averse, and instruments with new scales were necessarily supplied to the stations. In 1882 Dr. Hellmann was intrusted with the *ad interim* direction of the Institute, and many additional stations, especially for rainfall, were added to those which already existed, and finally (in 1885) the Institute was placed under the able superintendence of Dr. W. von Bezold, formerly director of the Bavarian system, with Drs. Hellmann, Assmann (also Director of the Magdeburg Observatory), Kremser, and Wagner, as principal assistants. The first volume of the new office has just appeared, and contains the observations at 271 stations during the year 1885 (246 pages, 4to, and 6 litho. tables), and also lists of all observations made since 1847. The stations are still very unequally distributed over the Empire, and no doubt improvements will be made in this respect, from time to time. It is plainly shown from the tables that while an open country position is most suitable *meteorologically*, yet for *duration* of the observations the large towns are preferable. These observations formerly appeared in the "Preussische Statistik," and in the publication of the Deutsche Seewarte, but will henceforward form an independent work. It is proposed in future to issue the tabular portion in quarterly volumes, and to publish pamphlets at irregular intervals under the title of "Abtheilungen," containing papers and discussions of a general nature. The Deutsche Seewarte at Hamburg is an independent Institution, dealing chiefly with maritime meteorology and weather telegraphy. J. S. HARDING.

THE HEIGHT OF SUMMER CLOUDS.

A KNOWLEDGE of the heights and movements of the clouds is of much interest to science, and of especial importance in the prediction of weather; the subject has therefore received much attention during recent years from meteorologists, chiefly in this country and in Sweden. In the last published Report of the Meteorological Council for 1885-86 will be found an account of the steps taken by that body to obtain cloud-photographs; and in the *Meteorologische Zeitschrift* for March last, MM. Ekholm and Hagström have published an interesting summary of the results of observations made at Upsala during the summers of 1884-85. They determined the parallax of the clouds by angular measurements made from two stations at the extremities of a base of convenient length, and having telephonic connexion. The instruments used were altazimuths, constructed under the direction of Prof. Mohn, specially for measuring the parallax of the aurora borealis. A full description of these instruments and of the calculations will be found in the *Acta Reg. Soc. Sc. Ups.* 1884. The results now in question are based upon nearly 1500 measurements of *heights*; the *motions* will form the subject of a future paper. It was found that clouds are formed at all levels, but that they occur most frequently at certain elevations or stages. The following are, approximately, the mean heights, in feet, of the principal forms:—Stratus, 2000; nimbus, 5000; cumulus (base), 4500, (summit) 6000; cumulo-stratus (base), 4600; "false-cirrus" (a form which often accompanies the cumulo-stratus), 12,800; cirro-cumulus, 21,000; cirrus, 29,000 (the highest being 41,000). The maximum of cloud-frequency was found to be at levels of 2300 and 5500 feet. Generally speaking, all the forms of cloud have a tendency to rise during the course of the day; the change, excepting for the cumulus-form, amounting to nearly 6500 feet. In the morning, when the cirrus clouds are at their lowest level, the frequency of their lowest forms—the cirro-cumulus—is greatest; and in the evening, when the height of the cirrus is greatest, the frequency of its highest forms—the cirro-stratus—is also greatest. With regard to the connexion between the character of the weather and the height of the clouds, the heights of the bases of the cumulus are nearly constant in all conditions. The summits, however, are lowest in the vicinity of a barometric maximum; they increase in the region of a depression, and attain their greatest height in thunderstorms, the thickness of the cumulo-stratus stretching sometimes for several miles. The highest forms of clouds appear to float at their lowest levels in the region of a depression. The forms of clouds are identical in all parts of the world, as has been shown in papers lately read by the Hon. R. Abercromby before the English and Scottish Meteorological Societies.

IVAN POLYAKOFF.

RUSSIA has lost one of her most promising men of science in Ivan Polyakoff, who died lately at St. Petersburg, from hepatic disease, at the age of about forty. He was born in the small village of Transbaikalia, on the Argun, of Cossack parents, descendants from the earlier settlers of Siberia, and received his first education in a military school for sons of soldiers and Cossacks at Irkutsk—a very limited education indeed. As his parents were poor, and life in his native village offered no attractions, he accepted the position of teacher at the same school where he had been educated. Zoology and botany became the sciences of his choice A large park belonging to the Governor, close by the military school, peopled with a variety of birds and insects, became the first field of his researches. As the spring came, he would spend the day in the garden, sometimes extending his excursions to the neighbourhood of Irkutsk, where so much is to be learned. He wrote down his observations, and published them in the *Irkutsk Gazette*. From the very first lines of his description one is struck by a remarkable feature of Polyakoff's mind—a feature which is to be found in all his later writings, and which cannot but be highly appreciated by a true naturalist: it is the simplicity of his conception of the animal world; I should say his intimacy, his familiarity, with every bird or animal he describes. He *understood* them. One must be born in a lonely Siberian village on the confines of the civilized world, at the border of the uninhabited Gobi steppe—the Argun is such a border—to be always in so close a contact with Nature.

Early in 1866 I was going to make a great journey to find out the long-searched-for route from the Lena gold-washings to the steppes of Transbaikalia. A topographer accompanied the Expedition; I undertook the geological exploration; for the botanical and zoological I invited Polyakoff to join us. We crossed the region from the Lena to Tchita, and thus Polyakoff and I were able to make a section of the backbone of the Asiatic continent, with its high and lower plateaus, their border-ridges, and the Alpine regions which fringe them. A zoologist like Polyakoff was thus enabled to obtain an insight into the whole of the Siberian fauna, as dependent upon orographical features. His descriptions of the fauna and flora of the region, especially with regard to the dependence of animals and plants upon their surroundings and their mutual interdependence—he excelled in that kind of research—are a most valuable contribution to the geographical zoology and botany of a wide region. His collection of plants was described by our friend the late Dr. Glehn.

Next year, Polyakoff made another little journey to the upper Irkut valley, from which he returned with something quite new—namely, a rich collection of stone implements. There he studied the actual position of the encampments of our Stone-Age ancestors, and the general surroundings of their life. Afterwards, wherever Polyakoff went—to Olonetz, on the Volga, on the Ural, or to Saghalien—he had only to take a short walk in the region he proposed to explore to have a general idea of it. Then he took a shovel, or invited somebody with a shovel, and indicated the place where some digging ought to be done, and stone implements (Neolithic) never failed to be found. His collections are as numerous as invaluable.

In 1868, he entered the St. Petersburg University—not without some difficulties on account of the Latin examinations—and the late Dean of the University, the much-regretted Prof. Kessler, at once perceived that he would have in Polyakoff a first-rate naturalist, and showed him much attention. Polyakoff's thesis for the degree of Doctor of Sciences—a monograph on the cartilaginous fishes—received high praise, and as soon as he was out of the University, he was appointed Conservator of the Zoological Museum of the Academy of Sciences at St. Petersburg.

After that time Polyakoff was almost always out on some expedition sent either by the Academy or by the Geographical Society. He explored the Olonetz region, the middle Volga, the lower Obi region, and recently he was sent by the Academy of Sciences on a long exploring journey to Saghalien and the Pacific littoral. It was on his return from this last journey that he fell ill at St. Petersburg, where he died in a hospital. A friend who learned of his illness, and went to visit him at the hospital, came too late.

His death is the more a loss for science, as he was going to work out in detail the exceedingly rich zoological and anthropological materials which he had collected during his last journeys. Only preliminary reports of these journeys have been published. Part of his researches on the Stone Age have been embodied in Count Uvaroff's work; others have appeared in the publications of the Academy of Sciences, the Russian Geographical Society, and the St. Petersburg University Society. His preliminary report on the Obi journey (containing an admirable description of the Ostiaks, whom he thoroughly understood) has been translated into German; and there is also a German rendering of his preliminary report, or rather letters, on Saghalien. But most of his observations remain unpublished. It is even doubtful whether his field note-books contain all his observations and generalizations, and whether they were kept in such a state as to render publication possible.

In zoology, Polyakoff's name will remain associated with the description of the *Equus prjevalski*, a separate species established by him, which is the real ancestor of our common horse, discovered by Prjevalski in the Alashan Mountains of Central Asia.

NOTES.

THE dinner given to Prof. Tyndall is going on at Willis's Rooms as we go to press. The hosts number more than two hundred, and many of the most eminent men in the country are present.

ON April 12, 1886, the Local Government Board appointed a Committee to inquire into the efficacy of M. Pasteur's treatment of hydrophobia, and into any dangers which might be connected with its employment. The Committee consisted of Sir James Paget, Dr. Lauder Brunton, Dr. Fleming, Sir Joseph Lister, Dr. Quain, Sir Henry Roscoe, and Prof. Burdon Sanderson, with Prof. Victor Horsley as Secretary. Dr. Lauder Brunton, Sir Henry Roscoe, and Dr. Burdon Sanderson, with the Secretary, visited Paris in order to study M. Pasteur's methods; and after their return Prof. Horsley conducted a series of experiments with a view to the settlement of certain points about which he and his coadjutors had felt some doubt. A copy of the Report of the Committee has been sent to the *Times*, and it appears that the Committee unanimously express confidence in M. Pasteur's system.

THE sixteenth meeting of the French Association for the Advancement of Science will be held at Toulouse from Thursday, September 22, to Thursday, September 29 next. Notice of intention to be present at the meeting should be given to the Secretary of the Association, 4 Rue Antoine-Dubois, Paris, before July 15.

THE *Evening Standard* of Tuesday is our authority for the statement that addresses from the Church of Ireland, the Metropolitan Board of Works, *the Royal Society*, and the Ancient Order of Foresters, were presented to the Queen on Monday last. Let us hope that this is not true.

THE third annual general meeting of the Marine Biological Association took place on Friday last in the rooms of the Linnean Society, Burlington House. Prof. Flower presided, and among those present were Mr. Thiselton Dyer, Mr. Crisp, Prof. Bell, Prof. Charles Stewart, Prof. Ray Lankester, and Sir John Staples. The report for the past year stated that the Council had devoted attention chiefly to the superintendence and fitting of the laboratory at Plymouth, and to preparations for the work of the Association in connexion with that laboratory. It is expected that the laboratory will be ready for partial occupation in the present summer, but the tanks and circulation of sea-water cannot be completed for some months to come. The Council had decided to issue a journal, which might serve not only for the circulation of the official publications of the Association, but also as a means of inquiry and exchange of information among those who are interested in marine biology in its relation to the sea fisheries of the United Kingdom. A first-rate biological library was one of the most important appliances which the Marine Biological Association must possess in its Plymouth laboratory. The Council trusted that the members and friends of the Association would assist in the formation of such a library by gifts of books. The Association was willing and anxious to co-operate with individuals or associations in any part of the British Islands who were engaged in the study of the natural history of marine fishes or in researches in marine biology. The Council had to record with deep regret the death of one of the vice-presidents of the Society, Mr. George Busk. Some formal business having been despatched after the adoption of the report, the meeting concluded with a vote of thanks to the chairman for presiding.

A SPECIAL general meeting of the Fellows of the Royal Horticultural Society was held on Tuesday "to consider the results of the negotiations and inquiries which have been made by the Council as to the future maintenance and housing of the Society." Sir T. Lawrence, who presided, said the Council thought it would be wise as soon as possible to carry on their operations at Chiswick; and this view met with general approval. The meeting adopted a resolution requesting the Council to take such steps for the housing and maintenance of the Society as might appear best calculated to preserve its character and utility and promote the horticultural interests committed to its charge, and insisting upon the importance of immediately taking steps to secure accommodation for the Society at the close of the year, either of a permanent or temporary character, in some central situation in or near the City.

On Jubilee Day the Royal Gardens, Kew, were visited by 31,000 people.

The *Meteorologische Zeitschrift* for June contains the first part of a comprehensive discussion by Dr. W. Köppen, of the Deutsche Seewarte, on the nomenclature of clouds. The author asks whether the same cloud seen from different sides should receive different names; for instance, when seen from the front, sideways, from behind, above, or below; or, whether the classification should refer generally to the properties observed in a particular cloud, especially as regards its density and dimensions. The *apparent* form plays an important part in Poey's classification, but Dr. Köppen shows that it sometimes leads to erroneous conclusions. The classifications of Hildebrandsson, Ley, Weilbach, and others, receive especial notice, but no reference is made in this first article to Mr. Abercromby's recent researches.

The Editorial Committee of the Norwegian North-Atlantic Expedition (1876-78) have published the eighteenth volume of their General Report (Christiania, 1887, pp. 209 and 48 plates). The memoir in question has been edited by Prof. H. Mohn, Director of the Meteorological Institute at Christiania, and deals especially with the depths, temperature, and circulation of the North Ocean, and to some extent with the winds and atmospheric pressure. The region embraced lies between Iceland and Norway, and extends northwards as far as Spitzbergen. The currents naturally receive much attention, and Prof. Mohn states that he has sought to explain the motion of the water as produced alike by the normal winds and by the difference in the density of the water; and he points out that, while the former cause predominates, the latter too has full significance. The maps are very clear, and the explanatory text is written both in Norwegian and English, as in the previous volumes.

The first of the vessels of the Norwegian seal-hunting fleet have returned to Hammerfest from the Arctic regions, and the captains report that the ice-belt this spring extended far south of Spitzbergen. It appears from their reports that when they left the ice-fields no vessels had succeeded in reaching that island. Seals were very plentiful, and nearly all vessels have full cargoes.

The three courses of Burnett Lectures on "Light," delivered by Prof. G. G. Stokes at Aberdeen in 1883, 1884, and 1885, have now been issued in a single volume belonging to the well-known "Nature Series." As we had something to say about each of these courses at the time of its publication in a separate volume, we need only remind our readers that the first course deals with the nature of light, the second with light as a means of investigation, the third with the beneficial effects of light. On this last subject, Prof. Stokes says in the preface to the new volume:—"The benefits derived from light, which form the subject of the third course, are, it might have been supposed, too obvious to require mention. Yet few, perhaps, have been in the habit of contemplating these benefits as a whole, or have perceived how far-reaching and of what vital importance are the advantages that we derive from light, if we include in that term not merely what the eye can perceive, but all that in its physical nature differs from visible light only in the way in which light of one colour differs from that of another colour."

The "Queen's Jubilee Atlas," which we have received from Messrs. George Philip and Son, contains an excellent series of maps, those relating to the British Empire being especially good. Each map is accompanied by a short explanation, with descriptive and historical notes and statistical tables. A physical map of England is given showing the coal-fields and the heights of all the mountains; this is followed by three separate maps of the British Isles showing the railways alone.

Messrs. George Philip and Son have also issued a "Handy-Volume Atlas of the World," containing 110 maps and plans, with complete index, and statistical notes, by Mr. J. F. Williams. The little volume has been carefully prepared, and is the first of a series designed to present all essential geographical information in a handy and accessible form.

Among recent publications in Paris we notice the "Éléments de Médecine suggestive" of MM. Fontan and Ségard, in which the authors give numerous illustrations of the effects of hypnotism in disease, whether mental or physical; a pamphlet, by Dr. Servier, on the Val de Grace, the military hospital in Paris, comprising the history of the buildings and of the institution; and a book by M. Ed. Dreyfus-Brisac, on "L'Éducation nouvelle," a series of studies well worth the attention of those who take some interest in the present evolution of public spirit concerning educational matters in France.

M. G. Pruvot, *maître de conférences* in the Sorbonne, has recently issued a course of lectures on "Worms and Arthropoda." The lectures were delivered by him in 1885-86. The work is profusely illustrated.

The *Archives Slaves de Biologie*, a periodical containing only Slavonic work in biology, are publishing a long series of papers by Danilewsky on the Hæmatozoa of Reptiles and Birds.

A full account of the New Zealand Industrial Exhibition, 1885, is presented in an Official Record, a copy of which has been sent to us. The facts are brought together in a way that will facilitate comparative study of the progress of the colony in the various arts and manufactures, and the volume will be of service to those who may undertake to organize any future exposition of the industrial resources of New Zealand.

An elaborate synopsis of the North American Syrphidæ, by Dr. Samuel W. Williston, has been issued as the thirty-first Bulletin of the United States National Museum. Dr. Williston explains that he has given especial attention to this family since he began his dipterological studies eight years ago, and that he has collected a large part of the species either in New England or in the West. The types of all but two or three of the new species described by him, together with his entire collection in this family, will be preserved in the National Museum for future reference and revision.

The Manchester Microscopical Society has just issued its Transactions and Annual Report for 1886. The volume contains a valuable address on "Fresh-water Animals," by the President, Prof. A. Milnes Marshall, F.R.S., and papers and communications read by the members. A short paper by Mr. Robert Parkes may, perhaps, suggest to some readers a pleasant way of spending a few of the approaching holidays. In this paper Mr. Parkes describes a dredging excursion he made some time ago to Lamlash Bay, Isle of Arran, in company with two friends. The excursion was very successful, and Mr. Parkes was able to exhibit to the Society some of the specimens he had secured. He assured the members that dredging was not a very expensive pursuit, to be followed only by the use of steam launches and a large staff. They could see by the collection before them that good results might be obtained by two or three joining together and dredging from an ordinary rowing-boat.

The fifth volume of the Journal of the Liverpool Astronomical Society has just been issued. It contains many papers, notes, and reviews, and has some good illustrations.

By permission of the President and Council of the Royal Astronomical Society, the annual general meeting of the Liverpool Astronomical Society will be held at Burlington House, London, on Friday, July 8.

THE problem of protection against yellow fever by inoculation seems in a fair way to solution by the Brazilian doctor Freire, who has been seven years at work on the subject. According to a recent account, the number of persons already inoculated is 6524. There died from yellow fever in Rio de Janeiro, between January 1885 and September 1886, 1675 persons, of whom eight had been inoculated (in 1884, the method being then imperfect). This gives a mortality of about 1 per 1000 for the inoculated, and 1 per cent. for the uninoculated. It is remarkable that there has been no epidemic of yellow fever in Rio de Janeiro this year (a thing not known for the last thirty-five years). The microbe of yellow fever is called *Cryptococcus xanthogenicus*. Dr. Freire gets a culture liquid for inoculation, on the principles of M. Pasteur's methods, and he injects about one gramme of it subcutaneously.

EXPERIMENTS have been recently made by S. Leone (*Gazetta Chimica Italiana*) as to how organic substances in water are affected by development of bacteria. He used distilled water, to which a little gelatine was added. The organic nitrogen and carbon are changed by the organisms into inorganic compounds, chiefly carbonic acid, ammonia, nitrites, and nitrates. It appears that up to the fifteenth or sixteenth day the ammonia steadily increased, then it decreased till it was quite gone. Meanwhile, nitrous acid appeared; it increased as the ammonia disappeared, and when this was gone, a formation of nitric acid began, at the cost of the nitrous acid, so that in thirty-five days the latter too was quite gone, and only nitric acid present. If a little gelatine was put in the water which had turned ammonia into nitrates, the reverse process began; ammonia was formed again, and even directly added nitrate was changed into this. If no fresh gelatine was added, however, nitrites and nitrates were again produced. The author ascertained that the same organisms that in *presence* of organic substances formed ammonia, in *absence* of such effected nitrification.

PROFS. KRUSS AND NILSON, of Stockholm, have succeeded in preparing a double fluoride of potassium and the new element germanium, K_2GeF_6, isomorphous with the corresponding double fluoride of ammonium and silicon, thus proving most conclusively that this recently discovered element belongs to the silicon, titanium, zirconium, tin, and lead group of the periodic classification. The fluoride of germanium, GeF_4, which is not gaseous, but resembles zirconium fluoride, ZrF_4, was first prepared by dissolving the oxide, GeO_2, in hydrofluoric acid; and the double fluoride separated in the gelatinous form on adding the calculated quantity of potassium fluoride. On filtering, however, the salt dried to a crystalline powder resembling potassium silicofluoride, but being more soluble than the latter, separated from solution in hot water in beautiful tabular crystals, and from a solution in cold water on evaporation over sulphuric acid in pyramid-capped prisms several millimetres long. Once more the value of Newlands' and Mendelejeff's generalization as an incentive to research is demonstrated, and confidence in its truth inspired, for Mendelejeff himself predicted that "ekasilicon will yield a double fluoride isomorphous with the double fluorides of silicon, titanium, zirconium, and tin, of greater solubility than that of silicon; and the fluoride, like the fluorides of titanium, zirconium, and tin, will not be gaseous."

A NEW synthesis of uric acid, directly proving its constitution, has been effected by Prof. Horbaczewski (*Monatshefte für Chemie*, May 28, 1887). The reaction is very simple and consists in fusing together 1 part of trichlorlactamide, $CCl_3—CHOH—CO—NH_2$, with 10 parts of urea, $CO(NH_2)_2$, when 15 per cent. of uric acid together with ammonium chloride, hydrochloric acid, water, and a few decomposition products are obtained. By a long process of separation and purification the uric acid was obtained quite pure, in crystals exactly resembling those obtained from natural sources. This method of synthesis points to the extreme probability that the constitution assigned by Medicus to uric acid is correct, and shows that it is the di-ureide of acrylic acid. Probably no work has been watched with keener interest than the attempts which have been from time to time made to solve the problem of the constitution of this complex molecule, and it is a matter of great satisfaction to have our knowledge of a substance so widely occurring in animal secretions, and parent of so many derivatives, founded upon a method of synthesis so direct.

SUPERFICIAL tension in liquids being, like the magnetic state, an essentially molecular phenomenon, we might expect that it and phenomena depending on it would be modified by action of an intense magnetic field. Prof. Dufour lately proved such an effect by making mercury flow through a horizontal capillary tube placed between the poles of a strong electro-magnet. The liquid describes a parabola, the vein being continuous to a certain distance from the orifice, when it separates into drops. While the magnet acts the parabola is stretched, and the continuous part of the vein lengthens, indicating more rapid flow.

ATTENTION has been lately called by Herren Kerner and Wettstein, in the Vienna Academy, to two carnivorous plants found in Germany. One of these is the leadwort root (*Lathræa squamaria*) which has no chlorophyll, and passes for a parasite, as it fixes, with small nipples, on the roots of fruit-trees. The pale stems, appearing in shady moist places in spring, are covered thickly with scale-like leaves, each of which has its upper half rolled back on the back of the lower, leaving a hollow space between. Into this open by small holes from five to thirteen separate chambers, having on their surface numerous tufted hairs and hemispherical horns connected with the vascular bundles. Various small animals get into these chambers, and ere long disappear. From both hairs and horns threads of plasma stream out, when the animals come into contact with them, and lay hold of them. Though it is not exactly proved that the plant benefits by the animals it thus catches, this seems very likely from its general character. It is more remarkable that a plant containing chlorophyll, and existing independently, like *Bartsia alpina*, should have similar organs for capture of animals, and should feed on such, as the authors assert. The plant forms in autumn underground buds covered with scales, whose lateral borders are rolled outwards, making a hollow in which are organs quite similar to those in the leadwort root.

THE habits of the rainbow trout (*Salmo irideus*) in their fry stage are in some respects very different from those of other species of Salmonidæ. At the present time many thousands of them may be seen in the ponds belonging to the National Fish-Culture Association at Delaford Park, where they were hatched out in the spring from ova sent from California. Instead of moving about in groups or shoals, they isolate themselves from one another, and are to be found in every part of the pond instead of at certain spots or on shallows. Again, the rainbow trout fry are visible within half a foot of the surface of the water, while other varieties hide from view. They appear to be exceedingly voracious, and this may account for their capacity for rapid growth, which exceeds that of their congeners.

IN one of the Selborne Society Letters, issued the other day, the Rev. S. A. Preston, the founder and for many years the President of the Marlborough College Natural History Society, sets forth his ideas as to the best method of promoting the study of natural history in schools. He thinks (1) that each boy should have the elements of two or three branches of natural history put before him,—as a general rule, nothing more than the elements would be required; (2) that this should be done so as to make it as interesting to him as possible, so that he may look forward to his natural history lesson; (3) that if he "takes" to any particular subject, means should be at hand to enable him to go on with it; (4) that he should be encouraged to work out of school. Mr. Preston is of opinion that the appointment of "science masters" is not necessary for the attainment of these ends. "Among any body of masters now," he says, "there are sure to be some who are fond of some branch of natural history, and who can teach the elements of their subject (as far as is necessary for boys in general), and do it in a pleasing and interesting manner. At the end of a long afternoon's work at regular school subjects, the master should occupy the last half-hour or so (if the other lessons have been well said) with a discussion upon his special branch, showing specimens, encouraging questions, and making this part of his work as different as possible from the ordinary work. Boys will look forward to this time, and will work all the harder at their other work to get this 'talk,' if a good lesson is required before the natural history one. By the end of a term, with a little system, the elements of the subject may easily be learnt. The next term, masters should change forms for this half-hour, and the boys thus have some new subject put before them. In a few terms, therefore, a very fair general knowledge of natural history may be secured. If a boy showed any aptitude in any one branch, there would be a master at hand ready to help him and get him on."

MR. C. S. WILKINSON, the New South Wales Government Geologist, reporting upon the seams of coal pierced in the diamond-drill bore at Holt-Sutherland, near Sydney, says that in this bore a depth of 2307 feet from the surface, or 2175 feet below sea-level, has been attained. This is the deepest diamond-drill bore in Australia. The diameter of the bore to a depth of 500 feet is $3\frac{1}{2}$ inches, and below that depth it is 3 inches. The strata passed through consist of Hawkesbury sandstones, 653 feet 6 inches; shales, sandstone, and conglomerates (the upper 314 feet consisting chiefly of chocolate-coloured shales), 1573 feet 3 inches; upper seam of coal, 4 feet 2 inches; shales, sandstone, and conglomerate, 65 feet; lower seam of coal, 5 feet 3 inches; black shaly sandstone, 5 feet 11 inches.

A HEAVY snowstorm is reported to have occurred on the Scheekoppe on June 11. On the Kapellenberg, between Hirschberg and Schönau, it snowed severely, and in the night the thermometer sank to 3°.

A TELEGRAM from Omsk to St. Petersburg of the 21st inst. states that there were several slight oscillations of the ground at Vernoé on that day. To the west of Karakoul the earthquake had been more violent than at the latter place; a lake in the neighbourhood had sunk 3 feet. Almost all the Government buildings at Vernoé are said to be destroyed.

Two beaver colonies have just been discovered at Amlid, near Christiansand, Norway. On the bank of a river the beavers have made lodges of branches of trees, which are held together with clayey mud, the whole resting on logs of wood. The entrance, a hole, faces the river, but is below the surface of the water. Round the entrance there are numbers of aspen and birch trees, the bark of which has served as food for the animals. The beaver gnaws the tree about 2 feet from the root, and if it finds the bark to its taste, cuts the tree up in pieces from 2 to 3 feet in length, which the animal then drags or carries down to its house—proceedings which are fully demonstrated by the many "log-runs" in the woods along the river bank. Observers have also noticed another remarkable habit of this interesting animal, viz. that on arriving by the water-side with such a log of wood it will poise the piece on the back of its neck and swim with it right into the lodge, where the bark is gnawed off and stored away for winter use. This accomplished, it will shoot the log into the river. The largest trees the animals have dealt with in this manner are 11 inches in diameter. The colonies are situated far from human dwellings, where people only come in winter, during the timber-felling season.

AT the Ladies' *Soirée* at the Royal Society on June 8, much attention was attracted by the fine exhibit sent from the Royal Gardens, Kew. Great credit is due to the officials at Kew for the care with which the objects were selected and displayed. The following is a list of the flowering plants:—*Myrmecodia Beccari*, *Myrmecodia* sp. New Guinea, *Leea amabilis*, *Impatiens Hawkeri*, *Primula Reidii* and *cortusoides*, *Piper porphyrophyllum*, *Streptocarpus Dunnii* and *polyanthus*, *Coffea liberica*, *Tillandsia splendens* and *usneoides*, *Caraguata Zahnii*, *Cypripedium Stoneii*, *Dendrobium Dalhousieanum* and *transparens*, *Epidendrum vitellinum*, *Odontoglossum Hallii*, *Miltonia vexillaria*, *Sarracenia Patersoni*, *Palumbina candida*, *Areca monostachya*, *Licuala grandis*, *Verschaffeltia splendida*, *Caryota Blancoi*, *Cycas undulata*, *Hemitelia Smithii*, *Adiantum amabile*, *Acrostichum crinitum*, *Brainea insignis*, *Saccolabium curvifolium*. There were cut flowers of *Hæmanthus magnificus*, *Randia Stanleyana*, *Hexacentris mysorensis*, *Senecio macroglossa*, *Iris Susiana*, *Chamædorea elegantissima*, *Bougainvillea spectabilis*, *Napoleona imperialis*, *Cochliostema Jacobianum*, *Pandanus odoratissimus* (cone), *Musa coccinea*.

THE additions to the Zoological Society's Gardens during the past week include a Moustache Monkey (*Cercopithecus cephus* ♀) from West Africa, presented by Mr. Bernard Lawson; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. G. Choutte; two Lions (*Felis leo* ♂ ♀) from Kittywar, Guzerat, India, presented by Major J. Humphrey; two Striped Hyænas (*Hyæna striata*) from India, presented by the Bombay Natural History Society; a Suricate (*Suricata tetradactyla*) from South Africa, presented by Mrs. H. A. Warwood; an Australian Crane (*Grus australasiana*) from Australia, presented by Mrs. M. S. Richman; a Ring-necked Parrakeet (*Palæornis torquatus*) from India, presented by Mrs. Crabtree; two Edible Frogs (*Rana esculenta*), European, presented by Mr. H. A. Crossfield; three Green Turtle (*Chelone viridis*) from Ascension, presented by Capt. C. Theobald, R.N.; a European Pond Tortoise (*Emys europæa*) from Venice, presented by Mr. Alban Doran; an Alligator (*Alligator mississippiensis*) from Florida, presented by Mr. Hugh Bellas; a Green Monkey (*Cercopithecus callitrichus* ♂) from West Africa, five Common Dormice (*Muscardinus avellanarius*) British, deposited; a Little Egret (*Ardea garzetta*), a Buff-backed Egret (*Ardea russata*), European, a Horrid Rattlesnake (*Crotalus horridus*) from Brazil, purchased; a Yak (*Poëphagus grunniens*), born in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 JULY 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 July 3.

Sun rises, 3h. 50m.; souths, 12h. 3m. 53·0s.; sets, 20h. 17m.; decl. on meridian, 22° 59′ N.: Sidereal Time at Sunset, 15h. 3m.

Moon (Full on July 5) rises, 18h. 22m.; souths, 22h. 48m.; sets, 3h. 11m.*; decl. on meridian, 19° 4′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	6 11	13 52	21 33	18 12 N.
Venus	7 55	15 11	22 27	13 54 N.
Mars	2 30	10 49	19 8	23 46 N.
Jupiter	13 35	18 53	0 11*	8 56 S.
Saturn	4 57	12 59	21 1	21 25 N.

* Indicates that the setting is that of the following morning.

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.	° °
6	π Capricorni	5	21 3	near approach	149 —
6	B.A.C. 7053	5½	21 49	22 53	37 280
6	o Capricorni	5½	21 49	22 54	38 280
9	42 Aquarii	6	0 48	1 45	43 320

Variable Stars.

Star.	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei	0 52·3	81 16 N.	July	7, 23 13 *m*
δ Libræ	14 54·9	8 4 S.	,,	9, 0 8 *m*
U Coronæ	15 13·6	32 4 N.	,,	9, 1 29 *m*
U Ophiuchi	17 10·8	1 20 N.	,,	6, 0 12 *m*
S Sagittarii	19 12·8	19 14 S.	,,	9, *M*
T Capricorni	21 15·8	15 38 S.	,,	9, *M*
δ Cephei	22 25·0	57 50 N.	,,	8, 1 0 *m*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

AT Monday's meeting of the Royal Geographical Society, Mr. J. T. Last gave a brief preliminary account of his recent explorations among the Namulli Hills, to the south-east of Lake Nyassa and along the River Rovuma. He found that, although the thermometer often falls below freezing-point, no snow exists on the Namulli Hills. At the same meeting, General Haig read an unusually interesting paper on a recent journey he made in the south-west corner of Arabia. He started from Hodeida, went inland to Sana'a, and south to Aden. He found himself in a region of mountains rising to over 10,000 feet, in many places terraced by the natives up to a height of 8000 feet. The scenery was often of the most magnificent and picturesque description, and the climate so comparatively temperate as to be suited for European settlement. The whole region of which this forms part, and indeed the entire southern portion of Arabia, including Hadramaut and Omân, is one that would richly repay serious exploration. General Haig made a journey of about fifty miles into the interior of Omân, and found that, while there was a rainfall of only 6 inches on the coast, at least 30 inches fell upon the hills of the interior.

SOME further steps have been taken in Australia for the prosecution of Antarctic exploration. The Antarctic Committee appointed by the Royal Society of Victoria and the Royal Geographical Society of Australia have memorialized the Premier of Victoria on the propriety of stimulating Antarctic research by the offer of bonuses. They recommend that a sum of £10,000 be placed on the Estimates for this purpose, and that tenders be solicited from shipowners for the performance of services in connexion with Antarctic exploration. It is stipulated that shipowners whose tenders are accepted shall provide, free of charge, cabin accommodation in each ship for two gentlemen, who will sail as the scientific staff; and a second cabin as instrument-room and office. The master of the ship must afford these gentlemen every facility for observing natural phenomena. The master will receive special bonuses for every hundred tons of oil from fish caught south of 60° S. The special services desired are as follows:—A flying survey of any coast-lines lying within the Antarctic Circle, and not laid down upon the Admiralty charts; the discovery of new waterways leading towards the South Pole, and of harbours suitable for wintering in. Opportunities must be afforded to the scientific staff to add to our knowledge of the meteorology, oceanography, terrestrial magnetism, natural history, and geology of the region. Special bonuses will be given for passing 70° S., and also for establishing on shore a temporary observing camp. Two ships are wanted, and both must be in Port Philip Bay and ready to start on October 15. The Premier of Victoria, we are glad to say, has promised to place £10,000 on the next Estimates for these purposes, on condition that the other colonies will join in the enterprise; this they no doubt will do.

THE Russian Government has decided to establish Chairs of Geography in the Universities of the empire. The first appointment will be to the University of St. Petersburg in the autumn of the present year.

MR. MCCARTHY, the Government Surveyor of Siam, has just returned to this country, with a very fine set of maps of that country, embodying the results of seven years' survey work. These he is working out at the Royal Geographical Society.

MR. W. J. STEAINS has just returned from Central Brazil, where he has spent a considerable time among the Botocudos, a savage people, concerning whom our information is exceedingly scanty. Mr. Steain's has collected much information concerning these people, and brought home some two hundred sketches, which he will probably publish soon in some form.

ONE of the public lectures at the Manchester Meeting of the British Association will be by Sir Francis De Winton, late Governor of the Congo Free State. Sir Francis, we believe, will illustrate his lecture with a series of maps (perhaps thrown on the screen) showing the progress of our knowledge of Central Africa from the time of Ptolemy down to the present day.

DISCOVERY OF FOSSIL REMAINS OF AN ARCTIC FLORA IN CENTRAL SWEDEN.

FOR the first time fossil remains of an Arctic flora have been discovered in the great stretch of land between Scania and Norrland. The discovery was made in a part where it was least expected, viz. just north of the town of Vadstena, close to the shore of the lake Wettern. The soil in the vicinity of Vadstena greatly resembles that of South-Western Scania, being mostly formed of moraine clay or clayey moraine sand, whilst marine formations appear to be absent in the former place; they are, however, found further to the north-east, but I have as yet been unable to ascertain the limits of the two districts. Within the moraine clay are found here and there little cavities or depressions, occupied by peat bogs or alluvial formations. Close to the shore of the lake Wettern, barely a third of a kilometre north-east of Vadstena, such a depression occurs, occupied by a peat bog. This peat bog continues to the north-east beyond the depression, a little way up the rising ground, caused by the existence here of some strong wells, around which in remote times considerable quantities of calcareous tufa have formed. My attention was drawn to this locality by Dr. J. Jönsson, who had noticed the tufa under some work effected for the Geological Survey of Sweden, but not having closely examined the fossil remains of plants in the same, he was only able to inform me that he had found mosses therein.

On examining the collection of specimens of the tufa obtained, I found at the back of one some well-preserved leaves of *Dryas octopetala*, L., other fossil remains in the same fragment, besides mosses, being branches of *Empetrum* and leaves of *Vaccinium uliginosum*, L. In consequence of this discovery, I decided to visit the spot myself, partly in the hope of discovering some more specimens of *Dryas*, and partly in order to study the adjacent layers of earth and the strata containing the fossil plants. But although I spent a whole day in examining loose blocks and the accessible parts of the strata I did not succeed in finding any more leaves of *Dryas*.

The calcareous tufa is, as I have stated, deposited on a declivity and around a well, and the latter, whose flow is rather strong, is now exposed through the removal of the peat (a couple of feet in thickness) which covered it, along with the tufa immediately round the well. The latter appears to have rested immediately on clayey moraine *débris* or moraine clay (bottom moraine), whilst nearest the well the lower layers are sinter-formed without distinct remains of plants, though probably containing such pine needles and mosses as are found in the upper layers. The mosses are in the upper part of the tufa in certain places common, and form sometimes separate layers consisting solely of such. The composition of the bed seemed to be as follows:—Lowest, the lime had formed round growing grass or Juncaceæ, the leaves of which are indicated by more or less perpendicular holes. Next above this appears a more distinctly stratified tufa, containing leaves and exterior bark of the pine, but, judging from the fragments thrown up in the vicinity, the layer containing *Betula nana* should be placed between these two. As a proof of such a layer are the mosses, leaves of *Vaccinium uliginosum*, *Empetrum*, and even needles of pine, although more seldom than in the true pine layer. From the layer containing remains of dwarf-birch the piece of tufa with the *Dryas* leaves was undoubtedly obtained. Some samples of this tufa show a relatively rapid precipitation, the needles and pieces of bark themselves being sometimes found intact on the cleaving of the tufa. In this layer are also found remains of a species of broad-bladed grass, and Herr Carlson has further found in it the imprint of a feather. Uppermost, at all events in certain spots, mosses only are found. The calcareous tufa, the greatest thickness of which is hardly more than 3 feet, is in turn covered with peat.

In the tufa are sometimes found, in layers, thin bands of remains of plants, chiefly of grasses and mosses, the vegetable substance of which is still preserved. In such a layer even a leaf of *Betula nana* was found. Besides the above-mentioned remains of plants may be mentioned leaves of at least three different kinds of Salices, one reminding of *S. cinerea*, one of *S. repens*, and one which most certainly cannot be referred to any of the varieties now found in Southern Sweden. In addition to those of *Betula nana*, imperfect leaves of a large birch-tree, probably *B. odorata*, have been found, and also a perfect leaf of one apparently corresponding with *B. intermedia*, although there is some probability that it may be a smaller leaf of *B. odorata*, it being generally impossible to define leaves varying so much as those of *Betula* and *Salix* from a single imprint.

Of the species just named, two at all events, viz. *Betula nana* and *Dryas octopetala*, are extinct near Vadstena. It is also probable that one of the varieties of *Salix* is now foreign to these parts. *Betula nana*, however, is still found in certain parts of Östergötland, but the nearest spot in which *Dryas* grows is in the mountains around the valley Herjedalen, about 4° to the north-west of the lake Wettern, and we may safely assume that the presence of these two plants in the same locality clearly indicates that at the time of the deposition of the older layers of calcareous tufa the climate was much colder than that now prevailing there. For my own part, I am even disposed to consider this discovery of fossil *Dryas* near Wettern as a proof of a purely Arctic flora having prevailed in these parts at an age older than that represented by the calcareous tufa; studies of the same in the province of Jemtland in my opinion indicating that a true Arctic climate is not favourable to the development of calcareous tufa, this mineral being first deposited after the climate has become milder. In Jemtland we certainly find Arctic plants in the tufa, but generally together with remains of pine, and they must therefore be considered as the last remnants of an Arctic flora, which already then was in course of being supplanted by the pine and accompanying species. Its greatest significance lies, not only in the proof of an Arctic flora once having flourished in these parts, but also in the circumstance that it proves that an Arctic flora could exist at such a low elevation.

It will further appear from the above exposition that the Arctic flora in this locality was followed by a pine vegetation, the process thus entirely corresponding with what took place in Scania and Norrland. Information from other localities in these parts is, however, required before we can arrive at general conclusions.

Finally, it may not be out of place here briefly to refer to a question which to some extent may be considered to be affected by this discovery. In a paper read in 1860 before the Academy by Prof. Sven Lovén, "On some Crustacea found in the lakes Wettern and Wenern," the author pointed out that, as regards Wettern, this lake sheltered a fauna belonging to deeper waters, of originally marine and at the same time Arctic character. This fauna Prof. Lovén considered to be a relic from the time when Wettern, by way of the Baltic and Lake Ladoga, was connected with the Arctic Ocean. He said:—"Some few favoured species, those which in a higher degree than others were able to adapt themselves to the new medium, and which already in their former habitat, the less saline Arctic Ocean, had accustomed themselves to live for instance where melting glaciers diluted the sea-water, or at the mouth of rivers, would in one or another of the great lakes thrive longer than others, and finally be the only ones surviving. Such a lake is Wettern." It should be pointed out that the discovery of the fossil *Dryas* leaves on the shore of the lake Wettern is of considerable significance in view of the opinion thus expressed by Prof. Lovén. For the calcareous tufa referred to here having been deposited *since* the sea had already receded from these parts, and this tufa nevertheless containing Arctic plants, we may conclude that the lake Wettern became separated from the sea whilst the climate was still Arctic.

A. G. NATHORST.

GEOLOGICAL STRUCTURE OF FINISTÈRE.

THIS article is founded upon the "Aperçu sur la constitution géologique du Finistère," prepared for a recent excursion of the Geological Society of France by Dr. Charles Barrois, of Lille.

Since the three great promontories of South Wales, Devon and Cornwall, and Brittany, are sharers in no small degree of a common geological history, English geologists can hardly fail to take an interest in the structure of the western extremity of Brittany. Dr. Barrois is very well known to many of us, and the fact that for some time past he has been engaged on the geological survey of Brittany renders his observations all the more valuable. From time to time he has furnished the annals of the Société Geologique du Nord with some of the results of his observations in that country. Of these we may mention "Le granite de Rostrenan (Côtes-du-Nord), ses apophyses et ses contacts," "Mémoire sur les schists métamorphiques de l'île de Groix (Morbihan)," "Mémoire sur les grès métamorphiques du massif granitique du Guéméné (Morbihan)," "Note sur la structure stratigraphique des Montagnes de Menez (Côtes-du-Nord)," and "Légende de la feuille de Châteaulin (Finistère)."

The department of Finistère is traversed from east to west by two parallel chains—on the south the Black Mountains, on the north the Mountains of Arrée. Between the first-named chain and the Atlantic lies the southern plateau of Brittany, whilst the northern plateau is situated between the Mountains of Arrée and the English Channel. Both plateaux are formed by Archæan (*primitifs*) and Cambrian rocks more or less injected by granite. The basin included between the two ranges presents a series of beds extending from the Silurian to the Carboniferous in parallel folds, and is evidently one of the most important physical features in the north-west of France.

The stratified rocks of the region present the following succession:—

Carboniferous.

Schists and Conglomerates of the Coal-Measures.
Schists and Sandstones of Châteaulin.
Porphyritic Tuffs.
Conglomerates and Porphyritic Tuffs.

Devonian.

Nodular Schists of Porsgruen.
Schists and Limestones of Néhou.
White Grit of Landévennec.
Schists and Quartzites of Plougastel.
Limestone of Rosan with *S. looiensis.*

Silurian.

Nodular Schists with *C. interrupta.*
Bituminous Schists with Graptolites.
White Sandstones.
Slates of Angers.
"Grès Armoricain."
Conglomerates and Red Schists of La Chèvre.

Cambrian.

Schists and Conglomerates of Gourin.
"Phyllades" of Douarnenez.

Archæan.

Schists of Groix.
Mica Schists of Audierne.
Granitic Gneisses of Pont-Scorff.

Archæan.—The most ancient group of rocks in Finistère consists of certain granitic gneisses and mica schists. The gneisses are devoid of white mica, consisting mainly of white and rose feldspar in large grains, with abundance of black mica, in foliations, sometimes replaced by hornblende in fragments, with granitoid and secondary quartz. These gneisses alternate with interstratified beds of mica schists and amphibolites, and pass into gneissic granites which penetrate them after the manner of an eruptive rock.

The injection of this *gneissic granite* may be explained in three different ways: (1) either it is contemporary with the gneiss and the mica schists, or (2) it may date from a later epoch, or (3) lastly, it may proceed directly from the gneisses by means of local recrystallisations under the action of a powerful general metamorphism. If we accept the first of these hypotheses, says Dr. Barrois, the conditions remind us somewhat of the *Dimetian* proposed by Dr. Hicks for Wales. It is evident, however, that he is more disposed to favour either of the other hypotheses.

Certain mica schists are largely developed in the southern plateau; they alternate with subordinate beds of fine-grained gneiss, amphibolites, chlorite schists, micaceous schists, and interstratified masses of diorites and "gneissites" of eruptive origin. These accessory rocks form, together with the mica schists into which they are injected, long parallel bands from one end to the other of the southern plateau. The "gneissites" include a complete assemblage of acid rocks, remarkable for their gneissic, ribboned, and glandular structure, rich in white mica, and in secondary feldspar, with "granulitic" quartz in elongated grains, in rounded drops, and in thin flakes. These include the rothe-gneiss, augen-gneiss, flaser-gneiss, stengel-gneiss, hälleflintas, and leptynites of the German geologists; as also the rhyolitic felsites, volcanic breccias, hälleflintas, and felsitic tuffs of the English geologists.

Is the injection of the "gneissites" contemporary with the mica schists, or should it be referred to a subsequent epoch? If we accept the first of these hypotheses, the stage so termed may be said to ally itself by its lithological characters to the *Arvonian* of Dr. Hicks. It would seem, however, that in making the (geological) sheets of Lorient and Chateaulin, Dr. Barrois and his coadjutors have adopted another view, by referring the characters of the "gneissites" or "granulites feuilletées" to their consolidation in special "encaissements" under suitable mechanical conditions of depth and pressure, and at an epoch different, but as yet undetermined, from that of the massive granulite of Pontivy.

The schists of Groix constitute a stage of micaceous schists, of chloritic and chloritoid schists, of carbonaceous schists, and of mica schists, especially remarkable for the abundance of the heavy minerals which they contain (staurolite, garnet, magnetite, &c.). The carbonaceous or graphitic schists sometimes referred to the Cambrian would appear to form the base of this stage. The boundary between it and the Cambrian is admitted to be obscure. If this stage, says Dr. Barrois, corresponds to the *Pebidian* of Wales, it is distinguished by its poverty in interstratified basic rocks, which are always of limited extent in Brittany.

Cambrian.—How far these greenish-gray satiny schists, with their beds of quartzite and veins of quartz, correspond to any British Cambrian beds, the author admits is uncertain. Moreover, we would observe that there is no mention here of any bed of conglomerate at the base of the Cambrian. Hence the evidence as to the antiquity of the presumably pre-Cambrian rocks fails in this important particular. The author estimates this stage (Phyllades of Douarnenez) at over 3000 metres. Above these are beds of schist and conglomerate in regular interstratification. The conglomerates are formed of little pebbles of quartz with about 1 per cent. of other stones. They are distinguished from the Silurian conglomerates by the smallness of their component parts, and by their inferior hardness. The equivalents of these beds in the north of the department are fossiliferous, and correspond to the *Paradoxides* beds of la Vega in the Asturias,[1] and to stage C of Barrande. Here then we have our first palæontological horizon in Brittany which would seem to be Menevian.

Silurian.—The lowest sta[ge] thus classified consists of red schists, variegated quartzites, and beds of quartzose conglomerate. This is succeeded by the famous "Grès Armoricain," which forms the most salient feature of Menez-Hom and the Black Mountains. It is characterised by *Scolithes*, *Bilobites*, *Lingulæ*, &c., and is the most constant of the fossiliferous beds of Finistère. Barrande's stage D is represented by the slates of Angers with *Calymene tristani*, &c. The three horizons of the third Silurian fauna are with difficulty traced on the north of the Black Mountains.

Devonian.—From a geognostic point of view the schists and quartzites of Plougastel, over 1000 metres in thickness, constitute the most important stage of this system, being largely developed in the roadstead of Brest and forming the northern crest of the Black Mountains. *Homalonotus* sp., *Rhynchonella puilloni*, and *Grammysia davidsoni*, are amongst the few fossils. Above these come beds recalling the Taunusian, Coblenzian, and Eifelian, for the most part fairly fossiliferous. As no higher ones are mentioned, we may presume that the Middle and Upper Devonian are absent.

[1] See *Geological Magazine*, 1883, p. 274.

Carboniferous.—The physical history of Brittany during this period was one of oscillation between terrestrial and marine conditions; it was a period of extensive eruptions and of great earth-movements. Hence a considerable portion of the sediments, especially towards the base, are of volcanic origin. The mass of the formation is comprised in what Dr. Barrois calls the "schists of Châteaulin," an alternation of schists, slates, and sandstones with *Spirifer striatus*, *Strophomena rhomboidalis*, *Phillipsia derbyensis*, and *Productus semireticulatus*: they also contain poor impressions of plants. In some respects this description reminds us of the Culm of Devonshire. This group rests unconformably on the various Devonian beds. The actual Coal-Measures form three small and distinct basins in Finistère of little economic value.

It is interesting to note that the volcanic phenomena in this region are referred to the Carboniferous rather than to the Devonian epoch, and this serves to recall the controversy as to the precise geological age of the rocks in the Brent Tor district —a doubt which is applicable to a large area of Palæozoic rocks lying to the north-west of Dartmoor. Since, in Brittany, the Carboniferous rocks are unconformable to the Devonian, whilst the intermediate deposits consist in many places of porphyritic tuffs, it is evident that the chief deposit of ashes and other volcanic material represent formations intermediate in respect of time. Why may they not in part be Middle and Upper Devonian? To the Carboniferous period also are referred the porphyroid granites of Rostrenan and other places, and the numerous veins of quartz porphyry, which are so apt to follow the synclinal folds of the sedimentary rocks, the prevailing direction being a little to the north of east. The eruptions, according to Dr. Barrois, must have commenced after the Devonian, and continued during the whole of the Lower Carboniferous. The most important development of quartz diorite, which follows the southern foot of the Black Mountains, he regards as posterior to the Devonian and anterior to the Carboniferous.

Lastly, Dr. Barrois speculates on the earth-movements that have helped to fashion the country of Finistère, which may be said to possess a radiate structure in consequence of the numerous flexures undergone by the rocks; the general orientation is east to west, but with a tendency to converge towards west. These directions correspond to axes of a complete series of synclinals and anticlinals. The eruptive rocks of the region have been affected at the same time as the sedimentary rocks, whose foldings they have followed; they made their appearance chiefly at two epochs, during the Archæan (*terrain primitif*) and during the Carboniferous, thus affording two periods of maximum eruptive force. The principal periods of flexing appear to have been five in number, and correspond in the main to the breaks in the great systems already detailed. The fifth and greatest flexure took place after the Upper Coal-Measures: it has left its mark on all the formations, and since that period Finistère has been in a condition of *terra firma*. W. H. H.

TEMPERATURE IN RELATION TO FISH.

THE influence of temperature exerts itself to such a marked degree upon the habits, food, reproduction, and migration of fish, that observations upon the subject are essential in determining the relations of certain forms to their surroundings. The National Fish-Culture Association have for some time past made investigations into the temperature of the ocean, not only at the surface, but also at the bottom, and the Council will shortly publish the results. In order to ascertain its effect upon fish maintained under artificial conditions, Mr. W. August Carter, of that body, has compiled the following statistics, showing the influence of temperature upon fish at the late South Kensington Aquarium, where the average depth of the tanks was 4½ feet. The statistics are derived from observations made daily during a period of three years by noting the temperature of the water in the tanks, and the death-rate prevalent at certain seasons of the year. By observing the degrees of temperature at which certain fish succumbed from time to time, Mr. Carter has drawn an average, showing the temperature adapted to various fish, and their capacity, in some instances, for withstanding extremes of heat and cold.

It must be borne in mind that the temperatures recorded are applicable only to fish in confinement, and living therefore under unnatural conditions. The temperature registered on the death of the fish named exceeded the highest and lowest degrees given below, which are, as already stated, intended to indicate the temperature of water in which they can be maintained in aquaria.

Marine Fish.

Species.	Temp. ° Fahr. Highest	Temp. ° Fahr. Lowest	Remarks.
Gurnard	62	49	Highly sensitive
Wrasse	55	50	,, ,,
Dogfish	71	45	Occasionally exist when in 38°
Mullet	70	35	Very hardy
Eels (Conger) ...	70	40	Occasionally at 30°
Bullhead	62	49	Thrives best at 55°
Skate...	70	45	
Sole	62	51	Thrives best at 56°
Flounder	70	35	
Plaice	70	35	
Bream	65	45	Thrives best at 58°
Bass	70	35	
Cod	70	35	Thrives best at 55°
Crayfish	60	45	Cannot exist in extremes
Blennie	58	43	
Fresh-water Fish.			
Trout...	71	34	
Perch...	65	43	
Dace	60	44	Occasionally at 32°
Tench (Common)	65	45	
,, (Golden)..	68	45	
Roach	60	50	
Catfish	70	43	Occasionally at 38°
Eels	70	35	
Carp	70	35	
Gudgeon	55	43	
Pike	70	36	
Minnow	55	46	
Chub...	50	40	

It will thus be seen that the dogfish, mullet, conger, skate, flounder, bass, cod, trout, catfish, pike, and carp are extremely hardy, and can exist in both a high and low temperature, ranging from 34° to 71°. On the other hand, the gurnard, wrasse, bullhead, sole, bream, crayfish, blennie, perch, dace, tench, minnow, chub, roach, and gudgeon show themselves sensitive to extremes of temperature.

SOCIETIES AND ACADEMIES.

London.

Royal Society, June 16.—"On the Tubercular Swellings on the Roots of *Vicia Faba*." By H. Marshall Ward, Fellow of Christ's College, Cambridge, Professor of Botany in the Forestry School, Royal Indian College, Cooper's Hill.

In this paper the author gives a detailed account of his investigations, of which the following is a short abstract.

The curious tubercle-like swellings on the roots of *Vicia* and other Leguminosæ have long been a puzzle to botanists and agriculturists. They have even been described as normal structures by some observers. The general opinion, however, has been that they are not so. Erikssen and Woronin at one time thought they contained Bacteria; Kny and others ascribed them to a Myxomycete; Frank and others had also observed certain extremely minute hyphæ in their tissues; but no one had been able to discover the connexion between the tubercles and a fungus.

By special methods of culture and observations extending over some time, Prof. Marshall Ward has discovered that the tubercles of *Vicia Faba* contain a fungus of a very definite kind, and he exhibited preparations showing the structure of the tubercles and fungus, and the entrance of the infecting hypha into the root-hairs of the plant: this infecting hypha passes down the root-hair and across the cortex, and then breaks up into finer hyphæ, from the ends of which are budded extremely minute germ-like bodies, which Woronin mistook for Bacteria. They are not Bacteria, however, but present more resemblance to the buds discovered by Brefeld in the *Ustilagineæ*.

The author has succeeded in artificially infecting the roots of beans with the fungus, and finds that the minute infecting spores are to be met with in all kinds of soil, so that it is a matter of some difficulty to obtain roots which are not attacked by the fungus. This can be done by burning the soil, and by means of pure water-cultures.

The affinities of the fungus are with the *Ustilagineæ*, and the case is a very remarkable instance of symbiosis.

"On the Structure of the Mucilage Cells of *Blechnum occidentale*, L., and *Osmunda regalis*, L." By Tokutaro Ito, F.L.S., and Walter Gardiner, M.A. Communicated by Prof. M. Foster, Sec.R.S.

The growing point of many ferns is found to be covered with a slimy mucilage, which arises from hairs situated on the paleæ and the leaves; this mucilaginous secretion serves a most important physiological function, in that it readily takes up and retains water, and thus keeps the young bud moist, and at the same time it prevents excessive transpiration. The authors investigated two cases of mucilaginous secretion, viz. *Blechnum occidentale*, L., and *Osmunda regalis*, L. They find that the mucilage arises from the protoplasm only, and not from the cell-wall, and that the whole process is distinctly intraprotoplasmic. They point out that the structure of mature mucilaginous gland is wonderfully like that of certain secretory animal cells recently investigated by physiologists; and they find that in the glandular cells of the ferns mucilage is secreted in the form of drops, and that each drop is further differentiated with a ground substance (gum mucilage), in which are embedded numerous spherical droplets (gum).

The secretion commences by the breaking down of a portion of the innermost layers of the endoplasm of a number of contiguous but isolated areas, and the result of these catabolic changes in the protoplasm is the formation of small but rapidly-growing mucilage-drops. The first formation takes place just beneath the free surface, equally around the whole cell-cavity, and the phenomenon steadily continues from within outwards, producing new drops basipetally, until the whole of the endoplasm has taken part in the process. The cell is now full of isolated drops, each inclosed by a portion of the delicate protoplasmic framework which still remains. A remarkable sequence of changes occurs in the drops themselves. At their first formation they are watery and by no means well defined; they shortly become denser, and then in the drops themselves a delicate reticulation may be observed, which gives way to the appearance of numerous minute and brightly shining droplets, all separate and distinct. The result of their observations makes the authors disposed to believe that during secretion the protoplasm gives rise to a gummy mucilage, and the latter undergoes further differentiation into a ground substance, which still retains its mucilaginous character, and into a gummy substance which is present as a number of isolated spherical droplets. Excretion takes place by the rupture of the cell-wall, all that remains in the cell being a layer of endoplasm with the disintegrated nucleus.

In the case of animal glands, *e.g.* serous and mucous salivary glands, the state of active secretion is followed by a resting-period, during which the protoplasm grows, forms new hyaline substance, and this again produces new granules. The authors believe that a series of changes essentially similar in character occur in plant-cells also. Usually speaking, plant-cells are incapable of such active and repeated secretion, and in many cases, *e.g. Blechnum* and *Osmunda*, the secretion-changes occur in the cell once and for all, and then the cell dies; in other instances, however, *e.g.* the glands of *Dionæa*, it appears exceedingly probable that the phenomena which accompany the repeated secretion are quite similar to those which happen in so many animal cells. They believe that in their main features the phenomena attending the formation of the secretion are very wide-spread, and limited neither to the ferns nor to the particular case of the secretion of mucilage.

Royal Meteorological Society, June 15.—Mr. W. Ellis, President, in the chair.—The following papers were read:—Amount and distribution of monsoon rainfall in Ceylon generally, with remarks upon the rainfall in Dimbula, by Mr. F. J. Waring. The principal feature in Ceylon as determining both the amount and distribution of rainfall is a group of mountains situate in the south central portion of the island, equidistant from its east, west, and southern shores. The south-west and north-east monsoons in Ceylon may be said respectively to blow steadily from May to August inclusive, and from November to February inclusive. In March and April, and in September and October, the weather is more or less unsettled, and no regular monsoon or direction of the air current is usually experienced. After giving details of the rainfall at twenty-five stations, the author concludes by remarking upon (1) the effect of the mountain zone in determining the amount and distribution of the rainfall; (2) the apparent gradual veering of the rain-bearing currents of air as each monsoon progresses; (3) the relative insignificance of the south-west monsoon as compared with the north-east monsoon in inducing rainfall; (4) the cause of the large general rainfall of the north-east monsoon throughout the island generally as compared with that of the south-west monsoon; and (5) the influence of the gaps in the external ring of the mountain zone, and of the central as well as the other ridges in it, in determining the amount of rainfall within the zone and in the neighbouring districts outside it.—Note on a display of globular lightning at Ringstead Bay, Dorset, on August 17, 1876, by Mr. H. S. Eaton. Between 4 and 5 p.m. two ladies who were out on the cliff saw surrounding them on all sides, and extending from a few inches above the surface to 2 or 3 feet overhead, numerous globes of light, the size of billiard-balls, which were moving independently and vertically up and down, sometimes within a few inches of the observers, but always eluding the grasp; now gliding slowly upwards 2 or 3 feet, and as slowly falling again, resembling in their movements soap-bubbles floating in the air. The balls were all aglow, but not dazzling, with a soft superb iridescence, rich and warm of hue, and each of variable tints, their charming colours heightening the extreme beauty of the scene. The subdued magnificence of this fascinating spectacle is described as baffling description. Their numbers were continually fluctuating; at one time thousands of them enveloped the observers, and a few minutes afterwards the numbers would dwindle to perhaps as few as twenty, but soon they would be swarming again as numerous as ever. Not the slightest noise accompanied this display.—Ball lightning seen during a thunderstorm on July 11, 1874, by Dr. J. W. Tripe. During this thunderstorm the author saw a ball of fire, of a pale yellow colour, rise from behind some houses, at first slowly, apparently about as fast as a cricket-ball thrown into the air, then rapidly increasing its rate of motion until it reached an elevation of about 30°, when it started off so rapidly as to form a continuous line of light, proceeding first east, then west, rising all the time. After describing several zigzags, it disappeared in a large black cloud to the west, from which flashes of lightning had come. In about three minutes another ball ascended, and in about five minutes afterwards a third, both behaving as the first, and disappearing in the same cloud.—Appearance of air-bubbles at Remenham, Berkshire, January 1871, by the Rev. A. Bonney. Between 11 and 12 a.m. a group of air-bubbles, of the shape and apparent size of the coloured india-rubber balls that are carried about the streets, were seen to rise from the centre of a level space of snow within view of the house. The bubbles rose to a considerable height, and then began to move up and down within a limited area, and at equal distances from each other, some ascending, others descending. These lasted about two minutes, at the end of which they were borne away by a current of air towards the east, and disappeared. Another group rose from the same spot, to the same height, with precisely the same movements, and disappeared in the same direction, after the same manner.—Mr. H. C. Russell, F.R.S., of Sydney, described a fall of red rain which occurred in New South Wales, and exhibited, under the microscope, specimens of the deposit collected in the rain-gauges.

Entomological Society, June 1.—Dr. David Sharp, President, in the chair.—Mr. Meyrick read two papers, on Pyralidian from Australia and the South Pacific, and descriptions of some exotic Micro-Lepidoptera. In these papers about sixty new species were described. A discussion ensued, in which Dr. Sharp, Mr. Stainton, Mr. McLachlan, and others took part. Mr. Meyrick stated that, as far as the Pyralidina were concerned, Australia could not be regarded as a separate region, for a large number were not endemic, but appeared to have been introduced from the Malay Archipelago. The method of this immigration seemed doubtful. Mr. Meyrick was of opinion that the insects flew very long distances, and effected a settlement through their food-plants being widely distributed and common. He instanced the undoubted immigration of certain Australian species into New Zealand, a distance of 1200 miles. Mr. Stainton adduced the instance of *Margarodes unionalis*, which is a South European insect, feeding on the olive, yet is occasionally found in Britain. —Mr. Meyrick also made some observations on the distribution of the insect fauna in the various regions of Australia: he said that it appeared to be more or less different in certain defined portions of the continent, which might be roughly regarded as oases in the midst of desert districts: all his observations, however, had tended to upset Mr. Wallace's theory that Eastern and Western Australia were originally separated, as the gradations in the insect fauna from east to west were quite gradual; in Western Australia the Tineina were the only group well represented by peculiar endemic forms.—Mr. Pascoe read a paper on the genus *Byrsops*, a genus of Curculionidæ.—The President announced that Lord Walsingham's collection of Lepidoptera and larvæ, recently presented to the nation, would be exhibited in the Hall at the Natural History Museum, South Kensington, until the end of June.

PARIS.

Academy of Sciences, June 20.—M. Janssen in the chair. —On the analytic theory of heat, by M. H. Poincaré. An attempt is here made to determine more rigorously than has hitherto been possible the principles from which are deduced the general laws of the analytical theory of heat in the case of any solid body whatever.—On the employment of crushers ("*manomètres à écrasement*") in measuring the pressures developed by explosive substances, by MM. Sarrau and Vieille. Continuing their studies on this subject, the authors here propose by means of the crusher to determine more especially the maximum pressure produced by an explosive under given conditions.—Fresh materials bearing on the relations which exist between the chemical and mechanical work of the muscular tissue, by M. A. Chauveau, with the assistance of M. Kaufmann. In continuation of previous papers, the author here deals with the nutritive and respiratory activity of the muscles which act physiologically without producing any mechanical work.—On collisions at sea, by M. Jurien de la Gravière. In connexion with the increasing number of disasters caused by preventable collisions, attention is directed to the practical measures recently proposed at various conferences by M. Riondel. Of these the most important are: (1) that all steamers be required to follow one outward and another homeward route, in order to divide the present single stream of traffic into two parallel streams; (2) that a maximum velocity be determined for vessels navigating narrow straits in foggy weather; (3) that the lighting of the high seas be rendered more powerful, and brought more into harmony with present rates of speed; (4) that international maritime tribunals be established in order to adjudicate between vessels of different nationalities. The latter proposition has already been approved by the United States, and several Governments have consented to take part in the future International Conference to which the whole question must be referred.—Observations on the Grazac meteorite, by MM. Daubrée and Stanislas Meunier. This meteorite, which fell two years ago, and to which M. Caravin-Cachin first drew attention, is of a new carbon type, somewhat analogous to those of Orgueil and of the Cape, but distinguished from them by its general appearance and chemical properties. Its breakage is granular, and in many respects it resembles certain varieties of the oxides of manganese and copper, and the bituminous cinnabar of Idria; density 4·16. This new specimen is all the more remarkable that it belongs to the class of rare and interesting meteorites which in their resemblance to our combustible minerals have suggested indications of biological phenomena beyond the globe.—On the molecular specific heats of gaseous bodies, by M. H. Le Chatelier. Since Dulong and Petit's discovery of the law of specific heats for solid bodies, numerous attempts have been made to generalize this law, and to extend it to the gases; but the experimental researches of Regnault have shown that at the ordinary temperature there exists no equivalence either between the molecular heats or the atomic heats of the gases. The experiments here described, on the combustion of gaseous mixtures, lead to the same conclusion for high temperatures.—On the calorific conductibility of bismuth in a magnetic field, and on the deviation of the isothermal lines, by M. Leduc. The discovery of the great increase in the electric resistance of bismuth, when introduced into a powerful magnetic field, has led the author to suppose that this field produces in the structure of the metal a modification, one of the effects of which is the deviation of the equipotential lines. It also occurred to him that this modification of structure should produce on a calorific flux the same alterations as on an electric current, and the experiments here described have fully confirmed these anticipations.—Application of the electrometer to the study of chemical reactions, by M. E. Bouty. In the author's last communication the problem was resolved in principle regarding the application of the electrometer to the study of chemical reactions. Here the subject is illustrated by the example of sulphuric acid and the sulphate of potassa.—On a new regulator of electric light, by M. Létang. The object of this apparatus is to obtain a distinct regulating control by means of a simple contrivance independent of any complicated machinery. The means employed to arrive at this result are based on the employment of a mechanism analogous to that of an ordinary system of electric chimes.—On the manganites of potassa, by M. G. Rousseau. The formation has already been described of a manganite of potassa by calcination of the permanganate at 240° C. But this method is useless for studying the variations of the molecular state of manganous acid combined with potassa under the action of a progressively increasing temperature. Hence the author has had recourse to the dissociation of the manganate of potassa in presence of an alkaline dissolvent.

BERLIN.

Physical Society, June 10.—Prof. Du Bois-Reymond, President, in the chair.—In connexion with his previous communications on the determination of the wave-length of light by the weight of a cube of quartz, Dr. Sommer spoke on the methods of determining the specific weight of bodies, with special reference to the method by weighing them in water. After having discussed the earlier methods and experiments of Marck and Lépiney, he gave an account of the methods he had himself employed in order to do away with the influence which the capillary forces at the surface of the water exert on the wire by which the solid is suspended. He surrounds the wire at the point where it enters the water with a glass tube 5 mm. in width, in which is placed one drop of a mixture of equal parts of olive-oil and benzene. From the lower end of the wire in the distilled water he hangs a tiny tray on which two cubes of quartz are placed. Using a wire 0·1 mm. in diameter, which he finds gives a result as accurate as weighing in air, he determines the weight of these quartz cubes in water, then pushes one of the cubes off the tray by means of a platinum wire which had been previously submerged, and weighs again. He then pushes the second cube off the tray and weighs a third time. These three weighings, taken in conjunction with the weight of the tray and cubes in air, yield an exactitude which up to the present time has either not been attained at all by hydrostatic methods or only by a laborious and roundabout process. The exactness of this method of determining the specific weight of quartz cubes surpasses that obtained by the use of a piknometer.—The President gave an account of a communication which had been made by Siemens at the last meeting of the Akademie der Wissenschaft. A steel tube 10 cm. long, with perfectly smooth external and internal surfaces and extremely uniform bore, and whose walls are apparently of perfectly equal thickness at all points, was prepared by the following method, patented by Männermann in Bemscheid. Two rollers, slightly conical towards their lower ends, are made to rotate in the same direction near each other; a red-hot cylinder of steel is then brought between these cylinders and is at once seized by the rotating cones and is driven upwards. But the mass of steel does not emerge at the top as a solid, but in the form of the hollow steel tube which Siemens laid before the meeting. Prof. Neesen gave the following explanation of this striking result: owing to the properties of the glowing steel, the rotating rollers seize upon only the outer layer of the steel cylinder and force this upwards, while at the same time the central parts of the cylinder remain behind. The result is thus exactly the same as is observed in the process of making glass tubes out of glass rods.

STOCKHOLM.

Royal Academy of Sciences, June 8.—Monograph of the Amphipoda Hyperiidea, part 2, by Dr. C. Bovallius.—Freshwater Algæ, collected by Dr. S. Berggren in New Zealand, and described by Dr. O. Nordstedt.—On a manuscript map of Scandinavia from the middle of the fifteenth century, found in the library of Comte Zamoisky, in Warsaw, by Prof. A. E. Nordenskiöld.—On the sequence of the Glacial beds, and the temperature during the various stages of the Ice epoch, Prof. O. Torell.—On the anatomy of *Hyperoodon diodon*, Miss A. Carlsson.—Some reptiles and fishes showing the called third eye, exhibited and demonstrated by Prof. F. Smitt.—Desmidiaceæ from Greenland, described by Herr Boldt.—On the distribution of Desmidiaceæ in the north regions, by the same.—Contribution to the knowledge of anatomical structure of the Dioscoreæ, by Herr J. P. Jungn —Studies on the spectra of absorption of the rare elements, Prof. L. F. Nilsson and Dr. G. Krüss.—An attempt to cal late the dissociation in the water of solution, by Dr. S. Arr nius.—Contributions to the theory of undulations in a gase body, by Prof. A. V. Bäcklund.—On the changes in volu and density of fluids through absorption of gases, by Dr. Ångström.—On the form of the crystals and twin-crystals scolecite from Iceland, by Herr G. Flink.—Mineralogical not by the same.

BOOKS, PAMPHLETS, and SERIALS RECEIVE

The British Moss Flora, Part x.: R. Braithwaite.—An Introduction the Study of Embryology: A. C. Haddon (Griffin).—Pola seine Vergang heit, Gegenwart und Zukunft; eine Studie (Wien).—Mount Taylor and Zuñi Plateau: Capt. C. E. Dutton (Washington).—Bulletin of the U Geological Survey, No. 38 (Washington).—Annalen der Physik und Chem 1887, No. 7 (Barth, Leipzig).

CONTENTS. PA

THURSDAY, JULY 7, 1887.

PROFESSOR TYNDALL AND THE SCIENTIFIC MOVEMENT.

THE complimentary banquet to Prof. Tyndall, to which reference has more than once been made in these columns, is described in detail elsewhere. We cannot, however, allow an event of so much interest, and which is, we believe, unique in the history of science in this country, to pass without comment.

Many notable gatherings have taken place in Willis's Rooms, but we question if English science has ever been more completely represented than at the "Tyndall Dinner." The President of the Royal Society was in the chair. The seven Vice-Chairmen were Presidents of the most important scientific Societies. The tables were crowded with men whose names are known wherever Nature is studied.

No every-day motive would suffice to bring together such an assembly, and it is not every day that we have an opportunity of doing honour to a life-work such as that of Prof. Tyndall. Others will rank beside or above him as investigators, but in the promotion of the great scientific movement of the last fifty years he has played a part second to none. The English people are a determined but somewhat slow-witted race, and it has been no easy task to convince them that a new era—that of science—was dawning. They have been content to pride themselves on industrial successes due for the most part to isolated efforts of genius, which was hampered by unnecessary difficulties, and which cannot be produced at will. They were long in seeing, they do not yet fully see, that our industrial position can only be maintained if armies of well-equipped followers are ready to seize the ground which the leaders win.

There is, however, a still harder lesson to learn. The industrial application of a scientific principle—vitally important to the well-being of the people as that application may be—requires nevertheless a lower form of intellectual energy than the discovery of the principle itself. The triumphs of applied science, of the physician, the engineer, the telegraphist, are readily "understanded of the people." The research laboratory, on the other hand, is open to few. The flash of genius which has wrung a fresh secret from Nature can only be fully appreciated by those who are intellectually competent to understand the difficulty and the success. And yet, if a widespread knowledge of science was to be, as it is, an essential condition of national well-being, it was absolutely necessary that the people should know something of, and be in some sort in sympathy with, the methods and conditions of scientific thought.

In supplying this need, Prof. Tyndall's greatest work has been done. Uniting scientific eminence of no ordinary kind with extraordinary gifts of exposition, he has, by his lectures and his books, brought the democracy into touch with scientific research. In dozens of lecture-rooms experiments devised by him are proving that a living science is a nobler instrument of education than a dead language. In hundreds of libraries his nervous English is convincing men of the value of a career like Faraday's, and teaching them to appreciate, if they cannot always in detail follow, the methods by which the victories of science are won. He has done perhaps more than any other living man to compel those who regard knowledge as valuable only in so far as it is immediately useful, to admit that the seed which is sown in the laboratory often produces the most abundant harvest in the workshop, and that a desire for knowledge is the mother of inventions which necessity could never have brought to the birth.

Such has been Prof. Tyndall's work ; and yet we venture to think that among those who met in Willis's Rooms a deeper feeling was aroused than admiration for an eminent worker and a useful career.

Many of the greatest masters both of the moral and intellectual life have sought the attainment of their highest ideals in a more or less complete withdrawal from society, and it may well be that some natures can best achieve in seclusion the concentration which a supreme effort demands.

But although the scientific movement of to-day may receive its highest inspirations from men who, like Darwin and Joule, have worked in self-imposed retirement, its distinguishing characteristic is that it is sweeping along with it all classes and all opinions. It is a new habit of thought in the light of which the foundations of our educational, industrial, and political systems are being reconsidered. It is a new and deliberate attempt to put into practice the belief that "the sovereignty of man lieth hid in knowledge, wherein many things are reserved which kings with their treasures cannot buy, nor with their force command ; their spials and intelligencers can give no news of them ; their seamen and discoverers cannot sail where they grow."

Thus it has come to pass that science has gathered round it a crowd of workers, engaged in very various tasks, but all of whom would be ready to admit that the cardinal principle of the movement in which they take part is the investigation of truth for truth's sake alone. They may be professors or manufacturers, soldiers or physicians. If only they are imbued with the desire to penetrate a little further into the mysteries which surround us, if only they are willing and able to add something to the sum of human knowledge, they are scientific men.

In part this army is organized. There is in England no Academy of Literature. The Academy of Arts, it is admitted, needs reform. The principal scientific Societies, however, with the Royal Society at their head, perform the duties of an Academyof Science to the general satisfaction. No human institution is perfect, but it may be fairly said that they set in their Transactions a high standard of scientific work, and that their judgment, whether of men or of investigations, is seldom challenged.

In spite of this advantage, neither the outside world nor scientific men themselves have as yet sufficiently realized that these Societies constitute a great guild of that learning which is the most powerful and the most characteristic influence of our age.

On an occasion such as the Tyndall Dinner this realization is quickened. The curious magnetic influence of numbers is felt. Minor differences disappear in the knowledge that all are workers in the same cause. Men become more vividly conscious that though students of Nature are excluded from the State recognition which is extended to the Church, to medicine, and to the law, they too are members of a great profession. They realize that, though State rewards are given only to those who have applied their knowledge to some directly useful end, in a gathering of the profession of science the true leaders are those who have wrested the deepest secrets from Nature, careless whether they could be turned to gold or no.

A meeting held in great numbers and for a common purpose may have an influence which many an apparently more useful testimonial would lack. Prof. Tyndall has done service in the cause of science which merited the unique compliment he received. He would, we believe, be the first to rejoice if in the future the Tyndall Dinner was remembered not only as a tribute to his own work, but as marking the beginning of a period in which the ranks of science were drawn closer together, and in which the further organization of the investigation of Nature claimed and received the attention which its importance demands.

THE GEOLOGY OF ENGLAND AND WALES.

The Geology of England and Wales. With Notes on the Physical Features of the Country. By Horace B. Woodward, F.G.S. Second Edition. (London: Philip and Son, 1887.)

THE student of physical geology has at least two large English text-books, interesting, full, accurate, judicial, and written by masters of the science; but he who would build on this foundation a knowledge of historical and palæontological geology is in a harder case, and finds either a meagre outline containing little but a few meaningless names of formations and fossil lists, or else an ill-digested and formless mass of matter, derived from everywhere, but leading nowhere. Perhaps the time has not yet come when stratigraphy can be treated from the stand-point of inorganic evolution, so that fact may be joined to his fact and an organized whole result.

While, however, we wait for one who shall give us geology in the form of the inorganic and organic evolution of the globe, we must not omit to notice the labour of those whose "work is to record the facts from which the pleasanter deductions may be made." Mr. Woodward has done wisely in republishing by subscription and in an enlarged form his admirable book on the geology of England and Wales—a veritable mine of facts, well indexed and admirably supplied with references for the advanced reader, forming a base-line for further study and research, but complete in itself for the more elementary student and rendered interesting by the author's fresh style, by his capital and apt illustrations, and by his wonderful faculty of seizing upon the individuality of the rock group he is describing and skilfully tracing its variations from place to place. This new edition is improved by a larger and better map, undertaken by Mr. Goodchild, by more free use of sections, illustrations, and fossil lists, and by the employment of local names with tables of correlation.

The author works his way upwards from the lowest rocks, but combines a geographical with a chronological arrangement, and varies his method from system to system in order to adapt it better to the rocks under consideration. Just occasionally one meets with a slip in method, as in the case of the Rhætic rocks, where for no apparent reason he has reversed his usual order and treated the White Lias first. Where the mass of facts is unusually great and somewhat barren of interest, the author has introduced little helps and alleviations for which the student will be truly grateful,—the character of the hero of a system sketched in one graphic touch, the origin of the name of a system or a fossil, or the discussion of the origin of some bed of palæontological or economic value (*vide* pp. 24, 47, 84, 266, 670).

It seems hard to criticise any points of detail in such well-intentioned and well-executed work, but the indication of a few lines for improvement will perhaps show better than anything else how little the author has left for others to suggest. First, with regard to the map. This is clearly engraved, and coloured with light but well-contrasted tints; every name on it suggests some fact interesting from a geologist's point of view, and the effect of the whole is pleasing. There is no special colour for the Permian (not an unmixed advantage), and, oddly enough, the Yorkshire coal-field is left uncoloured; the boundary is engraved, however, and the student can easily fill in the colour for himself. A point has been gained in using a distinctive colour for beds below the Bala, but one lost in not using still another for the lowest Cambrians. The igneous rock colours should have been used less sparingly, and surely the Arenigs and Snowdon deserve a volcanic tint as much as the Borrowdales and the Cheviot rocks. We miss, too, the north of England dikes and the Whinsill.

The book opens with an introduction containing a little history, a little cosmogony, and a few definitions. The latter are hardly needed, and might have made room for the accounts of the geology of different lines of railway, which found a place in the first edition but have been crowded out of this. A few words on the Palæozoic group are followed by an account of the Archæan system, in which too little is said of the new class of work amongst these rocks instituted by Prof. Lapworth, while Prof. Bonney's papers on the Bangor area are almost passed over. The table on page 52 hardly makes it quite clear that the Harlech group of St. David's is divided into the Caerfai and Solva groups, of which the former constitutes the *Annelidian* of Lapworth, and the latter, together with the Menevian beds, the *Paradoxidian*. On page 58 we find the time-worn section across that part of the Longmynd which teaches nothing of the succession of the Longmynd rocks; this and several other sections should have been orientated. On page 60 the Hollybush sandstone is omitted from the table of Shropshire Cambrians, and awkwardly placed on page 65, while the Shineton shales are correlated with the Dolgelly beds and Malvern black shales in the table, though afterwards correctly placed with the Dictyonema shales and Lower Tremadoc. A deceptive appearance of unconformity in sections on page 89 might easily have been removed, even if present in the original woodcut. It is good to see Mr. Lewis's name brought up with

Murchison's on page 103, in connexion with the Aymestry limestone.

The author mentions, but does not definitely accept, Prof. Hull's correlation of the Devonian rocks. Throughout the work, and particularly in the Carboniferous section, great care has been taken to show and where possible give the origin of the economic value of the rocks. A little more stress should have been laid on the relations of the Coal-measures to the underlying rocks, and one might notice here the absence of the Dudley, Sedgeley, and other inliers from the South Staffordshire coal-field on the map. An important feature consists in the description of Palæozoic rocks from all the deep borings (a list of these forms the first appendix); and a good section to express the present state of knowledge on the deep-seated geology of the London Basin is given on page 202.

The Permian and Lias form a single system, the Poikilitic, which is included in the Mesozoic, the author being guided by the widespread discordance between it and the older rocks. It is not quite easy to understand all the tables (pp. 286, 470), but these only echo the difficulties which exist in the rocks themselves. It would have been as well if the Yorkshire Cornbrash had found a place after the Upper Estuarine on page 321. A good opportunity was missed of discussing the anomalous beds of Faringdon and Blackdown, particularly in relation to Mr. Starkie Gardner's recent papers on kindred questions; and we should have liked to see the grit phases in the Jurassic clays of the eastern counties more accurately defined. A section might have been introduced to show the thinning of the Gault and growth of the Cambridge nodule beds; and Mr. Sollas's work on flints ought not to have been omitted.

The Upper Eocene beds are classed as Oligocene, but the Brockenhurst bed is put in its true place in the Headon.

There are some very suggestive remarks on the connexion between health and geology, between villages and springs and consequently the outcrop of porous rocks, and on the effects of percolation of spring and sea water through rocks. The section on igneous rocks is of necessity somewhat vague and unsystematic from its brevity, but room has been found to treat the volcanic rocks historically; the Nuneaton diorites are intrusive in pre-Carboniferous rocks only. There are concluding chapters on metalliferous deposits, and on scenery and geology, the latter containing a useful list of hills, valleys, plains, and forests.

A little more space might with advantage have been spent in indicating with greater fulness what is known of the physical geography of the different periods, and epochs of earth movements, their dates, directions, and effects should have been more fully dealt with in the last chapter. A capital synopsis of the animal kingdom is furnished in an appendix by Mr. Edwin T. Newton; and a grand index, occupying 45 pages of three columns each, and giving the dates of the birth and death of authors referred to, closes the volume, which is an excellent summary of the present state of our knowledge of British geology. The author has worked conscientiously and well, and that we have been able to suggest so few additions clearly shows that his labour has not been in vain.

W. W. W.

A TREATISE ON GEOMETRICAL OPTICS.

A Treatise on Geometrical Optics. By R. S. Heath, M.A., D.Sc., Fellow of Trinity College, Cambridge, Professor of Mathematics in the Mason College, Birmingham. Demy 8vo, pp. xvii. 356. (Cambridge: University Press, 1887.)

THIS treatise is based on the conception of a beam of light as consisting of a system of rays, which obey the laws of reflexion and refraction. The transformations of such a system and the construction and properties of optical instruments are deduced, so far as the latter are capable of explanation from this point of view.

In confining himself to geometrical optics in this sense, the author follows the mode of division of the science which has been usually adopted in text-books in this country, through the succession of Cambridge treatises by Coddington, Griffin, and Parkinson, and Lloyd's "Treatise on Light and Vision." The subject then splits up naturally into the theory of reflexion and refraction of systems of rays, which is in fact a department of geometry; and the more special discussion of the nature of optical instruments and the forms and positions to be given to their refracting surfaces to diminish spherical and chromatic aberration, which allies itself with the technical science of optical construction.

The book begins with a short chapter on the nature and properties of light, in which the theory of illumination is worked out as a consequence of the experimental fact that self-luminous surfaces appear equally bright in all directions and at all distances. The second and third chapters contain the statement, in geometrical and analytical form, of the laws of reflexion and refraction, and the investigation of conjugate foci for direct pencils.

In Chapter IV. the subject of refraction through lenses and systems of lenses is treated, use being made of the symmetrical analysis, by means of the convergents of continued fractions, to determine the principal points of a system whose refracting surfaces are specified. Free use is also made of the cardinal points of the system in the semi-geometrical manner introduced by Möbius. The following chapter is devoted to an account of the general analytical investigation by means of which Gauss placed the whole theory on an independent basis. The notion of the equivalent lens is here introduced to some practical purpose, for the investigations of this and the preceding chapter enable the author to specify the exact character of the equivalence that can be secured by a single lens or a single refracting surface: viz. that if the lens or surface occupied the position of one of the principal planes of the system, it would refract any beam incident along its axis into the same configuration as it actually possesses when it emerges through the other principal plane of the instrument; so that, neglecting aberrations, the equivalence holds in every sense except as regards the displacement along the axis, and is therefore complete for most practical purposes.

The theory of caustics is treated, chiefly by analytical methods; and the existence of wave-surfaces, which cut the system of rays at right angles in an isotropic medium, is established geometrically.

Chapter VII. is devoted to the discussion of the spherical aberration of direct pencils, which is perhaps one of the most difficult parts of the subject to present in an elegant manner, on account of the non-symmetrical character of the necessary approximations. The treatment here given seems to leave nothing to be desired.

Chapter VIII. begins with an exposition of the properties of a general system of rays: this with the cardinal result that the rays are all bi-tangents to a focal surface is ascribed to Kummer. They had, however, been previously given by Hamilton in his memoir on "Systems of Rays," in the discussion of ray-systems in a crystalline medium where the wave-surface no longer cuts the rays at right angles; and he in turn refers back to the same papers of Malus which contain the theorem of orthogonality in isotropic media.

The theory of the characteristic function is next applied to the solution of the general problem of the refraction of a narrow beam at a surface of double curvature; and to the analytical determination of the relation between the forms of such beams before and after passing through a general optical instrument whose internal structure is not specified. In these discussions the author has closely followed a series of papers by Clerk Maxwell which appeared about fifteen years ago in the Proceedings of the London Mathematical Society, and which presumably were to find a place in a book on optics then contemplated by their lamented author. It does not seem to have been much noticed in this country that the same formulæ for oblique refraction were developed a long time ago by Sturm and others, in a direct geometrical manner, from Malus's theorem; but the conciseness and precision which arise from defining a beam by means of its characteristic function give them an enhanced importance in optical theory. Their application is here given to some cases which we do not remember having seen published before: thus the modification impressed on a beam by refraction centrically through a single thin lens is expressed by means of very simple formulæ, from which several properties of considerable elegance and some practical value might be directly drawn.

The theory of dispersion and achromatism is treated in the ordinary way. In the chapter on vision are introduced discussions, chiefly from Helmholtz, of the mechanism of accommodation and the principles of binocular vision. Then follows a clear and valuable chapter on telescopes and microscopes, a chapter on miscellaneous optical instruments, and a brief account of atmospheric refraction, mirage, rainbows, and halos.

It may seem ungracious to expect more where so much is given, but we could have wished that the theory of refraction through general systems had been treated more from an historical standpoint. A difficulty often felt in this part of the subject arises from the way in which the geometrical and analytical methods of different writers are liable to be intermixed. The book was probably in the press before a recent note by Lord Rayleigh had brought again into prominence the large share taken by the English opticions of last century, notably Cotes and Smith, in the development of the general theory of this branch of the subject.

The list of treatises and memoirs might be improved by consulting the bibliographies given by Helmholtz and Verdet.

It is a misfortune incident on the scheme of the book that it is seldom able to say the last word in relation to the more delicate arrangements of telescopes and microscopes, where diffraction plays an important part. This becomes very patent, for example, in the account of immersion objectives. The theory of diffraction as applied to optical construction is for the most part purely geometrical, and it would much increase the value and interest of books on geometrical optics if that theory were explicitly included, and the subject introduced by the consideration of light as wave-motion, instead of the artificial conception of the reflexion and refraction of rays.

As is usual in English text-books, selections of problems have been added at the ends of the chapters. In this case, Cambridge examination-papers of recent years have been largely drawn upon for questions, with the result that some are included which are not of much value as illustrations of the subject, though they may be very useful as tests of mathematical power. Indeed it seems open to question whether the practice of adding large collections of examples is not now overdone in this country; it certainly in some cases tends to unfit the books which contain them for the use of students who do not possess the advantage of tuition, or some guidance in selecting the few that will be of value for them.

The treatise is, on the whole, a most welcome addition to our optical text-books. Much of its contents, though fundamental and elementary, has only hitherto been accessible in English through Mr. Pendlebury's treatise on "Systems of Lenses"; and there is more that now appears in a text-book for the first time. The printing and general appearance of the book reflect great credit on all concerned with it.

J. LARMOR.

OUR BOOK SHELF.

Shores and Alps of Alaska. By H. W. Seton Karr, F.R.G.S. (London: Sampson Low, 1887).

THIS is a very interesting account of a journey of exploration in a country which, as the author says, is probably destined soon to become better known. The most important part of the book is that which relates to the attempt made by Mr. Seton Karr and his companions upon Mount St. Elias. When this attempt was made, the combined "alpinism" of the climbers was "insignificant." Nevertheless, they achieved considerable success, and the writer has been able to present a vivid and striking record of their observations. The height of Mount St. Elias was differently estimated by the old navigators, and Mr. Seton Karr points out that it is the only mountain the real height of which has exceeded the first estimates made of it. The latest determination taken from Yakatat and from the United States Coast Survey schooner *Yukon*, gives 19,500 and possibly 20,000 feet. From its massive shape the mountain does not convey the impression of being quite so high as this, although "its whole altitude is presented to the eye, from its sharp summit down to the ocean at its foot." Of the scenery of which Mount St. Elias is the most prominent feature, Mr. Seton Karr writes most enthusiastically. He even goes so far as to say that "without a doubt the scenery at Yakatat is the most wonderful of its kind in the whole world." Seen early in the morning, when the air is remarkably transparent, the mountains seem "too ethereal to have any actual existence." The observer feels that "they cannot be anything except some unholy illusion that must dissolve and disperse when the sun rises."

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Relation of Coal-Dust to Explosions in Mines.

THE suggestion in my former letter on this subject (vol. xxxiv. p. 595) that "keeping the ventilating air-current saturated with aqueous vapour" might prove the most effective way of rendering the dust in coal-mines innocuous, has, I am glad to see, been since shown to be practicable, in a South Wales colliery. Since the above date, I have considerably extended my research, with results that confirm the conviction therein expressed that many of the most disastrous colliery explosions during the last seven years in this northern district have been practically *dust explosions*, and therefore preventable; that the rough method of watering the floors only, or floors and sides, of the mines is delusive, since it leaves the most dangerous dust undisturbed, the upper and flocculent dust; and last, that probably the reasons why dust in dry pits does not explode more frequently are now within grasp. To this latter conclusion, with your permission, I will now briefly address myself. That every firing of a shot that is accompanied by flame in a dry and dusty pit does not produce an explosion is well known; that *sometimes* such firing of a shot does is unhappily also well known. That the local presence of gas, even in small amount, is sometimes the reason of this is universally acknowledged. That the amount and condition of the dust present (even in the practical absence of gas) is at other times the reason is now believed by many. Setting aside the *amount* of dust, which every one will allow must be an essential factor, and also the varying energy which the shot, blown out or not, develops, let us look at the other conditions. The temperature and hygroscopic state of the air-current is one most important factor, and consequently the concomitant temperature and hygroscopic state of the dust traversed by such current. Beyond this, the *degree of fineness* and the *constituents* of the dust will have much to say in the matter. The finer the particles the more readily will they ignite, and the more completely will they place their substance under the influences present. Thus ordinary screen coal-dust will not ignite when a common match is lighted and applied to it, but it will when finely pounded in a mortar. Now the dust resting on the baulks and upper portions generally of the ways will invariably so light and burn when dry, although the constituents vary greatly in different pits and in different seams of the same pit.

What are the ordinary *constituents* of coal-dust? Two, perhaps three, important substances, and others unimportant: important, as being inflammable in varying degrees; unimportant, either from their uninflammability or from their excessively small amount. The three important are mother of coal, or *dant; coal;* and certain coloured bodies, probably *spores.* The unimportant are shale or other stone dust, iron pyrites, lime flakes, and incidentals, as animal and vegetable matters, and the results of the wear and tear of the haulage and winning apparatus, &c. Dismiss these last, as only one needs any attention, the shale; and that special, not general.

Dant lights most readily; the red end of a used match is often sufficient to fire it, and then it burns itself out whether resting on wood or stone. Burned in a retort, it loses little weight, and the fumes it gives off will not ignite. Now, this dant is largely present in upper and flocculent dust, reaching in some specimens even 70 or 80 per cent. Dant clearly therefore is not itself dangerously explosive, yet is admirably fitted to act the part that tinder used to do, when it handed on the spark from the flint and steel to the old-fashioned brimstone match.

Coal forms a considerable part of all upper and flocculent dust, and constitutes the great mass of the bottom dust along intake haulage roads. Coal-dust (got as free from dant as possible) when pounded very fine ignites with some difficulty, burns at first somewhat fiercely and with considerable smoke, but generally goes out leaving a portion of the heap unburned. Placed on an iron plate and burned by heating the plate, it threw off scintillations, its fumes readily took fire, and forty grains of dust were reduced to one grain of ash. In a retort it gave off first much smoke which would not light; soon, however, the smoke lessened, when its fumes lit and burned with a long bright flame. Such coal-dust is manifestly capable of producing an explosion. Under favourable conditions it can produce a considerable amount of ordinary illuminating coal-gas, whose presence would convert the air-current into an explosive mixture. Therefore, adopting the former simile, as the dant is the *tinder*, so this coal is the *sulphur match*, as the shot flame or other initial cause is the *spark* struck from the flint and steel.

Spores.—Nearly all dusts (and I have examined many) have shown under the microscope few or many orange, brown, or reddish flakes, very often triangular in shape and with concoidal fractures. I have not yet examined thin sections of these coals, but the fragments present much the appearance presented by the spores in the well-known spore coals of the Bradford "Better Bed," and Leicestershire "Moira." If these coloured bodies originate in Lycopodian and other microspores or macrospores, they may play an important part, for the resinous nature of the microspores of the *Selaginella selaginoides*, &c., of our northern hills is so well known that they were formerly used in theatres to produce artificial lightning. As my experiments and inquiries in this direction are yet incomplete, I will only suggest that their presence may account for some dusts being so much more dangerous (as the German experiments have conclusively shown) than others, and add the hope that these words may lead others to pursue this inquiry. ARTHUR WATTS.

Bede College, Durham, May 26.

Science for Artists.

OF the various optical errors in this year's pictures, certainly that in the elegant scene (624) of the Queen's Accession, in the morning small hours of June 20, 1837, is largest and most hopeless. Neither a source of light at 93,000,000 miles, nor one at 93 inches, could cast the bar-shadows. It is impossible to say whether they are meant to be aërial in the dust or mist, or cast on the walls and wainscot. But for either they are equally preternatural, though not by diverging perspectively. If cast on the solids they would, instead of being straight, be crooking in and out over the mouldings. But if they are in aërial mist or dust, the error is in supposing the same eye can see more than one of such shadows at a time. The eye requires to be very nearly in the plane of the shadow seen, so that, of those cast by parallel things, as window-bars, only one could be seen by any single eye, and only as continuing the line of the bar itself. The bar and its mist-shadow could never meet at an angle, as they all do in this picture. Another error (now common) is in there being no more penumbra than if the sun were a star, or a small electric arc-light. EDWD. L. GARBETT.

Weight, Mass, and Force.

WITH reference to the extract, as to the language employed in which Prof. Greenhill invites my criticism, I have no doubt that to an engineer it would convey perfectly definite and intelligible information, and that one who has mastered the fundamental notions of dynamics as a science would be able to divine its meaning, but Prof. Greenhill would hardly maintain that the language is scientifically accurate, and that, however sufficient as a shorthand for the trained engineer addressing engineers, it is not full of pitfalls for the tyro.

There is no need to object to the statement that "the *weight* is 137,000 pounds," though it is just as easy to say, "the *mass* is 137,000 pounds." But that "the boiler carries 160 pounds of steam," I find, means that the pressure of the steam is 160 pounds (weight) *per square inch*, while "a 96-feet grade" means "a gradient of 96 feet *per mile*." Surely, except as a recognized shorthand for experts, the suppression of the words in italics is unjustifiable and liable to lead into error.

It is more important, however, to observe that (as in a great majority of the cases an engineer has to deal with) the question here discussed is essentially a *statical* one. The motion of the train considered is uniform (30 miles per hour), and the variations in pressure in the cylinders, &c., are avoided by taking the "mean effective pressure," so that there are no *accelerations* to be considered, and only, in fact, a balancing of forces. The question of *mass* therefore, (a purely *kinetic* notion), can hardly arise, and there is no room for confusion between mass and weight. R. B. HAYWARD.

Upper Cloud Movements in the Equatorial Regions of the Atlantic.

I AM sorry that the observations of so good an observer as Capt. D. W. Barker should not agree with my own, but I certainly never confounded what he calls high low-level clouds with the true high clouds.

When clouds are being propagated in a different direction from that in which they are being blown—as sometimes happens—it is exceedingly difficult to ascertain the real direction; but that would not account for the discrepancy between our observations.

My own researches were specially directed to the doldrums, and the history of the Krakatão dust entirely confirms my observations; but in some low latitudes—as in Cuba—the highest clouds are usually from about south-west. This, however, does not affect the doldrum districts. RALPH ABERCROMBY.

21 Chapel Street.

Fish Dying.

IN a large pool in this county, well stocked with fish, especially trout and roach, a considerable number of the roach have been found dead every day during the last week. They are in fair condition, and show no evidence of poison or of parasitic disease. There is a certain amount of current through the centre of the pool, but the ingress of water has been, of course, much reduced by the drought. The pool, however, covers many acres, and there are twenty feet of water in the deepest parts. Can any of your readers suggest a cause for the death of the roach, and a remedy? No other species appears to have suffered. F. T. MOTT.

Birstal Hill, Leicester, July 4.

THE DINNER TO PROFESSOR TYNDALL.

THE dinner to Prof. Tyndall, as we stated last week, was going on at Willis's Rooms on Wednesday evening as we went to press. It was attended by as large and distinguished a company as ever assembled to do honour to a man of science. The chair was taken by Prof. Stokes, President of the Royal Society, who had acted as Chairman of the Organizing Committee. Among those who had consented to serve on the Committee were the Marquis of Salisbury, the Duke of Devonshire, the Duke of Argyll, the Right Hon. J. Inglis, the Earl of Rosse, Earl Granville, Sir F. Abel, Prof. Adams, and many others holding high positions in connexion with scientific and learned Societies, and Mr. J. Norman Lockyer and Mr. A. W. Rücker had acted as honorary secretaries to the Committee. Among those who attended the dinner were the Earl of Derby, Earl Bathurst, the Earl of Lytton, Sir F. Leighton, Lord Rayleigh, Lord Thurlow, Sir J. Lubbock, M.P., Sir W. Bowman, Sir F. Bramwell, Sir I. Lowthian Bell, M.P., Sir J. Lister, Sir H. Roscoe, M.P., Sir G. Richards, Lord A. Russell, Sir F. Pollock, Sir Lyon Playfair, M.P., Sir Prescott Hewett, Prof. J. C. Adams, Colonel Donnelly, Sir J. Hooker, Prof. Asa Gray, Prof. Flower, Dr. A. Geikie, Dr. Hirst, Mr. W. Crookes (President of the Chemical Society), Mr. G. B. Bruce (President of the Institution of Civil Engineers), Mr. D. Adamson (President of the Iron and Steel Institute), Dr. J. Evans (President of the Society of Antiquaries), Prof. B. Stewart (President of the Physical Society), Prof. Judd (President of the Geological Society), General Strachey (President of the Royal Geographical Society), Sir J. Fayrer, Sir H. Wilde, Sir H. Doulton, Sir J. Caird, Sir P. Magnus, the President of the Alpine Club, Profs. Frankland, Debus, Tilden, Ray Lankester, Liversedge, G. Darwin, Dewar, M. Foster, Carey Foster, Odling, Gamgee, W. G. Adams, Clifton, Humphry, and Dallinger, Messrs. Warren de la Rue, Gill, Kempe, J. Hopkinson, H. Pollock, E. Wood, Brudenell Carter, Romanes, Pengelly, Preece, Ellis, Vernon Harcourt, R. H. Scott, and others.

At the close of the dinner Mr. Norman Lockyer, at the request of the Chairman, read a list of absentees, from most of whom had been received letters expressing strong sympathy with the object of the banquet, and admiration of the career of Prof. Tyndall. Among the writers were the Marquis of Salisbury, Mr. Goschen, Mr. W. H. Smith, Lord Cranbrook, the Marquis of Ripon, the Earl of Rosse, Lord Monk Bretton, Profs. Max Müller, J. R. Seeley, T. H. Huxley, Sir F. Abel, and about thirty others identified with science and literature.

The first toast was "The Queen," and

The Chairman in proposing it said that the recent celebration of the Jubilee diminished the necessity for saying many words in commendation of the toast. All hearts were affected by the Queen's letter, in which she so touchingly acknowledged the manner in which she had been received. Those who were present at the scene in the Abbey were touched by the exhibition o family devotion and affection which took place at the conclusion of the service, when the Royal Family salutec her who was at the same time Sovereign and mother, anc received from her the kiss of affection. And as on tha occasion the Royal Family was united with the Sovereign so on the present occasion, in drinking the health of He Majesty, they would mentally include the health of th Prince and Princess of Wales and the rest of the Roya Family.

The toast was drunk with all the honours.

The Chairman in proposing the toast of the evening said:—My Lords and Gentlemen,—I now come to th toast of the evening, "The Health of Dr. Tyndall," an may he long enjoy the leisure which he has so wel earned. A social gathering like the present is not a occasion on which it is desirable to enter into detail a to the scientific labours of a man, however eminent. Ye the circumstances of the present meeting seem to deman that I should say a few words on some of Dr. Tyndall' researches. Some of his earliest scientific work related t diamagnetism and magnecrystallic action, and in part o this he was associated with the well known German phy sicist Knoblauch. But I cannot dwell on these now. An I will even dismiss with this brief mention his researche on the properties of ice and his application of them to th theory of glaciers and the observations which he made i common with his friend and colleague Prof. Huxley whose necessary absence from among us to-night we s much regret. If I be not trespassing too much on th patience of those who listen to me, I would wish to say little more on that elaborate series of researches, formin no less than six separate papers in the Philosophica Transactions, in which Dr. Tyndall investigated the rela tion of simple and compound gases and of vapours t radiant heat, especially radiant heat from sources at moderate temperature. According to his researche while the main constituents of the earth's atmospher nitrogen and oxygen, are practically diathermous, at leas with regard to radiations which can traverse rock-salt, a we know that by far the greater part of those that w have to deal with can, such is far from being the case wit other gases equally transparent with regard to light. D Tyndall found that as a rule the more complex the com position of a gas the greater is its defect of diathermancy To confine ourselves to the two gases which occur in th atmosphere mixed with its main constituents—I allude c course to carbonic acid and to water in the gaseous stat of vapour—he found that both, especially the latter, whic likewise is present in by far the larger quantity, are ver distinctly defective in diathermancy, and he conclude that the main part of the absorption of solar heat passing through the atmosphere, absorption as disti guished from scattering, is due to the watery vapo which it contains. From this result he drew importa inferences as to atmospheric temperature and climat logical conditions. Dr. Tyndall's researches on the rel tion of gases to radiant heat came naturally before r during my long tenure of office as one of the Secretaries of the Royal Society; and for my own part I may say that it seemed to me all along that the results were established on so firm a basis, and the conclusions regarding the invisible radiations were so perfectly analogous to what we know to be true regarding the visible ones, where the investigation is comparatively easy, that the work bore on it the stamp of truth. The conclusions were not, however, accepted without opposition. In the late Prof. Magnus Dr. Tyndall met a foeman worthy of his steel; a foeman, however, only in the sense of an intellectual athlete; for socially I doubt not they were the firmest friends, and their friendship was even cemented by the fact that they were both alike seeking after truth in a similar subject. But truth only gains by opposition: its defenders are led to engage in fresh researches, which end in strengthening its foundations. I think that the validity of Dr. Tyndall's results is now generally admitted. If some hesitation is still felt, it arises mainly, I think, from misconception; from imagining that assertions which were meant to apply only to heat-rays of such refrangibilities as to be absorbed by water were meant to be affirmed of the invisible radiations generally which lie beyond the extreme red. The time reminds me that I must only very briefly refer to another investigation in which Dr. Tyndall has more recently been engaged, and of which the interest is biological while the means of investigation are physical; I allude, of course, to the question of abiogenesis. Here, again, Dr. Tyndall was working on contested ground, and the objections of opponents stimulated him to fresh inquiries, which resulted in the continual strengthening of his negative conclusions. In the course of his work he was led, for instance, to the discovery of the great difference which exists between the germs of microscopic creatures and the creatures themselves, in relation to their power of resisting the destructive influence of a high temperature. This discovery not only detected a source of error in some experiments which had seemed to favour the hypothesis of abiogenesis, but threw important light on the conditions which must be fulfilled in order to secure complete sterility. But original research is not the only way in which a man can advance the cause of science. All-important though it is, it nevertheless often happens that an original investigation is too abstruse to be followed by more than a few experts; nor is it by any means necessarily the case that an eminent investigator is equally successful in expounding to others, especially to a mixed audience, the results at which he himself or other investigators may have arrived. The general diffusion of science depends largely on the clearness with which its leading principles and results are expounded, whether by lectures or by treatises, in which, while they are scientifically sound, popularity of style and general readableness are not sacrificed to the dry exactness of scientific detail. Most of us have had opportunities, whether at the Royal Institution, with which the name of Tyndall has so long been connected, or elsewhere, of being impressed with the singularly lucid style and graphic expression with which he expounded to his audience the salient points of the scientific subject which he brought before them. Nor was it only in clearness of verbal exposition that he excelled; the manipulative skill with which his original investigations were carried on served him in good stead in his more popular expositions; and by the aid of that "domestic sun," which even the murky atmosphere of a London winter could not obscure, he was enabled in very many cases to exhibit to the audience the actual results of experiments which had first been carried out in the quiet of the laboratory. Nor is it our own countrymen alone who have had the benefit of Dr. Tyndall's lucidity of exposition. Our friends across the ocean have flocked to hear and have appreciated the lectures which he has there delivered as a free gift to Transatlantic science. But oral lectures, after all—the lectures at least of one individual—can only reach a fraction of the community; nor do they admit of that pause for thought which the learner requires in endeavouring to make himself master of a new subject. But the same qualities of mind which enable a man to be a clear and interesting lecturer fit him also to be the author of eminently readable books; and for the general diffusion of science which is taking place we owe much to the writings of Dr. Tyndall. My lords and gentlemen, I fear that I have trespassed too long upon your time, and I will therefore now conclude by asking you once more to drink to the health of Dr. Tyndall. (The toast was drunk with great enthusiasm, the company rising.)

Professor Tyndall, on rising to respond, was received with loud cheers, the company rising. He said:—Mr. President, my Lords, and Gentlemen,—When the project of a dinner was first mentioned to me by a very old and steadfast friend of mine, who, to my regret and his, is not here to-night, had any dream, or vision, of the assembly now before me risen on my mind's eye, I should have declined the risk of standing in my present position; for I should have doubted, as I still continue to doubt, my ability to rise to the level of the occasion. Gratitude, however, is possible to all men; and I would offer you, Sir, my grateful thanks for the manner in which you have proposed this toast; I would thank with equal warmth an assembly which, in intellectual measure, is, probably, as distinguished as any of the same size ever addressed by man, for the way in which they have received it; and I would extend my thanks to my friends of the Department of Science and Art, for their spontaneous kindness to an old colleague, who for many years lent his humble aid to the Department in diffusing sound scientific knowledge among the masses of the people. My own scientific education began late. It had, of necessity, to be postponed until after I had reached the age of seven or eight and twenty. Notwithstanding this drawback, in learning, teaching, and working in the laboratory, I have been permitted to enjoy a spell of thirty-nine years. In 1850, during a flying visit from Germany to England, I stood, for the first time, in the bright presence of Faraday. In February 1853, I gave my first Friday evening lecture in the Royal Institution; and three months afterwards, on the motion of Faraday, the old Chair of Natural Philosophy, which had been filled at the beginning of the century by Thomas Young, was restored, and to it I was elected. It causes me genuine pleasure to think that I shall be succeeded in that Chair by so true and so eminent a man of science as Lord Rayleigh.

It is not my intention to overburden you with egotism to-night; but, casting an earnest glance back upon the past, a few words seem due from me to the memory of one or two of the group of good men, no longer with us, with whom I was so intimately associated. Regarding Faraday I will confine myself to stating that years have not altered my estimate of the beauty and the nobleness of his character. He was the prince of experimental philosophers; but he was more than this—in every fibre of his mind he was a gentleman. It is, however, of two of our honorary secretaries that I wish now to speak; premising that, for the first seven years of my life in the Royal Institution, the post of honorary secretary was held by a cultivated and very worthy gentleman, the Rev. John Barlow. From 1860 to 1873—that is, for a stretch of thirteen memorable years—I had the happiness of working hand in hand with Dr. Bence Jones. Never in my experience have I met a man more entirely and unselfishly devoted to the furtherance of scientific work. I hardly like to mention the following incident, because it furnishes but a scanty measure of his devotion. On one occasion I was in need of funds to carry out some experiments of a delicate and costly character. Bence Jones came to me, and after some hesitation—for he knew that money was likely to raise a difficulty between us—he said, with earnestness: "Dear Tyndall, behave as my friend; do me the favour and the honour of devoting this to your investigation. There is more, if you need it, where that came from." He handed me a cheque for £100. Had I asked for £1000, he would have given it to me, and the world, as far as he was concerned, would have been none the wiser. Bence Jones was a strong man, and liked to have his own way. At first, as was natural, we sometimes surged against each other; but these little oppositions were rapidly adjusted, and for many years before his death the tie of brother to brother was not truer or tenderer than that which united myself and Bence Jones. On my return from the United States I found him dying. In fact, the knowledge of his condition caused me to take leave, earlier than I otherwise should have done, of a people that I had learnt to trust and love. Soon after my return I saw him lowered into the grave.

The death of Bence Jones, whose steadfast loyalty to the Institution he loved so well, showed itself to the last, was a sore calamity to be met. At that time one man only seemed fitted to supply his place. That man was the beloved and lamented William Spottiswoode. To him I appealed to stand by the Institution at a critical hour of its fortunes. He had his own mathematical work on hand, and he was too well acquainted with the duties of our honorary secretaryship to accept them lightly. After much reflection, he wrote me a letter regretfully but distinctly declining the office. But he reflected a second time. He knew that his refusal would cause me pain, and his affection for me prevailed. When, therefore, the letter of refusal—for he sent it to me—came, it was accompanied by a second letter, cancelling the refusal and accepting the post. With William Spottiswoode I had the happiness of working in close companionship for six years. The diligence, wisdom, and success with which he discharged his onerous duties—the princely hospitality which shed a glow upon the office while he held it—are well remembered. Of the dignity with which he afterwards filled the high position now occupied by the illustrious man who presides here this evening it is needless to speak. Him also we have seen lowered to his rest, amid the grief of friends assembled to do honour to his memory. Such were the men who served the Royal Institution in the past; and their example has been worthily followed by other men of eminence, still happily amongst us. Never was an institution better served than the Royal Institution, and not by its honorary secretaries alone. With singleness of purpose and purity of aim, its successive Presidents, Boards of Managers, and honorary treasurers have unswervingly promoted the noble work of investigation and discovery. May they never lower the flag which, for well-nigh a century, they have kept victoriously unfurled.

The year after my appointment I was called upon to deliver, in conjunction with Dr. Whewell, Faraday, Sir James Paget, and some other eminent men, one of a series of lectures on scientific education. I then referred with serious emphasis to the workers in our coal-mines, and to the terrible perils of their occupation. I pointed to the intellectual Samsons toiling with closed eyes in the mills and forges of Manchester and Birmingham, and I said: "Give these toilers sight by the teachings of science, and you diminish the causes of calamity, multiply the chances of discovery, and widen the prospect of national advancement." Thus early, you will see, I was alive to the importance of technical education; and I am no less alive to it now. You will not, therefore, misunderstand me when I say that to keep technical education from withering, and to preserve the applications of science from decay, the roots of both of them must be well embedded in the soil of original investigation. And here let it be emphatically added, that in such investigation practical results may enter as incidents, but must never usurp the place of aims. The true son of science will pursue his inquiries irrespective of practical considerations. He will ever regard the acquisition and expansion of natural knowledge—the unravelling of the complex web of nature by the disciplined intellect of man—as his noblest end, and not as a means to any other end. And what has been the upshot of science thus pursued? Why, that the investigator has over and over again tapped springs of practical power which otherwise he would never have reached. Illustrations are here manifold. I might point to the industries which affiliate themselves with Faraday's discovery of benzol, and with his discovery of the laws of electrolysis. But I need not go further than the fact that in this our day a noble and powerful profession has been called into existence by his discovery of magneto electricity. The electric lamps which mildly illuminate our rooms, the foci which flood with light of solar brilliancy our railway-stations and public halls, can all be traced back to an ancestral spark so small as to be barely visible. With impatient ardour Faraday refused to pause in his quest of principles to intensify his spark. That work he deliberately left to others, confidently predicting that it would be accomplished. And, prompted by motives both natural and laudable, but which had never the slightest influence on Faraday, others have developed his spark into the splendours which now shine in our midst.

It would be a handsome Jubilee present, if it were a possible one, to roll up the career of Faraday into portable form, and to offer it to the Queen as the achievement of one of Her Majesty's most devoted subjects during her own reign. Faraday's series of great discoveries, however, began in 1831, which throws his work five or six years too far back. During the rest of his fruitful life he was a loyal son of the Victorian epoch. But, passing beyond the limitations of the individual, what is science, as a whole, able to offer, on the golden wedding of the Queen with her people? A present of the principle of gravitation—a handing over to Her Majesty of the bit and bridle whereby the compelling intellect of Newton brought the solar system under the yoke of physical law—would surely be a handsome offering. I mention this case of known and conspicuous grandeur, in order to fi[x] the value of another generalization which the science of her reign *can* proudly offer to the Queen. Quite fit to take rank with the principle of Gravitation—more momentous if that be possible—is that law of Conservation which combines the energies of the material universe into an organic whole; that law which enables the eye of science to follow the flying shuttles of the universal power, as it weaves what the Earth Spirit in "Faust" calls "the living garment of God." This, then, is the largest flower of the garland which the science of the last fifty years is able to offer to the Queen.

The second generalization is like unto the first in point of importance, though very unlike as regards its reception by the world. For whereas the principle of Conservation, with all its far-reaching, and, from some points of view, tremendous implications, slid quietly into acceptance, its successor evoked the thunder-peals which it is said always accompany the marriage of thought and fact. For a long time the scent of danger was in the air. But the evil odour has passed away; the air is fresher than before; it fills our lungs and purifies our blood, and science, in its Jubilee offering to the Queen, is able to add to the law of Conservation the principle of Evolution.

In connexion with these victories of the scientific intellect, I have mentioned neither persons nor nationalities, holding, as Davy expressed it, when the Copley Medal was awarded to Arago, that "science, like Nature, to which it belongs, is neither limited by time nor space. It belongs to the world, and is of no country and no age.

Still, it will not be counted Chauvinism if I say that in the establishment of these two great generalizations Her Majesty's subjects have quitted themselves like men. With regard to a third generalization, neither England nor Germany has been idle. Omitting the name of many a noble worker in both countries, the antiseptic system of surgery assuredly counts for something in the civilized world. And yet it is but a branch of a larger generalization, of momentous import, which in our day has been extended and consolidated to an amazing degree by a Gallic investigator. To some, however, any flower culled in this garden will be without odour. Let me therefore add a sweet-scented violet under the name of spectrum analysis which, besides revealing new elements in matter, enables the human worker to stretch forth his hand to sun and stars, to bring samples of them, as it were, into his laboratory, and to tell us, with certainty, whereof they are composed. Surely all these, and other discoveries of high importance, taken and bound together, form an intellectual wreath, not unworthy of Her Majesty's acceptance in her Jubilee year.

A short time ago an illustrious party leader summed up the political progress of the Queen's reign. What I have said will, I trust, show that the intellectual world is not entirely compounded of party politics—that there is a band of workers scattered over the earth whose arena is the laboratory rather than the platform, and who noiselessly produce results as likely to endure, and as likely to influence for good the future of humanity, as the more clamorous performances of the politician.

One word more. On the continent of Europe, kings had been the nursing fathers, and queens the nursing mothers, of science; while Republican Governments were not a whit behind in the liberality of their subventions to scientific education. In England we had nothing of this kind, and to establish an equivalent state of things we had to appeal, not to the Government, but to the people. They have been roused by making the most recondite discoveries of science the property of the community at large. And as a result of this stirring of the national pulse—this development of self-reliance—we see schools, colleges, and universities now rising in our midst, which promise by and by to rival those of Germany in number and importance.

It is time that I should cease. But before doing so, I would ask—as they do in the House of Commons—permission to say a word in personal explanation. I have climbed some difficult mountains in my time, and after strenuous effort for a dozen hours or more, upon ice, rock, and snow, I have not unfrequently reached the top. I question whether there is a joy on earth more exhilarating than that of the mountaineer, who, having achieved his object, is able to afford himself, upon the summit, a foaming bumper of champagne. But, my lords and gentlemen, the hardest climb, by far, that I have ever accomplished, was that from the banks of the Barrow to the banks of the Thames—from the modest Irish roof under which I was born to Willis's Rooms. Here I have reached my mountain-top, and you—God bless you!—have given me a bumper which no scientific climber ever before enjoyed.

Sir Frederick Pollock, in proposing the toast of "Literature and Art," said that on most occasions similar to the present one this toast was a triple one, and included the three sisters—Science, Literature, and Art. But this evening they were assembled together to do homage to science, in the person of one of its most distinguished votaries, and for the time the room in which they had met became a temple of science. In such a temple the principal figure, standing upon the pedestal appropriated to the presiding goddess, must be that of Science, and to her due rites had been already rendered. But for the sisters Literature and Art room must be found also in the sacred edifice; they too must have their altars and their shrines. He pointed out that the highest powers of the imagination were required by the man of science, as well as by the poet and the painter, and instanced the prediction by Fresnel of the bright spot in the centre of the shadow of a disk; and the suggestion made to Goethe of his theory of the development of the vertebrate skeleton, by his accidental observation of the scattered fragments of the deer's skull lying in his path. He adduced the names of Aristotle, Bacon, and other great men who had connected literature with science; and instanced Leonardo da Vinci, and Sir Christopher Wren, one of the founders of the Royal Society, as linking together science and art. He accordingly had great pleasure in submitting for acceptance "Literature and Art," coupling with it the name of Lord Lytton, who was not only a distinguished representative of modern literature, but had also a distinct hereditary claim to represent that of the last generation; and Sir Frederick Leighton, the distinguished President of the Royal Academy.

The Earl of Lytton,—In returning thanks for "Literature" upon an occasion when we are all met to honour science in the person of one of its most illustrious adepts, I cannot but forcibly remember that we are living in an age when inquiry is more active and more widespread than conviction, and it is natural that in minds of the highest order under these conditions even the imaginative faculty should be more powerfully attracted to scientific research than to purely literary production. But inquiry, I think, would be very sterile if conviction in some form or another were not the ultimate fruit of it, and I think that for a period of really vigorous, creative, imaginative art we must look forward in the course of scientific research to some such general re-settlement of ideas upon the basis of a common conviction—which is not now, perhaps, altogether attainable—as may enable art, instead of representing, as it does now, merely the mental attitude of the individual poet or the individual painter, once more to become the universally spontaneous and universally recognized imaginative expression of ideas and emotions which are common to a whole generation or a whole community. If that is the case, if science is ultimately to render this great service to literature and art, surely in the meanwhile we cannot but gratefully appreciate the literary labours of those men of science who in our own and in other countries are promoting or have promoted this result, not only as original discoverers but also as popular and powerful interpreters of scientific fact, and who in this latter capacity have already enriched contemporary literature with writings of rare literary value. If, instead of returning thanks for literature, I were permitted to return thanks on behalf of literature to those writers who have powerfully influenced my own generation, not only by thoughts which stimulate and instruct the intellect, but also by words which stir and elevate the heart, then assuredly I should ask leave to mention some distinguished names which occupy in the field of literature a position only second to the high rank they hold in the hierarchy of science; and foremost among those names I should not hesitate to mention with a special personal gratitude the name of the illustrious man who is the honoured guest of this great assembly to-night. I cannot say it is as a student of science that I myself have studied the writings of Prof. Tyndall, but this I can say, and most truly, that those writings have been to me, from a very early period of my life, companions so cherished that I learnt to look upon their writer as a dear personal friend and benefactor long before it was my privilege to be admitted to his personal intimacy. I believe that scientific research has succeeded in establishing on a physiological basis certain evidences of intelligence even among oysters; and certainly there is, I think, one form of intelligence which is conspicuously displayed by the oyster which might perhaps be cultivated with advantage by after-dinner speakers in my position. The oyster knows when to shut up. Admonished by that very interesting and suggestive fact in natural history, what little else I have to say upon behalf of literature I shall confine to the expression of a hope that the well-deserved relaxation from his more systematic scientific labours in connexion with the Royal Institution may enable my valued and honoured friend Prof. Tyndall to enjoy an increased leisure for the continued cultivation of that department of literature which has already been so richly adorned by his admirable writings.

Sir F. Leighton, who was to have responded for "Art," had been obliged to leave before this stage of the proceedings in order to receive Royal visitors at the Academy.

Sir Lyon Playfair, M.P., proposed the next toast, "The Public Services in Relation to Science." He said that undoubtedly the public services were intimately connected with science and were profoundly affected by its progress, but, unfortunately, the truth was only beginning to be recognized in this country. In the United States scientific men were attached to all public offices, but in this country the attachment was of the loosest possible character. Nevertheless, science had undoubtedly affected our public services in the most profound way. The telegraph had altered the whole system of commerce and also the methods and the powers of government. There was to be a great naval review next month; it would be interesting to imagine Elizabeth's thirty small ships, which conquered the Armada, sailing through two miles of modern ironclads. The largest piece of ordnance used in the Crimean War cost less than a single shot fired from the huge guns of our ironclads. But it was in peace rather than in war that science rejoiced in aiding government. A strong feeling was arising that we must improve our intellectual position as a nation, and this at last was being recognized by the Government. A material index of progressive civilization had always been desired. Liebig contended that the best index of civilization was the quantity of soap consumed. When the Queen ascended the throne we consumed per head $7\frac{3}{4}$ pounds of soap, and now we use 10 pounds per head. The consumption of paper was a more reliable index. At the commencement of the Queen's reign the consumption was $1\frac{1}{2}$ pound of paper yearly; now it was 12 pounds; while in the United States it was 10 pounds, in Germany 9 pounds, in France 8 pounds, and in Italy 4 pounds. But the main question was whether we were developing the national intellect at the same rate as other nations. Our general intelligence is still high, but our trained scientific intelligence is low. Our secondary education in all matters relating to science was far behind that of the United States, Germany, and France. Neither the Government nor the people governed could go on in simple faith on our practical aptitudes by relying on a blind and vain empiricism, like a tree severed from its roots.

The Earl of Derby,—My Lords and Gentlemen: You have asked me to return thanks on behalf of the public services in connexion with science, and Sir L. Playfair, in relation to that toast, has referred to the increased consumption of soap in this country. I have attended a good many public dinners, and I must say that the expenditure of what is vulgarly called soft soap has been great this evening. I am sincerely grateful to him for the quantity of that article which it has pleased him to expend upon me. But really the toast is one which hardly any man is competent to do justice to, and certainly not one who like myself has no connexion with science, except a sincere admiration and respect for its professors, and whose connexion with the public service has only been that of a Parliamentary chief. Under our system the Parliamentary head of a department is mainly concerned to keep it in harmony with the House of Commons and with the public. He has to warn the permanent officials that something that is done, or something that is left undone, or proposed to be left undone, is what public opinion will resent; and, on the other hand, he has to tell outsiders that the things they ask him and press him to do are things unwise or impossible from an administrative point of view. That is useful; it is certainly laborious, and it is often a difficult function; but it does not involve much more scientific knowledge than is implied in driving a cab through a crowded street. It does require some knowledge of men, but that is a department of study to which, as yet, no scientific formula has been found to apply. Sir L. Playfair told us, and I was sorry to hear it, of the loose connexions which exist between science and the Government. I can only say that I am entirely ignorant of any such immoral transactions. But if the departments were better represented here and if they could speak for themselves, I am sure that they would not be backward in acknowledging their obligations to science. The Treasury would tell you that those useful though sometimes ungraceful coins in which our dinner is paid for would not circulate through Europe as they do if they had not been subjected to a careful and complicated process, requiring scientific knowledge. The Excise might tell you, if they chose, of the frauds that might be perpetrated upon the revenue and the public if it were not for the careful and scientific examination of all taxable articles. The Post Office would find no difficulty in acknowledging its obligations to Watt and to Stephenson—for where would postal revenue be without railways?—and in later days to investigators whose researches made the telegraph possible. But the fighting departments, or the spending departments, which is their more common name in Downing Street, would have the most to return thanks for. They would point to the modern ironclad, the most elaborate, the most complete, and the most costly, of all contrivances in which the art of construction has been utilized for purposes of destruction. They would tell you how the chemist, metallurgist, the engineer, the electrician, the mathematician, have all contributed their share to that extraordinary result of science and skill. The War Office would follow the Admiralty. They would not say, as Frederick of Prussia did, that Providence is on the side of the biggest battalions, but they might possibly say that Providence was generally on the side of the army which could bring into the field the most scientifically effective weapon in the hands of the most carefully-trained soldier. If I were to turn to the line of business with which I had once something to do, I might ask any diplomatist or any statesman to explain to you how largely the position of Egypt, and, with that, the diplomacy of Europe, has been affected by that little scratch which the genius of M. de Lesseps drew across the Egyptian sands; and if, as is quite possible, the coal-carrying power of steamers and their speed and their economy are largely increased—I do not speak of those wilder predictions according to which steam is to be superseded as the motor power by something more efficient—suppose I say the large increase of the coal-carrying power of steamers, and the results to which I have referred may be again reversed; and again, at least in war time, the route to India may lie through the South African seas. If I speak of the colonies, everyone conversant with that department would admit that if we had had the ocean telegraph in existence twenty-five years ago half our little wars beyond the seas would never have taken place, and those that have taken place would have been disposed of in half the time. I know that these things are common-place, but I cannot help that. If I could tell you what the next great discovery was going to be, that would not be common-place. But, unfortunately, that is not in my power; and if it were I do not think I should be in a hurry about it, because I have observed that those who are the first to announce a discovery are generally rewarded by having a remarkably unpleasant time. But however great may be the gains which we have derived from the applications of science, they are nothing as compared with those which will and do accrue to us from the acceptance of scientific habits of thought. That is coming already, and it will come more in a not remote future. We have many things in this age and country of which we cannot boast, but we may boast that in science England has done something more than hold her own. The great name of Darwin will survive, it may be, the British Empire itself, and with him will be remembered some others also, whom to single out might perhaps be invidious. But we may be sure of this, that among their names will be included the name of our distinguished guest of to-night. It is a common complaint that politicians have done nothing for science. In that I do not agree. They have done the best they could for it—they have let it alone; they have not corrupted it by their intrigues, nor vulgarized it by their squabbles; and they being what they are, and science being what it is, that is probably the best service they could have rendered it.

Lord Rayleigh proposed "The Health of the Chairman."

Prof. Stokes briefly responded, and the company, which numbered nearly two hundred, separated.

THE ELEVEN-YEAR PERIODICAL FLUCTUATION OF THE CARNATIC RAINFALL.

MORE than fourteen years ago, in the pages of NATURE, Mr. Norman Lockyer first drew attention to an apparent periodical variation of the rainfall registered at the Madras Observatory; which seemed to be such that it reached a maximum and a minimum alternately, at about the same epochs as the corresponding phases of the sunspot frequency. The idea, once started, was followed up by others, among whom perhaps the best known is Dr. (now Sir) W. W. Hunter, whose pamphlet on the subject, without laying claim to any originality as regards its subject-matter, attracted very general attention by the charm of its style, and also by its attempt to identify the periodical occurrence of famines in Southern India with the epochs of minimum rainfall shown by the Madras registers.

When, however, the data on which these speculations were based came to be critically examined, the general verdict of men of science was that the conclusions were "not proven." This was certainly my own opinion; and General R. Strachey, in a lecture delivered before the Royal Institution in 1877, and, at greater length, in a paper communicated to the Royal Society in May of the same year, showed that any attempt to educe a true cyclical variation from the recorded figures, ended in a negative result. Admitting that when the annual quantities were tabulated in eleven-year cycles, the means of the homologous terms seemed to indicate a period of maximum between the third and seventh years, and of a minimum between the eighth and second years, he found that, when the mean difference of the individual years from the supposed periodical means was compared with the mean difference of the former from the arithmetical mean of the whole series, the results differed but little.

It was further shown by myself that the supposed connexion between the periodicity of the Madras (Observatory) rainfall and that of famines in Southern India was by no means so intimate as might appear at first sight. The famines in question had occurred sometimes in one part of the peninsula, sometimes in another, by no means always in the country around Madras; but no other station in the peninsula (of those then available for the inquiry) showed even such an approach to a periodical variation of the rainfall as did the Madras Observatory.

At this stage matters have since remained, with the exception that, in 1879, an apparent periodical fluctuation of a very different character was brought to notice by Messrs. Hill and Archibald in the winter rainfall of Northern India. This, which has an interest of its own, I shall not further discuss at present.

In the course of a general investigation of the rainfall of India, the first part of which only has been as yet published ("Indian Meteorological Memoirs," vol. iii. part 1), I have lately had occasion to reconsider these old questions, and to re-examine them by the light of the accumulated data of the last twenty-two years. For convenience of discussion, I have divided India and Burmah into twenty-four rainfall provinces, one of which is the Carnatic.

This consists of the plain below the Eastern Gháts, occupying the south-east of the peninsula, and extending from Cape Comorin to the mouths of the Kistna. Its area may be taken as 72,000 square miles. The town of Madras is situated nearly midway on the sea-coast of this province, and is a fairly representative station; but, in addition to the rainfall registers of the Madras Observatory, I have those of thirty-nine other stations, pretty equally distributed through the province; most of them extending back to 1864. The Carnatic is distinguished by one important peculiarity in the season of its chief rainfall. During the spring months, it receives a certain amount of rain, in common with the southern and eastern provinces of India generally; but while the heavy summer rains are falling in Central and Northern India, and also on the west coast of the peninsula, the Carnatic is but little affected by them. In its southern districts, indeed, the rainfall of June and July is less than that of May; and it is not until the rains are over in North-Western India, viz. in October and November, that this province receives the chief and heaviest rainfall of the year. Hence the vicissitudes of the rainfall of the summer months, which are all important in Central and Northern India, are relatively less important in the Carnatic, even if they affect that province in the same manner as Northern India—and this is far from being always the case—and as a final result the annual fluctuation of the Carnatic rainfall often differs widely from that of other provinces in the peninsula.

The mean annual rainfall of the Carnatic may be taken in round figures at 35 inches, which is about 7 inches less than the general average of the whole of India. The following table gives the annual variation from this average for the twenty-two years 1864–85, which results when the annual total fall of each individual station is compared with its local average, and the mean of all the differences taken for each year.

Annual mean rainfall variation of the Carnatic rainfall.

	Inches.		Inches.
1864 ...	− 5·0	1875 ...	− 5·2
1865 ...	− 5·0	1876 ...	− 13·2
1866 ...	− 4·0	1877 ...	+ 8·3
1867 ...	− 9·4	1878 ...	0
1868 ...	− 4·6	1879 ...	+ 2·3
1869 ...	− 0·3	1880 ...	+ 7·0
1870 ...	+ 1·8	1881 ...	− 2·1
1871 ...	+ 5·5	1882 ...	+ 4·4
1872 ...	+ 11·5	1883 ...	+ 5·2
1873 ...	− 0·1	1884 ...	+ 11·6
1874 ...	+ 7·3	1885 ...	− 1·1

During the first thirteen years (with the exception of 1873) the fluctuation, here shown, is remarkably distinct and regular. The rainfall reached a minimum in 1867, then rose steadily to a maximum in 1872, and after a drop in 1873, and partial recovery in the following year, fell rapidly to a second minimum in 1876. From 1877 to 1881 it oscillated considerably, but thereafter rose again steadily to a second maximum in 1884, dropping again in 1885 to something below the average. Thus we have, apparently, two complete cycles in the twenty-two years; the first remarkably regular, the second less so, but with the periodical fluctuation still dominant.

In order to ascertain with somewhat greater precision the probable character of this periodical fluctuation in an eleven-year cycle, the coefficients of the first two periodical terms of the harmonic formula have been computed, taking 1864 as the initial epoch. These coefficients are—

$$u' = 5{\cdot}340 \text{ inches.} \qquad u'' = 2{\cdot}873 \text{ inches.}$$
$$U' = 206^\circ\ 29' \qquad U'' = 247^\circ\ 15'$$

and the values of the eleven annual phases of the cycle thus found are—

			Inches.
1864	and	1875	− 5·1
1865	,,	1876	− 6·7
1866	,,	1877	− 4·4
1867	,,	1878	− 1·5
1868	,,	1879	− 0·6
1869	,,	1880	− 0·7
1870	,,	1881	+ 0·8
1871	,,	1882	+ 4·4
1872	,,	1883	+ 7·3
1873	,,	1884	+ 5·9
1874	,,	1885	+ 0·5

Taking the differences of these values from the recorded rainfall of each of the twenty-two years, the mean deviation of the latter in any one year from its periodical value is found to be—

$$\pm\ 3{\cdot}5 \text{ inches,}$$

which is only one-fourth of the range of the periodical variation as above determined; and the probable error ϵ, of the periodical value, as found by the formula—

$$\epsilon = 0{\cdot}6745 \sqrt{\frac{\Sigma\,(v^2)}{n(n-1)}},$$

is $\pm$ 0·70 inch.

On the other hand, the mean deviation of a single year from the general average is

$$\pm\ 5{\cdot}2 \text{ inches,}$$

and the probable error of that average $\pm$ 0·94 inch.

What, then, is the numerical probability of the cyclical variation, thus determined, being a true periodical fluctuation, representing a regularly recurrent phenomenon? As a general problem this cannot be solved, because we do not know all the variations to which the rainfall may conceivably be subject. But we can compare the relative probability of this particular variation being the result of a periodic law, and of its being a mere fortuitous series of variations from a constant average. That it is the most probable variation, having the assumed period of eleven years (with the exception of such as might be computed from a larger number of periodic terms), is assured by the method of its computation, which is based on that of least squares; and one may assume that this relative probability for a single year is represented by the inverse ratio of the probable errors of the two means above determined, viz.—

$$\frac{0{\cdot}94}{0{\cdot}70}.$$

This ratio of probability increases in geometrical progression, as the number of years during which it is found to hold good increases in arithmetical progression[1]; and, for twenty-two years, becomes—

$$\left(\frac{0{\cdot}94}{0{\cdot}70}\right)^{22} = 655 : 1.$$

This ratio, although by no means amounting to demonstration of the exact validity of this particular cycle, affords at least a very high probability that the apparent undecennial fluctuation is no chance phenomenon.

Apart from the approximate identity of its period, the oscillation of the rainfall, thus disclosed, is very different in character from that of the sunspot curve. The periodical minima of both rainfall cycles preceded those of the corresponding sunspot cycles by two years; the actual year of minimum rainfall coincided with that of sunspot minimum in the first cycle, and preceded it by two years in the second. The periodical maximum of the first cycle followed the sunspot maximum by two years, that of the second cycle coincided with the corresponding phase of sunspots, which, in this case, was retarded by two years. The actual rainfall maximum occurred two years later than the sunspot maximum in the first cycle, and one year later in the second.

Hence, as far as the evidence of two cycles goes, the minimum of the rainfall tends to precede the minimum of the sunspots, the maximum of the former to follow that of the latter; and it is noteworthy, as I shall afterwards show, that the droughts which, during the last century, have visited with more or less intensity certain portions of the Indian peninsula, have, on an average, preceded years of sunspot minimum by about one year.

In the other provinces of tropical India, an eleven-year cycle is hardly, if at all, to be detected; a conclusion fully in accord with that which I drew, in 1877, from an examination of the rainfall registers of Bangalore, Mysore, Bombay, Nagpur, &c. The more pronounced phases of the Carnatic cycle are indeed reproduced as a rule, more or less distinctly, as seasons of high or low rainfall respectively, in most parts of the peninsula; but some of the intermediate years are characterized by vicissitudes as great, and even greater than these, destroying the appearance of anything like regular fluctuation.

The Carnatic minimum of 1867, which was the culmination of five years' (1864–68) deficient rainfall, was represented also in Mysore and Bellary, in Malabar and the Deccan; but, in the last two of these provinces, 1866 had a still lower rainfall: and in Berar and Khandesh, while the deficiency of 1866 was (relatively to the average) greater than in any of the more southern provinces, that of 1867 was above the average. In the Konkan again, there was no very great deficiency before 1871, and this was shared more or less by the whole of the peninsula, excepting only the Carnatic and Malabar, which had an excess of 16 and 13 per cent. respectively.

The Carnatic maximum of 1872 was reproduced in Orissa and the Northern Circars—that is to say, in all the eastern provinces of the peninsula—and also in Berar and Khandesh; but in other parts of the peninsula the rainfall of this year differed but little from the average. 1874, however, was a year of excessive rainfall in all the western and southern provinces of the peninsula.

The great drought of 1876 (the second Carnatic minimum) extended with even greater intensity to Mysore, Bellary, Hyderabad, and the Deccan districts of Bombay, and affected more or less the whole of the peninsula, and, in addition, a great part of extra-tropical India. But in the Konkan and Malabar the deficiency was only 18 per cent. of an average fall. In the Konkan the deficiency of the following year was much greater; and in the northern provinces of Bombay, as well as in the greater part of North-Western India, the summer rainfall of 1877 failed almost completely; whereas in the Carnatic the rainfall of that year was remarkably copious.

[1] The probability of throwing any given series of numbers of a single die, in any prescribed order, repeatedly for n throws, is obviously the same as that of throwing a single given number n times in succession, viz. $\left(\frac{1}{6}\right)^n$; and the probability of throwing, in like manner, one out of a given series of dyads or triads, the dyads or triads varying in any prescribed order is $\left(\frac{2}{6}\right)^n$ or $\left(\frac{3}{6}\right)^n$. The relative probability of the dyad to the triad series is $\left(\frac{2}{3}\right)^n$; and generally the relative probability of a phenomenon, the law of variation of which is unknown, varying n times in succession, between limits $\pm\ p$ and $\pm\ (p + 2)$ respectively, is—

$$\left(\frac{p}{p + x}\right)^n.$$

Similar reasoning holds good when p and $p + x$ are the measures of the *mean* variation; and also when, as in the case before us, they represent the probable errors of alternative averages. Finally, the relative improbability of the more limited range, as a chance result—in other words, the probability of the limitation being the result of a regulating cause—is expressed by the inverse ratio.

The following year, 1878, was one of remarkably copious rainfall in nearly all parts of the peninsula, with the exception of the Carnatic, where the rainfall did not exceed the average. In Hyderabad it was greater than that of any other year since regular registers have been kept; and, on the general average of the peninsula (excluding the Carnatic), it is approached only by that of 1874 and 1882.

Finally, the Carnatic maximum of 1884 coincided with a small excess in Hyderabad and with a larger excess in the north-west of the peninsula (the Central Provinces, Berar, Khandesh, the Konkan, and Guzerat); but this was due to independent conditions. In Mysore, Bellary, Malabar, the Deccan, the Northern Circars, and Orissa, the rainfall of the year was more or less deficient, especially in Mysore, where the fall was only three-fourths of the average.

It may, then, be considered as demonstrated that the apparently periodical variation of the Carnatic rainfall is by no means representative of a similar variation in that of Southern India generally; and I might here conclude the discussion, were it not that the independent evidence of a certain apparent regularity in the recurrence of droughts and dearths seems to require a few words of notice.

At page 21 of the Report of the Indian Famine Commissioners is given a list of all the serious droughts, and consequent seasons of dearth, that have affected India during the last century. Selecting those that have chiefly affected some part of the peninsula, we have the following:—

Droughts.	Intervals.
1782	
1791	9 years.
1802	11 "
1806	4 "
1812	6 "
1823	11 "
1832	9 "
1844	12 "
1853	9 "
1865	11 "
1876	11 "

Omitting that of 1806, which divided the ordinary interval into two, the mean interval is 10·36 years, and the deviation from this mean in no case amounts to two years. According to Wolf's table, the years of minimum sunspots and their intervals were:—

Sunspot Minima.	Intervals.
1784	
1798	14 years.
1810	12 "
1823	13 "
1833	10 "
1843	10 "
1856	13 "
1867	11 "
1878	11 "

the mean interval being 11·18 years. The coincidence of these mean intervals is hardly so close as might be anticipated were there any real physical interdependence between recurrent phases of the sun's condition, and the recurrence of the droughts. And a comparison of the dates in detail brings to light further discrepancies. Thus the years of drought vary in their relations to the nearest years of minimum sunspots as follows:—

One, midway between two sunspots minima; seven years distant from each;
One, four years earlier;
One, three years earlier;
Three, two years earlier;
One, one year earlier;
One, coincident;
One, one year later;
One, two years later;
One, four years later.

Omitting the first (that of 1791), which occurred four years after a year of maximum sunspots, and midway between two minima, in an unusually prolonged cycle, the years of drought, on a general average, anticipated the sunspot minima by somewhat less than a year, instead of following the minima, as might have been expected on the hypothesis of the former standing to the quiescent condition of the sun in the relation of effect to cause.

I should not, however, hastily conclude from these facts that there is no relation between the recurrence of drought in Southern India, and the periodical variation of the solar photosphere; but merely that the interdependence of the two classes of phenomena, if real, is far from being simple and direct, and also that other and, as far as we know, non-periodic causes, concur largely in producing drought. If we accept the conclusions, drawn in the first part of this note, as to the highly probable periodicity of the Carnatic rainfall, one must admit that there is, in that province, a recurrent tendency to drought at eleven-year intervals, though it does not always culminate in drought of disastrous intensity; and this epoch anticipates by about two years that of the sunspot minimum. This tendency is evidently much weaker in other parts of the peninsula; and in Northern India there is some indication of a tendency to the recurrence of drought about the time of maximum sunspots, as in 1803, 1837, 1838, and 1860—all years of disastrous drought in Northern India; and the experience of late years has demonstrated that these droughts generally extend to the northern provinces of the peninsula.

HENRY F. BLANFORD.

NOTES.

WE print elsewhere a report of the speeches delivered by Mr. Goschen and by some members of the influential deputation who waited upon him last Thursday to press the claims of University Colleges. The deputation had certainly no reason to complain of the manner in which they were received. Mr. Goschen, speaking as Chancellor of the Exchequer, was of course obliged to adopt a cautious tone; but it was plain enough that those who addressed him represented a cause with which he had strong personal sympathy. His promise that the Government would give the matter "its most serious attention," means, we may hope, that the principle of State aid for University Colleges has been practically accepted.

ON Monday the foundation-stone of the Imperial Institute was laid by the Queen. No representative of science, as such, was invited to be present at the ceremony, and NATURE did not receive a Press ticket. Evidently science is to have little to do with the New Institute.

THE Prussian Society for the Promotion of Industry has recently offered a prize of about £150 for the most exhaustive critical comparison of all kinds of existing bronze, tombac, and brass alloys, used or recommended for machinery, giving their chief properties with regard to resistance, ductility, friction at different temperatures, malleability, electrical conductivity, behaviour with acids, hydrogen and carbon sulphides, chlorine, and other strongly corrosive substances met with in practice. The same Society also offers a gold medal and £250 for the best work on light and heat radiation of burning gases. The time limit in the former case is the end of 1887; in the latter, the end of 1888.

The National Association for the Promotion of Technical Education has now been formed. A meeting of persons interested in the movement was held on the 1st inst. at the rooms of the Society of Arts, Adelphi. Lord Hartington presided, and among those present were Lord Rosebery, Mr. John Morley, Sir Lyon Playfair, Sir John Lubbock, and representatives from Colleges, technical schools, trade-unions, School Boards, national Societies, and Chambers of Commerce.

About 40 members of Parliament were also present. Lord Hartington, in opening the proceedings, said their object was not so much to stimulate public interest in this great question as to consider from a practical point of view the channels into which such interest ought to be directed. He had been struck by the facts relating to technical education at home and abroad which had been presented in very voluminous form to the public in the reports of our Consuls. We had in this country attained to a great industrial and technical supremacy in the world. We had attained this position partly by the possession of great resources in coal and iron and other industrial materials, partly from the character, energy, and industry of our people, and partly—and here he might be trenching upon controversial grounds—from the fact of our having adopted a sound commercial policy. At the same time, concurrently with our attainment of this supremacy, wonderful scientific discoveries had been made, and more and more science was being applied to the industrial occupations of the world. Other nations had been quick to perceive this, and were striving to make their position equal to ours by developing at immense cost to the State and public funds that scientific instruction which would enable their manufactories and workmen to compete successfully with ours. If we were passive in the matter—if we were indolent—it was conceivable not only that foreign nations would rival us, but they might also succeed in passing us, with consequences which it would be difficult to contemplate. If we were satisfied to go on as we were, if we were content to rely in the future as we had done in the past on those advantages which had given us our present position, and if we did not think it necessary to organize more completely our system of technical instruction than at present, that decision should be the result of deliberate and well-formed consideration and not the result of apathy or indolence. Sir Lyon Playfair, moved that the Association be formed, that Lord Hartington be invited to become President, and the following gentlemen Vice-Presidents:—Lord Granville, Lord Ripon, Lord Rosebery, Lord Spencer, the Bishop of London, Mr. Broadhurst, Prof. Huxley, Sir John Lubbock, Mr. Mundella, Sir Lyon Playfair, Sir B. Samuelson, Prof. Stuart, Dr. Sullivan, Sir R. Temple, and Prof. Tyndall. Mr. John Morley, in seconding the motion, said the time for further inquiry had gone past, and the time had arrived when they could no longer with wisdom, or even with safety, delay the movement they that day commenced. The resolution was carried unanimously. Sir J. Lubbock moved the appointment of an executive Committee, which was carried; as was a motion, made by Mr. Mundella and seconded by Lord Rosebery, that those present be invited to join the Council. A discussion ensued on the proposed objects of the Association, after which Sir B. Samuelson moved, and Mr. Howell seconded, a resolution inviting the assistance of large towns and the chief industrial centres. The motion was duly carried, and votes of thanks closed the proceedings.

In his statement on Monday about the progress of business in the House of Commons, Mr. W. H. Smith said: "There is a measure for promoting technical education, which we have every reason to believe will be accepted unanimously by the House—at all events, we hope that a very slight discussion will be sufficient to pass that measure into law."

On July 25, 1837, the first practical essay in telegraph working was made by Messrs. Cooke and Wheatstone between Euston and Camden Town. In the material order of things few more magnificent triumphs have ever been achieved, and it has very properly been decided that the fiftieth anniversary of the occasion shall be celebrated. Some time ago an influential Committee was formed to take the matter into consideration, and the other day there was a well-attended meeting of the members at the offices of the Society of Telegraph-Engineers and Electricians. Mr. Preece, F.R.S., was appointed chairman. In his opening speech he said they had met to make arrangements for a dinner which was to be given in celebration of the jubilee of the telegraph. It was the success of the essay made by Messrs. Cooke and Wheatstone that led to the association of Robert Stephenson, George Parker Bidder, Brunel, and other well-known men in those days, with the telegraph, and from that little beginning they had seen how the telegraphs had spread all over the face of the earth. In England, where the first step was taken, they had succeeded in keeping well in the van, and it was only fitting that such an important event, probably the greatest event that had happened during the long reign of Her Majesty, should be celebrated, and that those who had been instrumental in bringing telegraphy to its present great position should meet together and talk over old times. It so happened that there were several reasons why the celebration should not take place on July 25, which was really the proper day. In the first place, Mondays were busy days with legislators in their House over the way, and it would be extremely difficult to get many of those whom they hoped to attract if the proposed dinner took place on a Monday; again, it was quite impossible on a Government night, like Monday, to get the Postmaster-General, who, it was thought, should take the chair, to attend; and, further, on July 23 there was to be a great naval review, and a great many who would wish to attend the dinner would not be able to get back until late on Monday afternoon. For those reasons it would be difficult to hold the dinner on the 25th, and Wednesday, the 27th, had been suggested as meeting everybody's convenience. He therefore moved that a dinner be held on July 27 to celebrate the jubilee of the telegraph. Mr. Willoughby Smith seconded the motion, which was unanimously agreed to. Discussion followed respecting matters of detail, and an Executive Committee was elected, consisting of Messrs. W. H. Preece, E. Graves, C. H. B. Patey, C. E. Spagnoletti, A. Siemens, and A. Stroh, with Messrs. H. Alabaster and C. H. W. Biggs as Honorary Secretaries, and Mr. F. H. Webb, Secretary of the Society of Telegraph-Engineers and Electricians, as Acting Secretary. The guarantee fund was at once opened, and names were soon down for upwards of £100. The meeting was adjourned until Tuesday, the 12th inst., when the Executive Committee will report as to the progress of the arrangements.

A number of vacation courses in Natural Science will shortly be started in Edinburgh. During the present summer there will be two courses—one on Practical Botany, conducted at the Royal Botanic Gardens of Edinburgh by Mr. G. F. Scott Elliot, an assistant to the Professor of Botany; another on Practical Zoology, conducted at the Scottish Marine Station, Granton, Edinburgh, by Mr. J. Arthur Thomson, Lecturer on Zoology in the School of Medicine, with the co-operation of Mr. J. T. Cunningham, the Superintendent of the Station. These courses ought to be of great service to teachers and others occupied during the University terms, for whom they are primarily intended.

Writing to us from Tashkend on June 12, M. Wilkins says that the city of Vernoje was completely ruined by the earthquake of June 9. More than 800 bodies had been excavated. "The disaster," he says, "is beyond description. Mud and water are said to flow abundantly from the disturbed mountains to the scene of the catastrophe, and many crevasses are noticed in the ground. The exact time of the tremendous shock is given as 4h. 35m. local time. At 4h. 18m. (Tashkend local time) on the same morning, we felt here a flat wave which set in motion suspended objects. Taking into account the difference of longitude between Tashkend and Vernoje and the consequent difference of time, it appears that the wave travelled in a straight line more than 400 miles in the short time of 13·5 minutes, crossing on the way, in a diagonal direction, the whole western half of the Thian Shan range."

On June 30 a Philippine Exhibition was opened at Madrid by Queen Christina. The most important exhibits are specimens of the natural products, vegetable and mineral, of the Philippine Islands. Some forty natives, male and female, with their native houses and arms, are present. The late King of Spain started the idea of a permanent Colonial Museum, to contain the mercantile products of the Spanish colonies. The opening of this Exhibition is regarded as the first step towards the realization of his scheme.

Another instalment of his valuable work on high temperature dissociations has just been given forth from the laboratory of Prof. Victor Meyer, at Göttingen. The molecular condition of phosphorus, arsenic, and antimony at the highest accessible temperatures has been the subject of this recent work, and the following are the experimental results obtained. As is well known, the experiments of Deville and Troost brought to light the fact that as high as 1040° in the case of phosphorus, and 860° in the case of arsenic, the observed densities are such as can only be explained on the supposition that the molecules of these elements consist of four atoms. J. Mensching and Victor Meyer now show that as the temperature is gradually raised to a red heat the molecular weights begin to diminish—that is, the four-atom molecules commence to break down—and at a white heat so large a number are dissociated that the values obtained for the vapour-densities approximate to those required on the supposition that the molecules each contain but two atoms. Hence, at a white heat the vapour-densities of phosphorus and arsenic are normal, and the molecules consist of the usual two atoms. In the case of antimony, no thoroughly trustworthy work has hitherto been published as to its molecular state, but it has been generally supposed to consist also of four-atom molecules. Mensching and Meyer, however, find that it behaves quite unlike phosphorus and arsenic, inasmuch as immediately on volatilization its density is found to correspond to a molecule of but three atoms, and although dissociation continues to the limit of terrestrially procurable temperatures, yet when this is attained, the level of the normal state is not reached, and more definite results must perforce be deferred until, by some ingenious device, temperatures far higher are obtainable.

The Royal Meteorological Institute of the Netherlands at Utrecht has recently published its *Jaarboek* for 1886, containing observations taken three times daily at ten places, and daily rainfall values at eighty stations. These volumes, which have now been regularly issued for thirty-eight years, form one of the most complete series of meteorological observations in Europe, and they also contain valuable discussions on the climatology of distant parts. The volume now in question contains observations taken at San Salvador (on the Congo) for 1885, and at Djedda (Arabia Felix), Paramaribo (Dutch Guiana), and Culebra (Panama Canal) for 1886. The Director of this Institution (Dr. Buys Ballot) first enunciated the law that now bears his name, showing the universal relation of the direction of the wind to barometric pressure, which has been so instrumental in popularizing weather knowledge. This Office also deals largely with maritime meteorology, and has published a long series of papers on this subject entitled "Uitkomsten van wetenschap en ervaring," as well as wind-charts for the various oceans.

An unusual number of foreign men of science will be present at the Manchester meeting of the British Association for the Advancement of Science. The following is the first list of foreigners who have accepted invitations to attend the meeting:—*Section A* (*Physics and Mathematics*): Cleveland Abbe, Meteorological Office, Washington; Von Hefner Altneck, Berlin; A. Cornu, Ecole Polytechnique, Paris; A. Crova, Montpellier; J. R. Eastman, U.S. Naval Observatory; W. Foerster, Director of the Berlin Observatory; W. de Fonvielle, Paris; A. Horstman, Heidelberg; F. Kohlrausch, Professor of Physics, Würzburg; A. Kundt, Professor of Physics, Strassburg; William Libbey, Princeton College, N.J.; G. Lippmann, Paris; R. Lipschitz, Professor of Mathematics, Bonn; Malcolm McNeill, Princeton College, N.J.; O. E. Meyer, Breslau; G. Quincke, Professor of Physics, Heidelberg; Schering, Director of the Observatory, Göttingen; Ernst Schroeder, Karlsruhe; J. Violle, Ecole Normale, Paris; E. Warburg, Professor of Physics, Freiburg; H. Wild, St. Petersburg; A. C. Young, Princeton College, N.J. *Section B* (*Chemistry*): A. Bernthsen, Heidelberg; J. W. Brühl, Freiburg; Caro, Mannheim; Le Chatelier, Paris; F. W. Clarke, Washington; De Clermont, Paris; F. B. Fittica, Marburg; R. Fittig, Strassburg; Hempel, Dresden; Reinhardt Hoffman, Biebrich; A. Ladenburg, Kiel; J. W. Langley, University of Michigan; A. Lieben, Vienna; C. Lieberman, Berlin; Oscar Liebreich, Berlin; Lunge, Zurich; J. W. Mallet, University of Virginia; C. A. Martius, Berlin; Mendelejeff, St. Petersburg; Menschutkin, St. Petersburg; Lothar Meyer, Tübingen; Noelting, Muhlhausen; Pauli, Höckst; Silva, Paris; G. Wiedemann, Leipzig; Otto Witt, Berlin; J. Wislicenus, Leipzig. *Section C* (*Geology*): E. Cohen, Greifswald; H. von Dechen, Bonn; Anton Fritsch, Prague; Alfred Nehring, Berlin; Abbé Renard, Bruxelles; F. Zirkel, Leipzig. *Section D* (*Biology*): A. de Bary, Strassburg; Von Boddaert, Cutsem; C. W. Braune, Leipzig; A. Chauveau, Paris; F. Cohn, Breslau; C. Dervalque, Liége; C. Gegenbauer, Heidelberg; Asa Gray, Harvard College, Cambridge, U.S.; W. His, Leipzig; A. Hubrecht, Utrecht; Ch. Julin, Liége; F. Kühne, Heidelberg; Count von Solms Laubach, Göttingen; Lortet, Lyon; Marey, Paris; C. S. Minot, Harvard College; G. S. Morse, Salem, Mass.; P. Preyer, Jena; Pringsheim, Berlin; J. von Sachs, Würzburg; De Saporta, Aix; A. Weismann, Freiburg; R. Wiedersheim, Freiburg. *Section E* (*Geography*): Comodore Jansen, The Hague; M. Lindemann, Bremen; M. Venukoff, Paris. *Section F* (*Economic Science*): Carl Greven, Leyden; Dana Horton; Judge Mackay; A. de Marcoartu, Madrid; Carl Menger, Vienna; *Section G* (*Engineering*): Thos. Egleston, Washington; J. B. Francis, Past President of the American Society of Civil Engineers; A. Gobert, Bruxelles; Quinette de Rochemont, Havre; R. H. Thurston, Sibley College, Cornell University. *Section H* (*Anthropology*): Dr. O. Finsch; Marquis de Nadaillac, Paris.

Messrs. Marcus Ward and Co. will publish, early this autumn, a work, in two volumes, on the Canary Islands. The writer, Mrs. Olivia M. Stone, author of "Norway in June," visited with her husband all the islands of the group—a feat which had never before been accomplished by English people. Illustrations from photographs taken during the tour, and eight maps made from the author's personal observations, will accompany the letterpress.

In a letter printed by us last week, describing a meteor which was seen in West Sussex by daylight, the meteor is said to have "disappeared near the *meridian* of Antares." For "meridian" read "position."

The additions to the Zoological Society's Gardens during the past week include three Blotched Genets (*Genetta tigrina*) from South Africa, presented by Gen. J. J. Bisset; an Ocelot (*Felis pardalis*) from South America, presented by the Earl of Dudley; a Barn Owl (*Strix flammea*), British, presented by Mr. —— Wickham; a White-tailed Sea Eagle (*Haliaetus albicilla*), European, presented by Mr. G. J. Mayer; a Ceylonese Jungle Fowl (*Gallus stanleyi*) from Ceylon, presented by Mr. Hugh Neville; six Corn-Crakes (*Crex pratensis*), British, presented by Mr. G. J. B. Willows; a Magpie (*Pica rustica*) from France, presented by Mr. Walter H. Ince; a Yellow-fronted Amazon

(*Chrysotis ochrocephala*) from Guiana, deposited; six Chinchillas (*Chinchilla lanigera*) from Chili, a Burrowing Owl (*Speotyto cunicularia*) from Buenos Ayres, two Hoopoes (*Upupa epops*), British, a Gould's Monitor (*Varanus gouldi*) from Australia, purchased; two Mule Deer (*Cariacus macrotis*), a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*) born in the Gardens; two Blood-breasted Pigeons (*Phlogœnas cruentata*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

Relative Positions of the Principal Stars in the Pleiades.—We have received vol. i., part 1, of the Transactions of the Astronomical Observatory of Yale University, containing an important paper by Dr. W. L. Elkin, giving the results of his researches with the Yale heliometer on the relative positions of sixty-nine stars situated in the above-mentioned group. The work consists, in reality, of two independent triangulations: one resting on measurements of the distance of each star in the group from each of four stars situated near its outer limits, so that nearly the entire group is inclosed symmetrically by the quadrilateral formed by them; the other resting on measurements of distance and position-angle from Alcyone, the central star of the group. These two independent determinations are in very satisfactory agreement, and Dr. Elkin has thus furnished a most accurate catalogue, for the epoch 1885, of the relative positions in R.A. and declination of these sixty-nine stars. For comparison of his results with the Königsberg places for 1840, Dr. Elkin has adopted the corrections to the latter resulting from Prof. Auwers's researches, and brought up the newly reduced places to 1885, exhibiting the comparison in the form of apparent displacements in R.A. and in declination, the place of Alcyone being made identical in both series. For the six largest cases of relative displacement there is a remarkable community both of direction and amount of apparent motion, and it is remarkable that this general drift is very similar to the reversed absolute motion of Alcyone as given by Newcomb. For two of the stars, Bessel's Nos. 14 and 35, the coincidence is, in Dr. Elkin's opinion, sufficiently close to warrant the deduction that these two stars at least do not belong to, but form only optical members of, the group. It is possible, if not probable, that the other four should also be placed in the same category. The general character of the internal motions of the group appears, however, to be extremely minute, and Dr. Elkin thinks that the hopes of obtaining any clue to the internal mechanism of this cluster seem not likely to be realized in the immediate future. Dr. Elkin also compares his results with the micrometrical measures of M. Wolf at Paris and of Prof. Pritchard at Oxford, and arrives at the conclusion that "the use of the filar micrometer for such large distances as those under consideration is likely to be accompanied with considerable casual error, and, unless great care is taken, with large systematic error. The conclusions of Messrs. Wolf and Pritchard as to the relative motions in the group have thus been unfortunately vitiated, and must be replaced by those formulated" in Dr. Elkin's most able paper.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 JULY 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 July 10

Sun rises, 3h. 57m.; souths, 12h. 5m. 1·7s.; sets, 20h. 13m.; decl. on meridian, 22° 16′ N.: Sidereal Time at Sunset, 15h. 27m.

Moon (at Last Quarter on July 13) rises, 22h. 32m.*; souths, 3h. 51m.; sets, 9h. 19m.; decl. on meridian, 8° 32′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	6 14	13 40	21 6	15 33 N.
Venus	8 10	15 10	22 10	10 57 N.
Mars	2 21	10 42	19 3	23 59 N.
Jupiter	13 8	18 26	23 44	9 4 S.
Saturn	4 34	12 35	20 36	21 16 N.

* Indicates that the rising is that of the preceding evening.

July.	h.	
12	2	Mercury at greatest distance from the Sun.
13	20	Venus at greatest elongation from the Sun, 46° east.
14	14	Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	July 12,	22 52	*m*
o Ceti	2 13·6	3 29 S.	„ 11,		*m*
Algol	3 0·8	40 31 N.	„ 12,	21 12	*m*
S Leonis	11 5·0	6 4 N.	„ 13,		*M*
W Virginis	13 20·2	2 48 S.	„ 11,	3 0	*M*
δ Libræ	14 54·9	8 4 S.	„ 15,	23 42	*m*
β Lyræ	18 45·9	33 14 N.	„ 12,	22 0	*M*
R Lyræ	18 51·9	43 48 N.	„ 16,		*M*
δ Cephei	22 25·0	57 50 N.	„ 15,	1 0	*M*
R Pegasi	23 1·0	9 56 N.	„ 13,		*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
From Herculis	271°	21° N.	Very slow.
Ophiuchus	280	14 S.	Very slow.
Near π Pegasi	329	36 N.	Swift. Red streaks.
From Andromeda	352	38 N.	Swift.

REPORT OF THE COMMITTEE OF INQUIRY INTO M. PASTEUR'S TREATMENT OF HYDROPHOBIA.

The following is the text of this important Report to the President of the Local Government Board:—

Sir,—In accordance with the instructions contained in a letter dated April 12, 1886, from your predecessor, the Right Honourable Joseph Chamberlain, M.P., appointing us to be a Committee to inquire into M. Pasteur's treatment of hydrophobia, we beg leave to present to you the following Report.

In order to answer the several questions involved in the inquiry, we found it necessary that some of the members of the Committee should, together with Mr. Victor Horsley, the Secretary, visit Paris, so as to obtain information from M. Pasteur himself, and observe his method of treatment, and investigate a considerable number of the cases of persons inoculated by him; and, further, that a careful series of experiments should be made by Mr. Horsley on the effects of such inoculation on the lower animals. The detailed facts of these observations and experiments are placed in the Appendix to this Report; a summary of them, and the conclusions which we believe may be drawn from them, are given in the next following pages.

The experiments by Mr. Horsley entirely confirm M. Pasteur's discovery of a method by which animals may be protected from the infection of rabies. The general facts proved by them may be thus stated:

If a dog, or rabbit, or other animal be bitten by a rabid dog and die of rabies, a substance can be obtained from its spinal cord which, being inoculated into a healthy dog or other animal, will produce rabies similar to that which would have followed directly from the bite of a rabid animal, or differing only in that the period of incubation between the inoculation and the appearance of the characteristic symptoms of rabies may be altered.

The rabies thus transmitted by inoculation may, by similar inoculations, be transmitted through a succession of rabbits with marked increase of intensity.

But the virus in the spinal cords of rabbits that have thus died of inoculated rabies may be gradually so weakened or attenuated, by drying the cords, in the manner devised by M. Pasteur and related in the Appendix that, after a certain number of days' drying, it may be injected into healthy rabbits or other animals without any danger of producing rabies.

And by using, on each successive day, the virus from a spinal cord dried during a period shorter than that used on the previous day, an animal may be made almost certainly secure against rabies, whether from the bite of a rabid dog or other animal, or from any method of subcutaneous inoculation.

The protection from rabies thus secured is proved by the fact that, if some animals thus protected and others not thus protected be bitten by the same rabid dog, none of the first set will die of rabies, and, with rare exceptions, all of the second set will so die.

It may, hence, be deemed certain that M. Pasteur has discovered a method of protection from rabies comparable with that which vaccination affords against infection from small-pox. It would be difficult to over-estimate the importance of the discovery, whether for its practical utility or for its application in general pathology. It shows a new method of inoculation, or, as M. Pasteur sometimes calls it, of vaccination, the like of which it may become possible to employ for protection of both men and domestic animals against others of the most intense kinds of virus.

The duration of the immunity from rabies which is conferred by inoculation is not yet determined; but during the two years that have passed since it was first proved there have been no indications of its being limited.

The evidence that an animal may thus, by progressive inoculations, be protected from rabies suggested to M. Pasteur that if any animal or any person, though unprotected, were bitten by a rabid dog, the fatal influence of the virus might be prevented[1] by a timely series of similar progressive inoculations. He has accordingly, in the institution established by him in Paris, thus inoculated a very large number of persons believed to have been bitten by rabid animals; and we have endeavoured to ascertain with what amount of success he has done so.

The question might be answered with numerical accuracy if it were possible to ascertain the relative numbers of cases of hydrophobia occurring among persons of whom, after being similarly bitten by really rabid animals, some were and some were not inoculated. But an accurate numerical estimate of this kind is not possible. For

(1) It is often difficult, and sometimes impossible, to ascertain whether the animals by which people were bitten, and which were believed to be rabid, were really so. They may have escaped, or may have been killed at once, or may have been observed by none but persons quite incompetent to judge of their condition.

(2) The probability of hydrophobia occurring in persons bitten by dogs that were certainly rabid depends very much on the number and character of the bites; whether they are on the face or hands or other naked parts; or, if they have been inflicted on parts covered with clothes, their effects may depend on the texture of the clothes, and the extent to which they are torn; and, in all cases, the amount of bleeding from the wounds may affect the probability of absorption of virus.

(3) In all cases, the probability of infection from bites may be affected by speedy cauterizing or excision of the wounded parts, or by various washings or other methods of treatment.

(4) The bites of different species of animals, and even of different dogs, are, probably, for various reasons, unequally dangerous. Last year, at Deptford, five children were bitten by one dog and all died; in other cases, a dog is said to have bitten twenty persons, of whom only one died. And it is certain that the bites of rabid wolves, and probable that those of rabid cats, are far more dangerous than those of rabid dogs.

The amount of uncertainty due to these and other causes may be expressed by the fact that the percentage of deaths among persons who have been bitten by dogs believed to have been rabid, and who have not been inoculated or otherwise treated, has been, in some groups of cases, estimated at the rate of only 5 per cent., in others at 60 per cent., and in others at various intermediate rates. The mortality from the bites of rabid wolves, also, has been, in different instances, estimated at from 30 to 95 per cent.

To ascertain, as far as possible, the influence of these sources of fallacy in cases inoculated by M. Pasteur, the members of the Committee who went to Paris requested him to enable them to investigate, by personal inquiry, the cases of some of those who had been treated by him. He at once, and very courteously, assented, and the names of 90 persons were taken from his notebooks. No selection was made, except that the names were taken from his earliest cases, in which the periods since inoculation were longest, and from those of persons living within reach in Paris, Lyons, and St. Etienne.

[1] The terms referring to "preventive" treatment will be used for that designed to prevent the occurrence of the disease in one already infected; those referring to "protective" treatment for that designed to protect a man or an animal from the risk of becoming infected. And it may be well to state that, though the usual custom is followed of employing the name of "hydrophobia" for the disease in men, and of "rabies" for that in animals, they are really the same disease.

The notes made on the spot concerning all these cases are given in the Appendix, and they include, as far as was possible, the evidence whether the dogs deemed rabid were really so, the situation and kind of bites, the immediate treatment of them, the statements of medical practitioners and veterinary surgeons to whom any useful facts were known.[1]

Among the 90 cases there were 24 in which the patients were bitten on naked parts by undoubtedly rabid dogs, and the wounds were not cauterized or treated in any way likely to have prevented the action of the virus; there were 31 in which there was no clear evidence that the dog was rabid; others in which the bite, though inflicted by undoubtedly rabid animals, having been through clothes, may thus have been rendered harmless. Among these, therefore, it is probable that, even if they had not been inoculated, few would have died. Still, the results observed in the total of the 90 cases may justly be compared with those observed in large numbers of cases similar to these as regards the uncertainties of infection, but not inoculated. The estimates published as to the mortalities in such unassorted cases are, as we have said, widely various. We believe that among the 90 persons, including the 24 bitten on naked parts, not less than eight would have died if they had not been inoculated. At the time of the inquiry, in April and May 1886, which was at least eighteen weeks since the treatment of the bites, not one had shown any signs of hydrophobia, nor has any one of them since died of that disease.

Thus, the personal investigation of M. Pasteur's cases by members of the Committee was, so far as it went, entirely satisfactory, and convinced them of the perfect accuracy of his records.

After the first few months in which M. Pasteur practised his treatment, he was occasionally obliged, in order to quiet fears, to inoculate persons who believed that they had been bitten by rabid animals, but could give no satisfactory evidence of it. It might, therefore, be deemed unjust to estimate the total value of his treatment in the whole of his cases as being more than is represented by the difference between the rate of mortality observed in them and the lowest rate observed in any large number of cases not inoculated. This lowest rate may be taken at 5 per cent. Between October 1885 and the end of December 1886, M. Pasteur inoculated 2682 persons, including 127 who went from this country. Of the whole number, at the rate of 5 per cent., at least 130 should have died. At the end of 1886, the number of deaths stated by M. Vulpian, speaking for M. Pasteur, was 31, including 7 bitten by wolves, in three of whom the symptoms of hydrophobia appeared while they were under treatment, and before the series of inoculations were complete. Since 1886 two more of those inoculated in that year have died of hydrophobia.

The number of deaths assigned by those who have sought to prove the inutility of M. Pasteur's treatment is, as nearly as we can ascertain, 40 out of the 2682; and in this number are included the seven deaths from bites by wolves, and probably not less than four in which it is doubtful whether the deaths were due to hydrophobia or to some other disease. Making fair allowance for uncertainties and for questions which cannot now be settled, we believe it sure that, excluding the deaths after bites by rabid wolves, the proportion of deaths in the 2634 persons bitten by other animals was between 1 and 1·2 per cent., a proportion far lower than the lowest estimated among those not submitted to M. Pasteur's treatment, and showing, even on this lowest estimate, the saving of not less than 100 lives.

The evidence of the utility of M. Pasteur's method, indicated by these numbers, is confirmed by the results obtained in certain groups of his cases.

Of 233 persons bitten by animals in which rabies was proved, either by inoculation from their spinal cords, or by the occurrence of rabies in other animals or in persons bitten by them, only 4 died. Without inoculation it would have been expected that at least 40 would have died.

Among 186 bitten on the head or face by animals in which rabies was proved by experimental inoculations, or was observed by veterinary surgeons, only 9 died, instead of at least 40.

And of 48 bitten by rabid wolves only 9 died; while, without the preventive treatment, the mortality, according to the most probable estimates yet made, would have been nearly 30.

[1] The Committee are much indebted to M. Arloing, Director of the Veterinary School at Lyons; M. Savary, Veterinary Surgeon at Brie-Comte-Robert; and M. Charlois, Veterinary Surgeon at St. Etienne, for assistance in their inquiries.

Between the end of last December and the end of March, M. Pasteur inoculated 509 persons bitten by animals proved to have been rabid, either by inoculation with their spinal cords, or by the deaths of some of those bitten by them, or as certified by veterinary surgeons. Only 2 have died, and one of these was bitten by a wolf a month before inoculation, and died after only three days' treatment. . If we omit half of the cases as being too recent, the other 250 have had a mortality of less than 1 per cent., instead of 20 or 30 per cent.

It has been objected that the number treated by M. Pasteur, which, from October 1885 to the end of 1886, included 1929 French and Algerians, was much greater than could reasonably be supposed to have been bitten by rabid animals. But there had hitherto been no careful registration of such cases, and the numbers that have occurred in the present year are not less than in the same part of last year, when the alarm about hydrophobia was greatest.

From the evidence of all these facts, we think it certain that the inoculations practised by M. Pasteur on persons bitten by rabid animals have prevented the occurrence of hydrophobia in a large proportion of those who, if they had not been so inoculated, would have died of that disease. And we believe that the value of his discovery will be found much greater than can be estimated by its present utility, for it shows that it may become possible to avert by inoculation, even after infection, other diseases besides hydrophobia. Some have, indeed, thought it possible to avert small-pox by vaccinating those very recently exposed to its infection; but the evidence of this is, at the best, inconclusive; and M. Pasteur's may justly be deemed the first proved method of overtaking and suppressing by inoculation a process of specific infection. His researches have also added very largely to the knowledge of the pathology of hydrophobia, and have supplied what is of the highest practical value, namely, a sure means of determining whether an animal, which has died under suspicion of rabies, was really affected with that disease or not.

The question has been raised whether M. Pasteur's treatment can be submitted to without danger to health or life; and, in answering it, it is necessary to refer to two different methods of inoculation which he has practised, and which are fully described in the Appendix.

In the first, which may be called the ordinary method, and which has been employed in the very large majority of cases, the preventive material obtained from the spinal cords of rabbits that have died of rabies derived, originally, from rabid dogs is injected under the skin, once a day for ten days, in gradually increasing strengths.

In the second or intensive method (*méthode intensive*) which M. Pasteur adopted for the treatment of cases deemed especially urgent, on account either of the number and position of the bites or of the long time since their infliction, the injections, gradually increasing in strength, were usually made three times on each of the first three days, then once daily for a week, and then in different degrees of frequency for some days more. The highest strength of the injections used in this method was greater than the highest used in the ordinary method, and was such as, if used at first and without the previous injections of less strength, would certainly produce rabies.

By the first or ordinary method, there is no evidence or probability that anyone has been in danger of dying, or has in any degree suffered in health even for any short time. But after the intensive method deaths have occurred under conditions which have suggested that they were due to the inoculations rather than to the infection from the rabid animal.

There is ample reason to believe that in many of the most urgent cases the intensive method was more efficacious than the ordinary method would have been. Thus, M. Pasteur mentions that, of 19 Russians bitten by rabid wolves, 3 treated by the ordinary method died, and the remaining 16, treated by the intensive method, survived; and he contrasts the cases of 6 children, severely bitten on the face, who died after the ordinary treatment, with those of 10 similarly bitten children who were treated by the intensive method, and of whom none died; and M. Vulpian reports that, of 186 persons badly bitten by animals that were most probably rabid, 50 treated by the intensive method survived, and of the remaining 136 treated by the ordinary method 9 died.

The rate of mortality after the intensive method was not greater than that after the ordinary method; for among 624 patients thus treated only 6 died, or, counting one doubtful case, 7. But that which excited suspicion was the manner of death in some of them; and this manner was observed in a man named Goffi, sent from England. On September 4 last, he was severely bitten at the Brown Institution by a rabid cat, to which in spite of repeated warnings, he exposed his naked hand. Twelve wounds were inflicted. They were at once treated with pure carbolic acid, and, six hours later, he was put under the influence of chloroform at St. Thomas's Hospital, the wounded portions of skin were freely excised, and the wounds thus made were treated with carbolic acid. On the same evening he was sent to Paris, and on the following morning M. Pasteur commenced the intensive treatment, and it was continued during twenty-four days. During all this time the man was repeatedly intoxicated.[1] He once fell into the Seine; and while crossing the Channel on his return home he was severely chilled.

On October 10 he returned to his work, and appeared to be in his usual health; but he became unwell, with pain in the abdomen, like colic, and with pain in the back. On the 18th he had partial motor paralysis in the lower limbs, and on the 19th complete motor paralysis of these limbs and of the trunk, and partial motor paralysis of the upper limbs and face. He was taken to St. Thomas's Hospital, where he died on the 20th.

To the last he was free from all the usual symptoms of hydrophobia, and the progress of his disease and the manner of his death were so similar to those of what is described as acute ascending paralysis, or Landry's paralysis, that a verdict to this effect was given at a coroner's inquest. But the certainty that his death was due to the virus of rabies was proved by experiments by Mr. Horsley. A portion of his spinal cord was taken to provide material for inoculations, and rabbits and a dog inoculated with it died with characteristic signs of paralytic rabies, such as usually occurs in rabbits.

In most of the other cases of death after treatment by the intensive method, the symptoms have been nearly the same as those just related; but in none of them has the same test of death from hydrophobia been applied. The likeness of the symptoms to those of the form of rabies called dumb or paralytic, usually observed in rabbits, has suggested, as we have said, that the deaths were due not to the virus of the rabid dog or cat, but to that injected from the spinal cord of the rabbit. But this is far from certain. In the case of Goffi, especially, the incubation period was such as would have followed the bite of the cat, not the inoculation of highest intensity; and the incubation period in the rabbits and dog inoculated from his spinal cord were such as have been observed after similar inoculations with virus derived, not only from rabbits inoculated in series by M. Pasteur, but from a dog, a cat, and a wolf that died of ordinary rabies. It may well have been, therefore, that the intensive inoculations in him and in the other persons who died after them were not themselves destructive, but that they failed to prevent the rabies which was due to the bites. They may also have modified the form in which the rabies manifested itself; giving it the characters of the paralytic rabies usual in rabbits, instead of the convulsive or violent form usually, but not always,[2] observed in man after bites of cats or dogs.

The question is likely to remain undecided; for to avoid the possible, however improbable, risk of his intensive treatment M. Pasteur has greatly modified it, and even in this modified form employs it in none but the most urgent cases.

The consideration of the whole subject has naturally raised the question whether rabies and hydrophobia can be prevented in this country.

If the protection by inoculation should prove permanent, the disease might be suppressed by thus inoculating all dogs; but it is not probable that such inoculation would be voluntarily adopted by all owners of dogs, or could be enforced on them.

Police regulations would suffice if they could be rigidly enforced. But to make them effective it would be necessary: (1) that they should order the destruction, under certain conditions, of all dogs having no owners and wandering in either town or country; (2) that the keeping of useless dogs should be discouraged by taxation or other means; (3) that the bringing o

[1] Other cases, as well as this, have led M. Pasteur to believe that the risk of death from hydrophobia is much increased by habits of drunkenness.

[2] Cases of paralytic hydrophobia have been observed, though rarely, in men bitten by rabid animals, and not treated by inoculation. It may, indeed, be suspected that at least some of the cases of "acute ascending paralysis" may have been cases of this form of hydrophobia, although, in the complete absence of the usual violent symptoms, no suspicion of the source of the disease was entertained.

dogs from countries in which rabies is prevalent should be forbidden or subject to quarantine ; (4) that, in districts or countries in which rabies is prevalent, the use of muzzles should be compulsory, and dogs out of doors, if not muzzled or led, should be taken by the police as "suspected." An exception might be made for sheep-dogs and others while actually engaged in the purposes for which they are kept.

There are examples sufficient to prove that, by these or similar regulations, rabies, and consequently hydrophobia, would be in this country "stamped out," or reduced to an amount very far less than has hitherto been known.

If it be not thus reduced it may be deemed certain that a large number of persons will every year require treatment by the method of M. Pasteur. The average annual number of deaths from hydrophobia, during the ten years ending 1885, was, in all England, 43 ; in London alone, 8·5. If, as in the estimates used for judging the utility of that method of treatment, these numbers are taken as representing only 5 per cent. of the persons bitten, the preventive treatment will be required for 860 persons in all England ; for 170 in London alone. For it will not be possible to say which among the whole number bitten are not in danger of hydrophobia, and the methods of prevention by cautery, excision, or other treatment, cannot be depended on.

We have the honour to be,
Sir,
Your obedient Servants,
(Signed) JAMES PAGET, Chairman,
T. LAUDER BRUNTON,
GEORGE FLEMING,
JOSEPH LISTER,
RICHARD QUAIN,
HENRY E. ROSCOE,
J. BURDON SANDERSON.

VICTOR HORSLEY, Secretary, June 1887.

The Report is followed by appendices, two of which we reprint :—

Abstract Report of Mr. Horsley's Experiments.

The first object of the experiments was to test M. Pasteur's method of transmitting rabies by inoculation, and to compare its effects with those of rabies due to the bites of dogs found rabid in the streets.[1]

Through the kindness of M. Pasteur, two rabbits inoculated by him were placed at the disposal of the Committee on May 5, 1886, and were conveyed within 24 hours safely to the Brown Institution, where the experiments were carried out by Mr. Horsley.

In these two rabbits the first symptoms of rabies appeared on May 11 and 12, and the disease followed exactly the course described by M. Pasteur.

At first the animals appeared dull, but continued to take food readily until symptoms of paralysis appeared. The first of these symptoms was commencing paralysis of motion of the hind-legs, not accompanied by any loss of sensibility. The paralysis soon extended to the muscles of the fore-legs, and later to those of the head, and the animals died comatose.

After post-mortem examination, portions of the spinal cord of each of these rabbits were crushed, according to M. Pasteur's method, in sterilized broth, and the liquid so obtained was injected beneath the dura mater into four rabbits and the same number of dogs, all being first rendered insensible with chloroform or ether.[2]

Of the four rabbits so inoculated, the first two showed the first symptoms seven days after the inoculation ; the third and fourth on the sixth day. The symptoms as well as the incubation period exhibited by these rabbits were exactly the same as were observed in those brought from M. Pasteur's laboratory. Careful notes and photographs were taken in the case of all the animals, in order that the constant and specific nature of the disease might be demonstrated by observations during life and after death. It was also observed that during the incubation period the temperature of the body remained normal, that is, about 39°·4 C. With the first definite symptom the temperature rose to about 40°·4 C., which is the temperature usually observed during the first day of the obvious illness. By the next day it began to fall, and on the third day after the appearance of the first symptom it averaged 37°·5 C. On the last day it was always below normal, and on one occasion fell before death to 24° C. The animals did not appear to suffer any pain whatever in the course of the disease. They were free from the spasms which, in the earlier stages of the malady in man, form so painful a feature of the disease, and indeed the disease in them resembled throughout that rapidly fatal, but painless, disease of man known as acute ascending paralysis.

The post-mortem appearances in the rabbits were remarkably uniform. As a rule nothing abnormal, save congestion, presented itself either in the brain, spinal cord, heart, blood-vessels, or serous membranes. The larynx, pharynx, and, more especially, the epiglottis, and the root of the tongue, were frequently intensely congested. The lungs showed almost invariably capillary congestion ; and sometimes small patches resembling broncho-pneumonia were observed. The mucous membrane of the stomach was very markedly congested, and there were at its cardiac extremity numerous hæmorrhages.[1] The constancy of these appearances was most remarkable, and corresponded in every particular with those subsequently observed in rabbits which had died of rabies from the bite of rabid dogs.

Of the four dogs inoculated, the first showed on the eighth day after inoculation an alteration in the voice and commencing excitement ; on the following day the excitement became excessive, and the bark was quite characteristic ; on the eleventh day the dog was aggressive, notwithstanding slight paralysis of the legs ; on the twelfth day the paralysis had increased, and on the next day there was complete paralysis and coma, and death occurred on the fifth day after the onset of the symptoms.

The second dog showed the first symptom on the ninth day after inoculation, when it was very dull and partially paralyzed ; its bark was characteristic. Next day the paralysis was almost complete, and on the twelfth day the animal died. This was therefore a case of the rapid paralytic form ; whilst in the first dog the disease was of the ordinary furious form of rabies terminating in paralysis.

The third dog showed the first symptom on the ninth day after inoculation, and from that time became gradually paralyzed, and died on the sixteenth day.

The fourth dog showed the first symptom in from eight to nine days after inoculation, and during the first day was extremely aggressive ; on the two following days the characteristic bark was observed ; and on the twelfth day there was paralysis of the hind-legs ; it died on the thirteenth day. Thus the furious form and the paralytic or dumb form of rabies were represented in equal numbers, whereas, in the usual mode of infection by biting, the former is more prevalent.

The post-mortem appearances were as follows :—The brain and central nervous system were in some of the dogs the seat of considerable congestion ; in others these organs appeared normal. The serous membranes were perfectly normal ; the larynx especially, and sometimes the pharynx, were congested ; the lungs always congested, especially in the lower lobes ; the heart normal ; the blood usually fluid, occasionally with post-mortem clots ; the stomach was always found to contain foreign bodies, such as straw ; and its mucous membrane was congested, frequently showing numerous hæmorrhages ; the small intestine was always empty, and the large glandular organs showed venous congestion.

For the purpose of exact comparison of the disease just described with that produced when rabies is communicated to the rabbit in the ordinary way, some rabbits previously narcotized with ether were caused to be bitten by rabid dogs of the streets, or were inoculated by trephining with material obtained from the spinal cord of dogs or other animals which had died of rabies, and in one instance from that of a man who had died with hydrophobia.

Four series of experiments of observations in which rabbits were bitten by rabid dogs from the streets were made. In one of them the dog by which the rabbit was bitten exhibited the dumb form, in others the furious form, of the disease. In each series excepting the first a large proportion of the rabbits died ; the symptoms presenting themselves in these cases were identical with those observed in the rabbits inoculated from M. Pasteur's virus, but the duration of the symptoms was usually longer. As

[1] This expression is adopted from that usual in France, "*rage des rues*"

[2] All the experiments performed in this inquiry were thus made painless.

[1] In some, signs of post-mortem digestion were found.

has been stated, rabbits inoculated by M. Pasteur's virus rarely show symptoms during more than three days before death, whereas the rabbits bitten by rabid dogs from the streets often live for a week after the appearance of the first symptoms.

The post-mortem appearances in the rabbits dying after having been bitten by rabid dogs of the streets were the same as those already described in rabbits inoculated with the virus from M. Pasteur's rabbits.

In the case of rabbits inoculated by trephining with the virus from animals dying of rabies of the streets, the incubation period was from 14 to 21 days. In all cases the symptoms were similar to those produced by M. Pasteur's virus, and those of rabbits bitten by rabid dogs from the streets; but in the prolongation of the disease approached more closely in character to the latter.

The results of these experiments confirm several of the chief observations made by M. Pasteur; especially—

(1) That the virus of rabies may certainly be obtained from the spinal cords of rabbits and other animals that have died of that disease.

(2) That, thus obtained, the virus may be transmitted by inoculation through a succession of animals, without any essential alteration in the nature, though there may be some modifications of the form, of the disease produced by it.

(3) That, in transmission through rabbits, the disease is rendered more intense; both the period of incubation, and the duration of life after the appearance of symptoms of infection, being shortened.

(4) That, in different cases, the disease may be manifested either in the form called dumb or paralytic rabies which is usual in rabbits; or in the furious form usual in dogs; or in forms intermediate between, or combining, both of these, but that in all it is true rabies.

(5) That the period of incubation and the intensity of the symptoms may vary according to the method in which the virus is introduced, the age and strength of the animal, and some other circumstances; but, however variable in its intensity, the essential characters of the disease are still maintained.

The certainty that the virus of rabies can thus be transmitted without essential change made it desirable, in the next place, to ascertain whether, as M. Pasteur states, it can be so attenuated that it may be inoculated without risk to life, and whether animals thus inoculated are thus made safe from rabies. The methods for this protective inoculation which M. Pasteur has employed are described.

To test them, six dogs were "protected" by injecting subcutaneously the emulsions of spinal cords of rabbits which had died of rabies; beginning with that of a cord which had been dried for 14 days, and, on each following day, using that of a cord which had been dried for one day less, till at last that from a fresh cord was used.

None of these dogs suffered from the injections; and when they were completed, the six dogs thus "protected," and two others unprotected, and some rabbits unprotected, were made insensible with ether, and were then bitten by rabid dogs, or by a rabid cat, on an exposed part.

A "protected" dog, No. 1, was bitten on July 8, 1886, by a dog which was paralytically rabid. It remains perfectly well.

An "unprotected" dog, No. 1, was bitten a few minutes afterwards by the same rabid dog, and died paralytically rabid.

A "protected" dog, No. 2, was bitten on November 6, 1886, by a dog which was furiously rabid; it remains well. At the same time, four "unprotected" rabbits were bitten by the same rabid dog, and of these two died of rabies in the usual form (*i.e.* 50 per cent. of animals bitten).

The same results followed with the "protected" dog, No. 3, and the "unprotected" rabbits, bitten at the same time. The dog still lives, the rabbits died of rabies.

The "protected" dogs, Nos. 4 and 5, were bitten on January 20, 1887, by a furiously rabid dog; and on the same day the "unprotected" dog, No. 2, and three "unprotected" rabbits were bitten by the same dog. The "protected" dogs remain well; the "unprotected" dog and two rabbits died with rabies (*i.e.* 75 per cent. of the animals bitten.)

The "protected" dog, No. 6, was bitten on three different occasions by a furiously rabid cat on September 7, 1886; by a furiously rabid dog on October 7, 1886; and by another furious rabid dog on November 6, 1886. It died ten weeks after being bitten for the third time, but not of rabies. It had been suffering with diffuse eczema during the whole of the time that it was under observation, and it died of this. At the post-mortem examination, no indication of rabies was found; and two rabbits, inoculated by trephining with the crushed spinal cord, showed no sign of rabies, either during life or, when they were killed several months afterwards, in any appearance after death. It was thus made certain that the dog was not rabid.

Thus, all the experiments performed by Mr. Horsley have confirmed those of M. Pasteur, and the experiments last described have shown that animals may be protected from rabies by inoculations with material derived from spinal cords prepared after M. Pasteur's method. The protection may be deemed somewhat similar to that given by the inoculation for anthrax, or by vaccination for small-pox, though the theory of the method of inoculation devised by M. Pasteur is very different from that upon which vaccination for small-pox and inoculation for anthrax is based. The further step, the prevention of rabies or hydrophobia in animals or in persons into whom the virus has already been introduced by bites or otherwise, is considered in the body of the Report.

In the course of his experiments, Mr. Horsley observed many interesting facts concerning the modification of the action of the virus according to the method of its inoculation, and the condition of the animal inoculated; but he found nothing to justify a belief that any animal not inoculated is insusceptible of rabies or that the disease ever arises spontaneously.[1]

Coincidently with these experiments, some were made by Mr. Dowdeswell for the purpose of ascertaining whether any drugs can protect an animal from rabies. Their result is recorded in a paper read before the Royal Society, and may be summed up in the statement that rabies can neither be prevented nor influenced in its course, unless it be for the worse, by any of the drugs that were employed, including allyl alcohol, atropine, benzoate of soda, chloral, cocaine, curare, iodine (dissolved in iodide of potassium), mercuric perchloride, quinine, salol, strychnine, urethane.

M. Pasteur's Methods of Preventive Inoculation.[2]

M. Pasteur believes that the virus of rabies is a living micro-organism, and that, like some others, it produces in the tissues it invades an excretory substance by which, when present in sufficient quantity, its own development and increase are checked, as are those of the yeast ferment by the alcohol produced in the vinous fermentation. In accordance with this theory, he thinks that the spinal cords of animals that have died of rabies contain both the virus and this excretory substance which, practically, may be deemed its antidote. He believes therefore that by injections of an emulsion from such spinal cords into the systems of animals bitten or inoculated with the virus of rabies, the antidote may be able, during the period of incubation, to arrest and prevent the fatal influence of the virus. But, in order to avoid the possibility of injecting a still potent virus, M. Pasteur holds that the virus in the spinal cord must be weakened by drying the cord in a pure and dry atmosphere at a temperature of 20° C.; in which drying the efficiency of the antidote may be reduced to a much less extent than the potency of the virus. By such drying this potency may be so reduced that an emulsion of the dried spinal cord may be injected without any risk of producing rabies: and this risk is in no measure increased by the daily injections of emulsions from cords dried during a gradually less number of days, and which, though more virulent than those first used, still contain a larger proportion of the antidote than of the virus.

In accordance with this theory, the method of the preventive injections first used by M. Pasteur was adjusted in the following manner:—

Days of Inoculation.	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th
Days during which the spinal cord had been dried	14	13	12	11	10	9	8	7	6	5

In consequence of some deaths among those who had been thus treated, M. Pasteur deemed it necessary, in cases of very severe bites and of persons bitten long before the treatment

[1] The minuter facts connected with all these experiments will soon be communicated to one of the scientific Societies.

[2] As derived from the observations made by the Committee, and from a full description supplied by Prof. Dr. Grancher, April 11, 1887.

could be commenced, to increase the intensity of the treatment by more speedily increasing the strength of the injections, by more frequent repetitions of them, and by using on certain days spinal cords dried during only three, two, and one days. Thus in September and October 1886 he adopted the following formula :—

Days of Inoculation.	1st.	2nd.	3rd.	4th.	5th.
Days' drying of the cords	14, 13, 12	11, 10, 9	8, 7	6, 5	4, 3

Days of Inoculation.	6th.	7th.	8th.	9th.	10th.	11th.
Days' drying of the cords	2	1	6, 5	4, 3	2	1

In very severe and perilous cases this course was repeated even three or four times. It was distinguished as the *méthode intensive*, and among such severe cases it was followed by a marked diminution of mortality. But when it appeared possible that it might be dangerous, M. Pasteur changed it for that which he now uses, and which may be thus represented :—

Days of Inoculation.	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.
Days' drying of the cords	14, 13	12, 11	11, 10	10, 10	9, 9	9	8

Days of Inoculation.	8th.	9th.	10th.	11th.	12th.	13th.	14th.	15th
Days' drying of the cords	8	8	7	7	7	6	6	5

The material for injection is prepared by crushing portions of the dried spinal cord, and diffusing them in sterilized broth free from all risk of putrefaction, decomposition, or any change due to the presence of other micro-organisms ; and the injection is made with syringes through fine tubular needles into the subcutaneous tissue.

For transmissions of rabies through rabbits, in order to obtain the spinal cords required for its prevention in other animals, injections of virus of highest intensity are made through minute holes in the skull into the space under the dura mater or fibrous covering of the brain.

The materials for the protective inoculations are prepared in the same manner as those for the preventive, from spinal cords dried from ten days to one day.

UNIVERSITY COLLEGES AND THE STATE.

ON Thursday last, June 30, a deputation consisting of members of Parliament and others had an interview with the Chancellor of the Exchequer, who was accompanied by Mr. Jackson, M.P., to urge that Government assistance should be extended to local university colleges situated in various parts of the country. Among those present were Sir John Lubbock, M.P., Mr. Mundella, M.P., Mr. J. Chamberlain, M.P., Sir Lyon Playfair, M.P., Mr. Bryce, M.P., Mr. Arnold Morley, M.P., Mr. Jesse Collings, M.P., Mr. R. Chamberlain, M.P., Sir U. Kay-Shuttleworth, M.P., Mr. Theodore Fry, M.P., Mr. Burt, M.P., Sir Henry Roscoe, M.P., Sir A. K. Rollit, M.P., Prof. Stuart, M.P., Sir Bernhard Samuelson, M.P., Mr. Howard Vincent, M.P., Sir W. H. Houldsworth, M.P., Dr. Percival, and Sir Philip Magnus.

Sir John Lubbock, as the representative of the University of London, introduced the deputation. Their request was that a Parliamentary grant should be made to English colleges, as was already made to those in Ireland, Scotland, and Wales. The colleges on behalf of which they appeared were doing excellent work, but were greatly hampered for want of funds. The claims of these colleges were not based alone on their services to learning and study ; they were calculated to contribute largely to the material prosperity of the country. We now imported £150,000,000 worth of food annually, and our population increased at the rate of about 350,000 a year. How were so many to be fed, and how could a revival and return of trade be promoted? Our rivalry with foreign nations was now not on the battlefield but in the manufactory and the workshop ; and it was none the less severe because it was a competition rather than a contest. The need of the assistance for which they asked was very pressing. Without going into details as to particular colleges, he observed that the more recent institutions were generally spending more than their income, and even the oldest and the richest were sadly crippled for want of funds. It was found practically impossible to increase the subscriptions, and local authorities, as a rule, had no power to supplement their funds. As to raising the fees so as to make the colleges self-supporting, that might be possible but would be very undesirable. He only wished the fees could be abolished altogether, for those receiving education at the colleges benefited not only themselves but the whole nation. As to the expenditure on education, it was in the opinion of some people very large, but it was small in comparison with other items. Our ignorance cost us very much more than our education. Moreover, the principle for which they contended had been conceded in regard to Scotland, Ireland, and Wales. The grants to Irish colleges amounted to £25,000, to Scotland £16,000, and to Wales £12,000. The University of Glasgow had a special grant of £150,000 for building. None of the English colleges had such aid. Their request simply was that in this matter of education England should be treated in the same way as Ireland, Scotland, and Wales.

Mr. J. Chamberlain said that he attended as the representative of Mason College, Birmingham. Their case was the same in principle as that of all the other colleges. They urged that State-aided education had been accepted in principle in England and in all other countries, but in England alone we had not followed out the principle to its logical conclusion. We had stopped at the lower grade, and in this respect had made a great mistake. If it was of national importance that every one should have placed within his reach the instruments of education, it was of equal importance that they should be stimulated and encouraged to make use of these facilities. An attempt had been made in some halting fashion to redress the inequality in which this country was placed. The Charity Commissioners had recently been diverting funds which were, to some extent at all events, intended for the benefit of the poorer classes of the population to the purposes of higher secondary education. That practice was open to very serious objection, because it was robbing Peter to pay Paul ; and also because under that system nothing whatever was done for the colleges represented by the deputation, which were carrying on and extending the education given in the primary and secondary schools. The enormous development of primary and secondary education had created a demand for higher education. Proof of that was to be found in the fact that, although the institutions now represented were nearly all of them the creation of the present generation, they had had, in spite of deficiency of means, the most remarkable success ; and the daily increasing number of their students showed that they were established to meet a real want. The pressure of commercial competition came almost exclusively from those nations in which technical instruction and higher education had been developed and stimulated by the action of the State. The demand now made upon the Government was really a very moderate one, and the sum asked for was never likely to assume any very large amount. He believed the grants for primary education amounted to something between £2,000,000 and £3,000,000 a year, and that the additional grant now asked for would only amount to something like £50,000.

Mr. Goschen,—Will the deputation be able to supply me with a scheme for the distribution of the £50,000 or with the principle?

Mr. Chamberlain replied that, in his opinion, the grants should be made conditional upon further local aid. In that way the Treasury would be able to distinguish the colleges which were entitled to share in the grant.

Mr. Goschen,—Conditional upon further local aid?

Mr. Chamberlain,—Proportioned, in the first place, to the number of students, and, in the second place, conditional upon the amount of local aid.

Mr. Mundella,—Not in all cases further local aid.

Mr. Chamberlain agreed with Mr. Mundella that in some cases large local contributions were being made, and those cases should be taken into account. In conclusion, he urged upon the Government the consideration of a nationality that was sometimes apparently forgotten, and in the name of the 25 millions of English population he asked that they should receive a recognition in the matter of education proportionate to that given to Ireland, Scotland, and Wales.

Mr. Mundella pointed out that there were precedents for what they were asking in the grants given in Scotland, Wales, and elsewhere, and, in his opinion, Sir John Lubbock had rather underrated the benefit which schools in Scotland derived from the system. The high schools in Scotland were aided by grants out of the rates, and all middle-class education was largely supplemented by grants. In every country in Europe which really rivalled England a first-class technical education was within the reach of the humblest classes. In France very much the same education as given in our colleges could be obtained free and at the expense of the State. There was at present no power to aid the colleges in England, but he felt sure the Education Department would have no difficulty in framing a scheme for the purpose; and he ventured to hope the Chancellor of the Exchequer would assist them, and that grants should be allowed to these institutions in proportion to the efficiency of each college.

Sir Lyon Playfair observed that as the Government was going to introduce a Bill for giving to School Boards and other authorities power of rating for higher and technical education, he thought it would be well to extend the operation of the Bill by giving power to the same bodies to rate for higher colleges. The authorities were quite ready to be rated, and only wanted the necessary powers. The experience of commercial nations throughout the world was that the competition of industries was a competition of intellect. The difference between the policy of this and other countries was that while in other countries the State recognized the fruits of education and acted upon their perception of them, we left the first steps to the efforts of intelligent men in various localities. These men had now done their part, and, in consequence of the action of the English Government in the past, he thought it was the duty of the Government to come to the rescue of this small and highly intelligent body of men who had got up these colleges, and give to them that permanence which they were not likely to have without some small support from the public funds.

Sir Bernhard Samuelson said that in the course of the investigations of the Technical Education Commission he had the opportunity of visiting nearly all the colleges now appealing for aid, and, as far as his judgment enabled him to form an opinion, he must say that, considering the means at their disposal, these colleges were doing a thoroughly good work, and a work which deserved the encouragement of the community.

Mr. Thomas Burt, M.P., was not quite clear that this was the best way of spending money educationally, but he was quite sure it was a very good one. He could testify to the value conferred upon the miners and the industrial classes generally of the North by the College of Science in Newcastle. That institution had been greatly crippled in its resources. There was among the miners a widespread desire for improved education, and the College of Science and the University Extension lectures had not only given positive instruction of a valuable kind, but had conferred still greater advantages by creating and stimulating a desire on the part of the industrial classes for improved education. If the Government could see their way to help this and kindred institutions, a very great benefit would be conferred on the industrial classes of the country.

After a few words from Prof. Tilden (who differed from other speakers as to charging local rates), Dr. Perceval, and Sir George Young,

Mr. Goschen, in reply, said:—When Mr. Tilden sat down just now I was thinking on the whole that it was rather advisable for the deputation that the list of speakers was very nearly exhausted, because the differences of opinion began to be manifest. Mr. Tilden objected to powers being given to corporations or boroughs to rate themselves, whereas one of the objects of the deputation, or, at all events, one of the suggestions made to me during the course of this deputation, was that we should be sure to give powers to localities to rate themselves for these purposes. I do not know what the view would be of the deputation upon the subject, but I suspect that the bulk of the deputation is in favour of power being given, which of course would be optional, for large towns to rate themselves for this purpose. Mr. Tilden argued that it would be unfair that a college which drew studen from other quarters should be supported by local rates; bu am inclined to think that that is a dangerous argument to u because you might find whole masses of the population—t agricultural population, for instance, which would derive co paratively little advantage from these colleges—who might s that they would not wish to be taxed towards national fun which were to be applied to the big towns for the support their colleges. So that I think it is rather a dangerous arg ment, and I further think the towns derive a very considera advantage from having these large institutions, and that th ought not to look too narrowly to the area over which th rates would be charged. Then, Mr. Burt, I think, made remark which to my ears was rather significant—that thou this was a good way of spending money, he was not sure th this was the best way of spending money upon educatic The interpretation I put upon that remark was that probab a further demand would be made upon the national pu for educational assistance in other directions which, in M Burt's opinion, would be the best. No one is more awa gentlemen, than you, who have studied this subject so deep that there are many directions in which, and many points of vi from which, this educational question has to be considered. am glad to have received this most important deputation. might almost call it in some respects a formidable deputatio but I know that this is not the only direction in which pressu would be put upon the Treasury with regard to education. Y represent here what I understand to be the higher forms technical, scientific, and I think you may also say of literary struction. There is the elementary education, and the expe attendant upon that; then there is the Science and Art Depa ment, which in some respects is apart from the element education; and there remains a field for which I am sure pr sure will be put upon the Treasury, which is that techni education which lies between the elementary education and t higher form of education which I understand these colleges gi I make these remarks to show that it would be impossib I think, for the Government—though, of course, upon that would be rather for the Education Department than the Chancellor of the Exchequer to speak—to look upon t matter simply from a partial point of view. We must revi the whole of the demands which are likely to be made upon Government for educational purposes. It is certain that comparative test which has been applied by this deputation i very dangerous one to the finances of the State. The grant certain colleges is certain to lead to a grant to other colleg and if some of the gentlemen present had heard how the Sco gentlemen argued that their case was practically peculiar, and t the assistance given to the Scotch Universities could not possibly made applicable to the English colleges, they wo see the scope of the remarks which I have made. I far from saying that, while there is this serious pressure lik to be put upon the Government, this is not a question wh must be carefully and deliberately faced, and looked upon all its bearings. You have come to me to-day, I presume, order to remove what we may call any financial scruples w regard to your proposals; but, of course, it would mainly with the Education Department to work out any system if th colleges are to be granted assistance upon the scale which y suggest, and so you will not expect me to give you any decla tion of policy to-day. But I presume that you are anxious t the arguments you have used should sink into my mind, in or to remove any opposition I might make financially to propos that would be made by other departments and through ot channels. Now, from the financial point of view, I gather t you assent to certain propositions, and the main of those prop tions is that assistance should only be given by the State wh there has been a distinct local effort in support. I admit t we cannot argue any more that the State should not aid educat up to a certain line. That line seems to be by public opin raised every day, and while formerly it was only the very p now it is a higher class; and so from class to class it appear me the demand for State education is rising very rapidly, p haps I might say very formidably. But I am glad to take n of the fact that, anxious as you are, representing as you immense educational efforts in various parts of the country, do not wish in any way to stop that magnificent flow of priv contributions towards education which has been the g of this country in many ways. It would be deplorabl by the action of the State you were to arrest that acti you are rather anxious that that action should be stimulated by moderate contributions from the public funds and from rates. As to contributions from rates, I think that as the municipal institutions of our country are more and more reformed and developed, and the more power is given to them in the course of that process of decentralization which is now accepted by almost all politicians, the more power you may give to these localities to act with a certain freedom in the way of assisting such institutions as they think are calculated to advance their interests in every way. There is one point on which I should like to say something from a personal point of view. When I made a remark that your colleges were partly literary and partly technical, I did not wish to convey the impression that I think colleges for general culture deserve less recognition than colleges for technical education I think that they must to a very great extent stand or fall together, and that it would be an error—though I am aware there may be others here who take a different view—if technical education were too much to displace that general education and development of the intellect which surely must always be one of the great objects of education in every form. I do not know whether the sum which was first mentioned, I think, by Sir John Lubbock has been arrived at by any general agreement, college by college, or whether it is a mere general guess. But I am sure it would be necessary, as a preliminary examination of the case which you have put before me, that there should be some standard suggested, either of numbers or of local contribution, and also of work, before the matter could be taken into serious practical consideration; because not only are there these twelve colleges, but I fancy that, as soon as any arrangement had been made in favour of them, we should find another list of colleges, not precisely on the same footing, but which were sufficiently strong to make a kind of claim on that comparative system which is constantly increasing the national expenditure. I think those of the deputation who are members of Parliament will acknowledge that it would be perfectly impossible to deal with the matter in the supplementary estimate this year, even if we assented to it, without much further examination, for it is really the Education Department which must examine this matter. I have not had the opportunity of consulting my colleagues on the magnitude of the sum which you suggest, or on the general principle. All I can say to-day is that I am glad to receive the suggestions which you have made; that I recognize, of course, the great importance of further developing technical and scientific education; but I cannot pledge myself to any particular sum or to any particular mode of carrying out your wishes. The deputation may rely on the Government giving the matter its most serious attention, and we shall be most willing to receive suggestions from such men as Sir Lyon Playfair, Sir John Lubbock, Mr. Mundella, and the other gentlemen who take so deep an interest in education, to see what practical shape can be given to the wishes of the deputation.

Mr. Mundella observed that the condition of some of the colleges was such that it was desirable that the intentions of the Government should be known at the earliest possible moment.

Sir John Lubbock moved, and the Mayor of Sheffield seconded, "That the thanks of the deputation be given to Mr. Goschen for his courtesy."

The deputation then withdrew.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 16.—"Dispersion Equivalents," Part I. By J. H. Gladstone, Ph.D., F.R.S.

The object of this paper was to bring to the notice, especially of chemists, the subject of dispersion equivalents; a property of bodies similar to the refraction equivalents which are now generally recognized. In the paper of Gladstone and Dale in the Phil. Trans. for 1863 they had adopted the difference between the refractive indices for the solar lines A and H as the measure of dispersion. This, divided by the density, gave the specific dispersion. In 1866 they multiplied this by the atomic weight, and termed the product the dispersion equivalent. The subject has scarcely been touched since that time either by English or Continental observers.

The author holds that the following conclusions are warranted by the accumulated data:—

(1) That dispersion, like refraction, is primarily a question of the atomic constitution of the body: the general rule being that the dispersion equivalent of a compound is the sum of the dispersion equivalents of its constituents.

(2) That the dispersion of a compound, like its refraction, is modified by profound differences of constitution; such as, changes of atomicity.

(3) That the dispersion frequently reveals differences of constitution at present unrecognized by chemists, and not expressed by our formulæ.

The dispersion equivalents of a few elements may be determined by direct observation, such as phosphorus, sulphur, selenium, &c.; but more important results have been obtained from organic substances by comparing the dispersion equivalents of different liquid or dissolved compounds of carbon.

From a consideration of the data afforded by Continental observers, or obtained by the author himself, the following table has been drawn up. The dispersion figures must be taken merely as approximate.

Substance.	Atomic weight.	Refraction equivalent A.	Dispersion equiv. H—A.
Phosphorus	31	18·3	3·0
Sulphur, double bond	32	16·0	2·6
,, single bonds	,,	14·0	1·2
Hydrogen	1	1·3	0·04
Carbon	12	5·0	0·26
,,	,,	6·1 ?	0·51
,,	,,	6·1	0·66
Oxygen, double bond	16	3·4	0·18
,, single bonds	,,	2·8	0·10
Chlorine	35·5	9·9	0·50
Bromine	80	15·3	1·22
Iodine	127	24·5	3·65
Nitrogen	14	4·1	0·10
CH_2	14	7·6	0·34
NO_2	46	11·8	0·82

An examination of many salts of potassium and sodium in aqueous solutions led to the conclusion that there was always a difference of about 0·09 in their dispersion equivalents; but it was not so easy to determine the actual dispersive value of the metals in question, the determinations of potassium from different salts varying from 0·40 to 0·59.

The main conclusion is that the specific dispersive energy of a compound body is a physical property analogous to, but distinct from, its specific refractive energy; and that the phenomena of dispersion are capable in like manner of throwing light upon chemical structure.

Royal Microscopical Society, June 8.—The Rev. Dr. Dallinger, F.R.S., President, in the chair.—Dr. E. M. Crookshank exhibited a series of cultivations of micro-organisms, and called attention to the somewhat unusual circumstance of being able to show such a typical series all growing at the same time. One of the specimens shown was a chromogenic *Spirillum*, which had developed its colour in the depths of the gelatine, contrary to the general rule. He also showed a micro-organism which had been said to cause the swine fever—or, rather, swine erysipelas—in Germany. It was to be noted that in Germany there had been many cases of swine disease, and that a different organism had been found associated with it there from the one found here, and recognized as the cause of Dr. Klein's swine fever. So far as he (Dr. Crookshank) had been able to make out, they were not identical, the German form being an extremely minute Bacillus forming only a cloudy appearance and seeming to be similar to mouse septicæmia. He thought there was good ground for regarding the two diseases as distinct from each other, the German form being swine erysipelas as distinct from swine fever. He also exhibited an example of a Bacillus obtained from putrid fish, which caused the remarkable phosphorescence frequently noticed when fish was decaying.—Mr. Freeman exhibited a number of series of sections of the anatomy of spiders, worms, &c., made by Mr. Underhill at Oxford.—Mr. Eve called attention to some specimens of *Actinomyces* from the jaw of an ox, and described the effect of the disease upon the animal.—Prof. Rupert Jones and Mr. C. D. Sherborne's paper "On the Foraminifera, with especial reference to their variability of form, illustrated by the Cristellarians," was read.—Mr. G. Massee gave a *résumé* of his paper on the genus Lycoperdon, illustrating the subject by drawings on the blackboard.—Prof. Bell said that the Fellows of the Society would remember that in the course of last winter he described what he had observed in some diseased grouse which had been sent to him for examination. Within the last few weeks, the disease, whatever it might be, had been killing grouse in considerable numbers on the moors in the south-west of Scotland. He had received some of these grouse, and examined them very carefully to see if he could discover any cause of death. In the case of the first, though there were tapeworms, there was no evidence that they were the cause of death; in the second case the birds had died from inflammation of the intestines, the cause of which was not quite clear; and in the third case they died of *Strongylus*. It would therefore appear that what was called "grouse disease" must be either more than one disease, or it must be a disease which could kill its victims in different stages. He was himself disposed to think that there was more than one cause of disease, but up to that time there was no diagnostic sign internally to show conclusively what those causes were.—Mr. Grenfell's paper on "New species of *Scyphydia* and *Disophysis*" was read.

Paris.

Academy of Sciences, June 27.—M. Hervé Mangon in the chair.—Remarks accompanying the presentation of a volume on the geodetic and astronomic junction of Algeria with Spain, by General Perrier. In this work, published at the joint expense of the French and Spanish Governments, a detailed account is given of the methods of observation and of the results obtained by the protracted operations which secure for the physical science of the globe the accurate measurement of an arc of the meridian of over 27° comprised between the Shetland Islands and Laghwat in Algeria.—Remarks accompanying the presentation of the first volume of a course of infinitesimal analysis intended for the use of persons who study this science with a view to its mechanical and physical applications, by M. Boussinesq. This work, the first volume of which deals with the differential calculus, is addressed more especially to those physicists, naturalists, engineers, and others, who are little accustomed to the treatment of algebraic formulas, but who, for their special purposes, feel the want of understanding in its essence and chief results the calculus of the infinitely little, or of continuous functions.—Memoir on submarine sound-signals, by M. Brillouin. The two chief results already obtained are transmission of sound to a distance of thirty-five kilometres, and the neutralization of all violent surface disturbances, such as thunderstorms or hurricanes. A summary description is given of the apparatus, together with a general statement of the circumstances under which these signals might be used with advantage.—Observations of Barnard's Comet (May 12, 1887), made at the Bordeaux Observatory with the 0·38 m. equatorial, by MM. G. Rayet, Flamme, and F. Courty.—Observations of a planet sighted at the Observatory of Marseilles, by M. Borrelly. The observations of this body, which is of the twelfth magnitude, extend from June 9 to June 19 inclusive.—On linear differential equations of the third order, by M. Paul Painlevé. In supplement to the paper published in the *Comptes rendus* of May 31, the author here deals with the linear and homogeneous equation of the third order—

$$y''' + ay'' + by' + zy = 0;$$

and he arrives at the general conclusion that, given a linear and homogeneous equation of the third order, it may always be ascertained, by a limited number of purely algebraic operations, whether its integral be algebraic, or the equation may be reduced to a quadrature.—Determination of the quantity of bisulphate of potassa in a diluted liquor, by M. E. Bouty. The author here deals with the difficulty of determining this quantity, which arises from the fact that in diluted solutions the bisulphate of potassa is always accompanied by sulphuric acid and sulphate of potassa. He shows that the bisulphate is stable especially in hot and concentrated liquors, and that the proportion of this salt increases with the excess of one or other of the reacting bodies.—On the ammoniacal vanadates, by M. A. Ditte. The vanadates here treated are those of methylamine, of ethylamine, ammoniaco-magnesian vanadate, and the double ammoniacal vanadates. The general study of these vanadates, prepared by the dry and wet processes, shows that all these compound bodies are reducible to a few well-defined types and simple formulas, such as: $3VO_5,MO$, $2VO_5,MO$, $3VO_5,2MO$, for the acids; VO_5,M the neutral vanadates; $VO_5,2MO$, $VO_5,3MO$, $VO_5,4M$ the basic salts, apart from the water, the quantity of which according to the circumstances in which the crystallizat effected.—Solubility of uric acid in water, by MM. Ch. and G. Denigès. For the determination of this point the a have applied the process of analysis by chameleon indica their previous note.—On the hydrochlorate of ferric chlori M. Paul Sabatier. M. Engel having recently announce he had succeeded in preparing this substance, which had anticipated but not isolated by M. Sabatier, the author re that so early as 1881 he had obtained and fixed the compo of the hydrochlorate of ferric chloride (*Bulletin de la Chimique*, second series, p. 197, 1881).—On the ident dambose and inosite, by M. Maquenne. A careful stud remarkably pure specimen of dambonite prepared accordi M. Girard's indications from the caoutchouc of the Gaboon, that dambose is identical in every respect with the inosite a described by the author. Hence dambonite should be cons as the dimethylene of inosite, and the term dambose shou replaced in chemical nomenclature by that of inosite, whi the right of priority and is in other respects more convenie

BOOKS, PAMPHLETS, and SERIALS RECEIV

Fungi, Mycetozoa, and Bacteria: A. de Bary; translated by G and Bayley (Clarendon Press).—Physiology of Plants: J. von Sachs lated by H. Marshall Ward (Clarendon Press).—La Exposicion N de Venezuela en 1883, tomo i., Texto: A. Ernst (Carácas).—Transacti Proceedings of the Royal Society of Victoria, vol. xxiii. (Williams an gate).—Transactions of the Astronomical Observatory of Yale Univ vol. i. part 1 (New Haven).—Report on the Mining Industries United States: R. Pumpelly (Washington).—Proceedings of the Am Association for the Promotion of Science, August 1885 (Salem).—Bi in England: E. L. Arnold (Chatto and Windus).—Text-book of Gu new edition: Major G. Mackinlay.—The Owens College Course of Pr Organic Chemistry: J. B. Cohen (Macmillan).—Hay Fever and P mal Sneezing, 4th edition: Morell Mackenzie (Churchill).—Technical and College Building: E. C. Robins (Whittaker).—Melting and Point Tables, vol. ii.: T. Carnelly (Harrison).—Smithsonian Report part 1 (Washington).—A Contribution to the Study of Well-Waters son).—Peabody Institute, Baltimore, 26th Annual Report.—Electric Primer: C. S. Levey (New York)—Beiblätter zu den Annalen der und Chemie, 1887, No. 6 (Barth, Leipzig).—Beiträge zur Biolog Pflanzen, v. Band, 1 Heft (Breslau).—American Journal of Mathe vol. ix. No. 4.

CONTENTS.

THURSDAY, JULY 14, 1887.

ELEMENTARY PRACTICAL PHYSICS.

Lessons in Elementary Practical Physics. By Balfour Stewart, M.A., LL.D., F.R.S., Professor of Physics, Victoria University, the Owens College, Manchester, and W. W. Haldane Gee, B.Sc., Demonstrator and Assistant Lecturer in Physics, the Owens College. Vol. II. Electricity and Magnetism (London: Macmillan and Co., 1887.)

THE second volume of the now familiar "Stewart and Gee" has at length appeared, and it is satisfactory to find that the hopes and expectations to which a just appreciation of the first has given rise have not been formed in vain, but that the same store of exact information down to the minutest details is to be found in "Electricity and Magnetism" as in "General Physical Processes."

While in this the second volume the authors "have adhered to the plan of subdivision into a series of lessons each descriptive of something to be done by a definite method with definite apparatus," they have treated the subject, to a certain extent, twice over –in the first three chapters generally, with simple and easily extemporized apparatus, and in the remaining chapters as exactly as possible, with all that care and attention to the details of standard patterns of instruments which are essential to the claim, in the title, of "practical." Thus in the second chapter instructions are given for measuring M and H by the method of Gauss without any bought apparatus, while in the sixth chapter is to be found a most complete description of the Kew unifilar magnetometer with figures of the instrument in its two positions and of its parts. There is here a fully worked example, from which a student who has not the advantage of using such an instrument may get an idea of the accuracy obtainable, and from which he may realize the relative importance of the numerous corrections which are applied. To facilitate the calculation of these corrections tables are supplied which will be of value to those using the magnetometer.

Though all will agree that a little practical introduction to the subject generally is of advantage to the student, and that therefore these three introductory chapters serve a really useful purpose, many will question the wisdom of devoting space in an essentially practical book to an explanation of such terms as electromotive force, conductivity, resistance, or of the theory of the battery or the meaning of Ohm's law. These are text-book matters for which a student does not depend on a practical book, nor is he expected to do so. The ten pages devoted to these points are in fact ten pages wasted.

The fourth chapter deals with the measurement of resistance. As this extends over 108 pages, and is divided into sixteen "lessons," it is clear that this very important branch of the subject receives its fair share of attention. The measurement of the resistance of every kind of material, from thick copper bars to insulators, is fully set forth. It is in this chapter that an explanation is given of the method of putting up a reflecting galvanometer and its several adjustments. The directions for fixing the instrument in the most perfect way are excellent. Not a word is said, however, to show that for many purposes such a galvanometer set up in any azimuth and almost anyhow is not less serviceable than when arranged as described. No trouble should be spared, and none is spared by the authors, in showing how to do anything which shall improve the accuracy of work done; but in some cases a mass of elaborate adjustment serves no purpose whatever, and then it should be pointed out that, though useful for this or that purpose, such adjustment may be dispensed with.

In the chapter on the tangent galvanometer the construction of standard and ordinary instruments is of course explained, and the method of using them. Their application to the determination of electro-chemical equivalents and of Joule's equivalent is given here. In the account of the method of finding the quantity of heat developed there is, apparently, a slight oversight. The authors speak of the mean deflection during the time the experiment lasts. If the current is practically constant the mean deflection may be taken without appreciable error; but if it varies, then the square root of the mean square truly represents the heating effect of the current. It is true that it should be the aim of the experimenter to avoid such variations; but if for some reason they should occur, the student should be told how to make the best of his experiment.

In the sixth chapter, already referred to in part, we find a most admirable description of the methods of determining the magnetic elements. This chapter leaves nothing to be desired.

The chapter on electro-magnetic induction contains an account of a great many experiments on induction of magnetism by currents, of currents by currents, and of currents by magnets; but, as in the third chapter, space is devoted to matters which might with advantage be left to the ordinary text-books. For instance, there is no necessity to prove the expressions for the ballistic galvanometer, or to explain the theory of damping and logarithmic decrement. The determinations of the coefficients of self and mutual induction form the subject of one lesson only. This part may now with advantage be greatly extended, since, lately, Prof. Foster has brought the subject before the Physical Society, and many others have followed suit.

In the last two chapters the condenser and electrometer are treated.

A good deal of useful matter is to be found in some of the appendixes. In the first we find the Wheatstone net and Kirchoff's laws; in the second and third, the theory of the electrical units. The fourth will be found the most valuable, in the laboratory, as there is here much additional information on the comparison of electromotive forces and the construction of standard cells. That on additional practical details is useful as far as it goes.

There is nothing about electro-capillarity, or about instruments depending upon any action of the kind; there is practically nothing about the electro-dynamometer, and there is no index.

Though a few faults have been found, they are mostly unimportant; and it is a matter of opinion whether some, especially the introduction of theoretical explanations into a book intended for use in the laboratory are faults at all. The book will be found to be of the greatest service in every physical laboratory, and to be a fitting companion to that already so well known. It is to be hoped that the remaining volume, on light, heat, and sound, will soon be ready.

THE ROYAL BOTANIC GARDEN, CALCUTTA.

Annals of the Royal Botanic Garden, Calcutta. Vol. I. The Species of Ficus of the Indo-Malayan and Chinese Countries; Part I. Palæomorphe and Urostigma. By George King, M.B., F.L.S., &c., Superintendent of the Royal Botanic Garden, Calcutta. (London: Reeve and Co., 1887.)

DR. KING deserves well of botanists for his protracted, though evidently profitable, labours on so varied and difficult a genus as *Ficus*. From obvious causes, a large proportion of the large arboreous tropical genera of plants are still very imperfectly known, and, prominent among them, *Ficus*; therefore Dr. King could hardly have extended his researches in a more useful direction. The present publication, which, from its general title, we may assume will not be limited to a monograph of the Asiatic species of *Ficus*, is a tall quarto of sufficient size to illustrate adequately almost all the species of the genus in question. Indeed, this monograph possesses a quite special value, inasmuch as every species is carefully figured in natural size, with enlarged analyses of the floral structure.

Most persons interested in such matters will be familiar with Fritz Müller and Solms Laubach's investigations of the sexual conditions in the flowers of various species of *Ficus*, and the singular phenomena attendant on the fertilization of the ovules. Nevertheless, it may be convenient to give here a brief account of the process.[1] The edible fig, which may be given as an example of the fruit of the genus generally, consists of a thick hollow receptacle, the inner surface of which is thickly studded with flowers; and, in the edible fig, exclusively with female flowers. Male flowers of this species of fig are borne on different plants, called the caprifig; and associated with these male flowers in the same receptacles are numerous female flowers, occupying the greater part of the space. Invariably these female flowers are infested by gall-producing insects, hence they are termed gall-flowers, and very rarely indeed is a single ripe seed found in a receptacle of the caprifig. The insects hatched and nourished in the gall-flowers leave the receptacles of the caprifig at a period when the pollen of the male flowers is being shed, and in making their exit bear some of it with them to the receptacles of the edible fig, which they next visit; but they are unable to deposit their eggs in the perfect females, and only serve to convey pollen to them. On similar mutual adaptations the fertilization of all the species of *Ficus* seems to depend.

In an introduction to the descriptive part of his work, Dr. King details the results of his own examination of several hundred species, extending over some nine years; and he states that Solms-Laubach anticipated him only in his explanation of the true nature of the "gall-flowers," for he had found them in every species of the genus that had come under his notice. He also enters into some further particulars concerning the insects acting in the process of fertilization, though he adds nothing more conclusive. While admitting, and even assuming, that the pollen of the males must be conveyed by the insects developed in the gall-flowers "to the perfect females imprisoned in the neighbouring receptacles," he is still puzzled as to the way in which it is done. We are under the impression that Solms-Laubach indicates, if he does not actually state in so many words, that he had not only frequently seen the winged female insect issuing from the receptacles of the caprifig, but that he had likewise occasionally observed them enter the receptacles of the cultivated fig, which is the female of the same species.

[1] Further details will be found in NATURE, vol. xxvii. p. 584.

This, the first part of King's monograph, contains descriptions and figures of seventy-six species of *Ficus*, whereof ten belong to his section *Palæomorphe*, and the rest to *Urostigma*, which was originally proposed as an independent genus by Gasparrini, and provisionally retained as such by Miquel. King found five different kinds of flower, variously associated or removed, in the Asiatic species of fig; and upon characters derived from the differentiation and arrangement of the sexual organs he classifies the species in two primary groups and seven sections. The species of the relatively small group *Palæomorphe* are distinguishable from all the others by having spuriously bisexual flowers associated with gall-flowers, while the fertile females occupy separate receptacles. In the definitions of the sections, the pistil in the functionally male flowers is described as rudimentary, though perhaps sterile would be a better term to use, because, as figured, and designated in the explanations of the figures, it is a fully-developed gall-pistil. This condition is regarded as the nearest approach to assumed original complete hermaphroditism.

In all six sections of the larger group the sexes are strictly separated, as to the individual flowers; and in the section *Urostigma*, male, gall, and perfect female flowers are intermingled in the same receptacles. We have overlooked it if there is any explanation of the advantage derivable from the presence of gall-flowers where both sexes are also found in the same receptacle; but it may, perhaps, be found in the fact that the inflorescence is proterogynous or proterandrous, hence insect agency is as necessary as in those species where the sexes are in different receptacles.

In the remaining five sections the male and gall-flowers are invariably borne in one set of receptacles, and the fertile female flowers in another set; and the presence of neuter flowers in the female receptacles characterizes the section *Synœcia*. The neuter flowers contain rudiments of neither sex, which condition King explains by saying the neuter flowers are asexual.

Neuter flowers are wanting in the sections *Sycidium*, *Covellia*, *Eusyce*, and *Neomorphe*; but the arrangement of the flowers is otherwise the same as in *Synœcia*. The two first of these sections have monandrous male flowers, and the two last have diandrous or triandrous male flowers: while the receptacles of the first and third are mostly axillary, those of the second and fourth are usually borne in fascicles on the stem and branches. Thus it will be perceived that the distinctive characters of these four sections are somewhat artificial. However, it is only fair to say that the author himself points out this fact.

We have very little to say except in favour of this work, which is certainly one of the most important of recent contributions to systematic botany; but we should have liked to see a closer adherence to established usage in the application of certain botanical terms. To use the terms monœcious and diœcious in relation to the individual receptacles as well as the whole tree is perplexing, and also unnecessary, because suitable terms for expressing these distinctions are current, and even employed by the author himself in some passages. W. B. H.

OUR BOOK SHELF.

Year-book of Pharmacy for 1886. (London: Churchill, 1887.)

General Index to Year-books of Pharmacy, 1864–1885. (London: Churchill, 1886.)

THE "Year-book of Pharmacy" for 1886 contains a larger number than usual of abstracts of papers. Amongst the most interesting of them are perhaps those treating of coca and substances obtained from it. It appears that when the active principle, cocaine, is heated with water it decomposes, losing methyl (CH_3), which is replaced by hydrogen. The product of this decomposition is benzoyl-ecgonine, which can again be converted into cocaine by heating with methyl iodide and methyl alcohol. The replacement of methyl by hydrogen in the conversion of cocaine into benzoyl-ecgonine produces a very marked change in the physiological action of the substances, for while cocaine is distinguished by its extraordinary power of paralyzing the sensory nerves and thus producing anæsthesia of any part to which it is applied, this power is completely absent in benzoyl-ecgonine. Benzoyl-ecgonine, however, has a physiological action very closely allied to that of caffeine—a circumstance which is very interesting in relation to the use of coca and coffee as a beverage.

Another substance used as an intoxicating drink in the South Sea Islands—namely, Kava, obtained from the root of *Piper methysticum*—has been found, like cocaine, to have a powerful local anæsthetic action.

Other abstracts of great interest are those which relate to ptomaines and leucomaines, or alkaloids formed from the decomposition of albuminous matters either outside or inside of the body. These alkaloids are becoming more and more important from the fact that they are now recognized as not only causing poisoning where meat has been taken in a state of putrefactive change, but as causing abnormal symptoms in some diseases. Thus it has been found that in typhoid fever a large quantity of ptomaines occur in tbe fæces, and it is supposed by one writer that the utility of *tisanes* in illness may be due to their aiding the removal of these alkaloids from the body through the kidneys.

By cultivating the comma-bacillus in broth, an alkaloid has been obtained which appears to be identical with that already isolated from the dejecta of cholera patients. In relation to these alkaloids produced in the body, it is very interesting to note that alkaloidal substances may be formed by the action either of ammonia or of compound ammonias on glucose.

A number of new alkaloids have been isolated from plants, and the actions of several of these are described.

The General Index to the "Year-books of Pharmacy" for the Years 1864–1885 inclusive is of great service, saving much time, and enabling one not only to find any paper readily, but to see at a glance what work has been done on a particular subject within the last twenty years.

A B C Five-Figure Logarithms. By C. J. Woodward (London: Simpkin Marshall and Co., 1887.)

TO those who work in physical and chemical laboratorie this little book will be an immense help, for, in th ordinary work of the laboratory, errors of experimen exceed any error of calculation introduced by five-figur logarithms, while the time saved in calculation is ver great.

The tables are indexed ledger-fashion, so that the re quired mantissa may be found in a moment. The differ ences for the 5th and 6th figures of sequences are found b using side letters denoting the line at the foot of eac table in which the required difference is presented. Muc greater accuracy is obtained by the last figure of certai mantissæ having dashes above and below to indicat departures from the normal difference. At the end ar added a few chemical and physical constants and table including some on gas analysis.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinio expressed by his correspondents. Neither can he unde take to return, or to correspond with the writers o rejected manuscripts. No notice is taken of anonymo communications.*

[*The Editor urgently requests correspondents to keep the letters as short as possible. The pressure on his spa is so great that it is impossible otherwise to insure t appearance even of communications containing interestin and novel facts.*]

Lighthouse Work.

IN the second of the very interesting articles on "Lighthous Work in the United Kingdom," by Mr. J. Kenward, which ha appeared in your pages, some words are used, not intentionall I believe, but which, by those who are unfamiliar with t subject, might be construed in such a way as to deprive the la Mr. Thomas Stevenson of the credit due to him as the invent of the dioptric mirror. The following is an extract from M Thomas Stevenson's "Lighthouse Construction and Illumin tion," published in 1881, which puts the matter on a corre footing:—

"*Mr. J. T. Chance's Improvements of* 1862 *on Stevenson Dioptric Spherical Mirror.*—Mr. Chance proposed to genera the prisms of the spherical mirror round a vertical instead of horizontal axis, and also to arrange them in segments. He sa (*Min. Inst. Civ. Eng.* vol. xxvi.):—'The plan of generati the zones round the vertical axis was introduced by the autho who adopted it in the first complete catadioptric mirror whi was made, and was shown in the Exhibition of 1862 by t Commissioners of Northern Lighthouses, for whom it was co structed, in order to further the realizing of what Mr. Thom Stevenson had ingeniously suggested about twelve yea previously. During the progress of this instrument the id occurred to the author of separating the zones, and also dividing them into segments like the ordinary reflecting zones a dioptric light; by this means it became practicable to increa considerably the radius of the mirror, and thereby to rend it applicable to the largest sea light, without overstepping t limits of the angular breadths of the zones, and yet witho being compelled to resort to glass of high refractive power.'

"There can be no doubt of the advantage of these improv ments, and it is without any intention of derogating from M Chance's merit in the matter that it is added that my first id was also to generate the prisms round a vertical axis. But t flint glass which was necessary for so small a mirror cou not be obtained in large pots, and had to be taken out in ve small quantities on the end of a rod and pressed down into t mould. I was therefore obliged to reduce the diameter of t rings as much as possible; and it was thought by those whom consulted at the time (Mr. John Adie, Mr. Alan Stevenson, a Prof. Swan) that by adopting the horizontal axis the mo important and most useful parts of the instrument near the ax would be more easily executed, inasmuch as those prisms we of very much smaller diameter. Mr. Chance not only adopt the better form, but added the important improvement of separating the prisms and arranging them in segments."

Edinburgh, June 28. D. A. STEVENSON.

IN addition to several errors into which Mr. Kenward, in his third article on "Lighthouse Work," has fallen, he seems to have overlooked the experiments made by Messrs. Stevenson, in 1870, on paraffin as an illuminant for lighthouses, and which were fully detailed in the Parliamentary Paper 318, Session 1871. Experiments had been made with some degree of success with burners having one and two wicks, but all attempts to burn paraffin efficiently in the large concentric-wick burners were unsuccessful until Capt. Doty solved the problem. Unaware of what had been done in France, Messrs. Stevenson, early in 1870, had been conducting a train of experiments on paraffin, and had reached important conclusions on the subject, and good flames were got with the single and double Argand lamps, when Capt. Doty submitted his burners to them. The Doty burners were then subjected to crucial tests in Edinburgh, and also to actual trial for a month in a first-order lighthouse. The conclusions Messrs. Stevenson then arrived at and reported to the Scottish Lighthouse Board may be summarized as follows: that paraffin as now manufactured, with a high flashing-point, is safe and suitable as a lighthouse illuminant; the flames of the Doty burners are of great purity and intensity, and easily maintained at the standard height; the lamp-glasses and lamps in use for colza are equally suitable for paraffin; the varying state of the atmosphere does not affect the penetrability of the paraffin light more than the colza light; no structural alterations on the existing apparatus are necessary; the initial power of the lights will be exalted from 10 per cent. in the four-wick burner to fully 100 per cent. in the single-wick burner; and that the use in the Scottish lighthouses of the new illuminant would effect an annual saving of £3478. These conclusions, which subsequent experience has fully borne out, settled the relative merits of paraffin and colza so far as British lighthouses were concerned; and the first four-wick paraffin burner ever permanently installed in a lighthouse was at Pentland Skerries on February 15, 1871, while Argand paraffin burners were in use at Pladda in December 1870, and at the catoptric lights of Great Castle Head in December 1870, and at Flamborough Head in June 1872.

With reference to Ailsa Craig the facts are that in 1878, when Messrs. Stevenson were considering the problem of effectively guarding the Fair Isle by fog-signals, they consulted Prof. Holmes as to the feasibility of working the signals from a central station and sending the compressed air through a long length of piping, and he concurred with them regarding its practicability, and stated that he had worked a signal in Canada at a distance of half a mile. When Ailsa Craig came to be dealt with, the Fair Isle scheme was reverted to, and Mr. Ingrey's firm contracted to carry out the work in accordance with Messrs. Stevenson's specification. The automatic appliances for securing the true periodicity of the siren blasts were designed by Mr. Ingrey.

In giving the history of gas-engines applied as a motive power for actuating fog-signals, a most important advance in lighthouse work, Mr. Kenward does not state that this was done on the Clyde by Messrs. Stevenson in 1875, and that since then they have introduced gas made from mineral oil for driving gas-engines at Langness in 1880, at Ailsa Craig, and at the Clyde.

D. A. STEVENSON.

84 George Street, Edinburgh, July 4.

The Use of Flowers by Birds.

I HAVE just read in NATURE of June 23 (p. 173) Mr. W. White's letter, and should like, with your permission, to add a few words on this subject. A quiet, leafy home has made me well acquainted with the commoner birds, therefore I speak. In the first place, with regard to the non-protective colour of the laburnum blossoms, it must be remembered that the flowers thus used have two other qualities that recommend them to the nest-builders: flexibility and length. Everyone must have noticed how sparrows and other birds steal anything long and limp—pieces of string, &c.—when they are building. Only the other day I caught a sparrow trying hard to untie a piece of thick string with which the branch of a tree had been tied back, and it would have succeeded if I had not gone to the rescue. I have had the ties of budded roses taken away by them also. I have been told by a lady that she once lost a lace handkerchief in a mysterious manner, which was at last discovered—through a telescope—on a high tree, on the nest of a rook or daw. All the flower-sprays mentioned were long and limp. I have seen birds take those of the clematis also.

But there can be no doubt that birds have a very keen sense of the protectiveness of colour; if you startle a blue tit it will seek a high branch against the sky—blue, and brown, and green; a robin flits away to the brown shadow of a bush; I have even known a young robin, threatened by an elder (they are great disciplinarians), take refuge near a reddish-brown dress.

A thrush is wonderfully clever almost as soon as it is fledged in finding its own tints on some wall or tree-trunk, and making believe to be a piece of it to such an extent that one may approach quite close to it and it will remain absolutely motionless as long as one's eye is upon it; but if the eye is removed, even for a "twinkling," the bird will have hopped down noiselessly behind something before one can look again.

With respect to the yellow flowers, may there not be some quality attached to the colour that birds like, or find profitable? I have watched a thrush during a long hard frost, *devour*—not merely pull to pieces, but eat voraciously—large bunches of yellow crocuses. All the earlier bunches were eaten. When the purple and white came out later it was still faithful to the yellow, and never touched any other; and so eager was it, that when the blossoms were gone it would dig its beak down into the buds and pull out the least bit of yellow that appeared. I watched it from a window close above the bed, and there was no possibility of making any mistake about it. The bird—a very large one—took some again this year, but not many. It could hardly be all for love of colour, though no doubt that is very strong in birds as in children. Birds are very like children.

The sparrows mentioned in my last note made two more trials after I sent it—five in all; and the last time their attempt was nearly composed of white alyssum. After that they gave it up, but I get a severe scolding from them sometimes if I go near the place. They tried to build there last year, and I removed two or three nests, but I allowed a thrush, that had built below and brought forth a brood before I perceived it, to remain. When they left, the sparrows immediately built *on the top* of the forsaken thrush's nest. They seem to have drawn the conclusion—rather hastily, but not irrationally—that *that* was a safe place, and whether or not their thoughts took the shape of words, they chattered over their work immensely. And I do not know where the line can be drawn between words and exclamations (the foundation-stones of language), nor between those and the notes and cries of birds, which are much more numerous and varied and distinctive of purpose than most people imagine, especially those of the robin. The strangely human and canine cries of a party of quarrelling sea-gulls are extremely expressive.

It may be said that there is no progress, no addition to the language of birds; but I am not sure of that. Last winter, a robin, accustomed to be fed at my window on bits of bacon, invented a note by which it called me to feed it. It was quite peculiar—hushed, short, and muttered, as it were. Its object seemed to be to reach my ear and not that of rival birds. It would take a few little bits—very few—when offered, look gratefully in my face, with its head on one side, and away, till it was again hungry; then—*da capo*. The same robin is hopping in and out of the open window continually now, taking what it pleases for itself and young of food set for it.

That birds should be subject, like ourselves, to the tyranny of fashion seems not at all unlikely if one considers the nature of that tyranny. The feeling that seems to oblige people to adopt, notwithstanding their sense of beauty and fitness, fashions that are positively monstrous, must have its roots low down in the scale of Nature. It seems to be composed of a sense of association and a love of the accustomed—both very strong in birds; association, for instance, of wisdom and authority with a wig, of the delightfulness of well-bred women with the extremely undelightful outlines they contrive to give to their figures, &c., &c. The pleasure that the accustomed gives is, I suppose, that of rest. No doubt fashion may reign in the lower regions; may it not control, in a somewhat transient manner, the bee that packs its load from the pollen of a particular flower, of one colour and no other? J. M. H.

Sidmouth, July 3.

Spawn of Sun-fish (?).

DURING a cruise on the west coast of Ireland, from which I have recently returned, I captured a long ribbon of spawn about 40 feet long, 3 feet deep, and a quarter of an inch thick. The ova, about the size of No. 2 shot, were set in a firm gelatinous mass, which floated edgeways in a frilled form. I saw it floating about a foot below the surface, and succeeded in gaffing it and towing it behind the punt by getting some of it fixed over the gunwale. The embryos had developed so as to show eyes when first taken, and in the two days, during which some of it remained alive in a deep can, a further advance took place; but then, owing to the heat of the weather, the ova whitened, its buoyancy was lost, and decomposition set in.

As we saw several specimens of the sun-fish (*Orthagoriscus mola*) in the vicinity, and as the spawn must have belonged to some very large fish, I think it probable that what we found was the spawn of a sun-fish.

I should be glad if any of your readers could throw more light on the subject. W. S. GREEN.

Carrigaline, co. Cork, July 4.

After-Glows.

IN reply to the letter of Mr. L. P. Muirhead in NATURE of June 23 (p. 175), I would ask to be permitted to state that the after-glows are very rich and conspicuous here evening after evening, and occasionally discernible till 10 p.m.

Worcester, July 4, 1887. J. LLOYD BOZWARD.

The Cuckoo in India.

I HAVE been here for just one month, and during that time have constantly heard the cry of the cuckoo. Last Sunday I heard it at Lackwar, fifteen miles from here. This would apparently point to Jerdon's not being correct in saying the cuckoo is rare in India. F. C. CONSTABLE.

Mussoorie, June 15.

Mr. Mutzler, the owner of this hotel—the Charleville—tells me the cuckoo is constantly heard from spring to October.

Luminous Boreal Cloudlets.

IN NATURE, vol. xxxiv. p. 192, attention was invited by the writer to what appeared to him to constitute a special class of self-luminous cloudlets in the northern sky at night, for which, if so recognized, the name "nubeculæ boreales" was suggested.

A careful look-out was kept every night last autumn, winter, and spring for their reappearance here, but to no effect till the night of the 19th inst. Then, and subsequently on the 21st, 24th, and 26th inst., there was an increasing development of the phenomenon in a north polar horizontal arc of 50°, or 25° on each side the true north. At length on the 28th, and last night, the 29th inst., there was a magnificent and marked display.

One of your able correspondents of last year seemed to consider he had already drawn attention to the subject in a previous year in your columns. It appeared, however, he had only remarked upon sunlit clouds, as a phase of the cloud-forms attracting latterly special attention.

It is quite out of the question to attribute the luminosity now referred to in any respect to direct solar illumination at midnight; and fortunately the eminent Astronomer-Royal for Scotland was led to apply the spectroscope, confirming the writer's conjecture as to the sub-auroral and self-luminous character of these cloudlets. His letter of July 31 will be found in NATURE, vol. xxxiv. p. 311.

The recent works of Lemström and Koch, reviewed in NATURE, vol. xxxv. p. 433 *et seq.*, have followed up the subject in noting a sudden and wide-spread development of cirrus clouds and luminous mists in auroræ of Sweden and Labrador.

Dundrum, co. Dublin, June 30. D. J. ROWAN.

The Migrations of Pre-Glacial Man.

WILL Dr. Hicks kindly explain the statement cited in NATURE (vol. xxxvi. p. 185), that the migration of pre-glacial man to this country was "*from* northern and north-western directions."

June 25. GLACIATOR.

On the Pliocene Deposit of Marine Shells near Lattakia, and a Similar Deposit in the Island of Zante.

ON p. 384, vol. xxx. of NATURE, Prof. Hull published an account, furnished him by myself, of the shell deposit in the marl of the Lattakia plain. Since that time I have submitted these specimens to Mr. Etheridge, F.R.S., of the British Museum, who has kindly furnished me with their specific names, *as far as they are determinable.* The subjoined list fixes the geological date or succession of the deposit, which belongs to, or is of the same age or period as, the Pliocene or Crag deposits of Essex, Norfolk, and Suffolk. The fossils from the raised beaches may be of post-Pliocene.

MOLLUSCA.

Class I.—*GASTEROPODA.*

Order I.—PROSOBRANCHIATA.

Sec. A.—**Siphonostomata.**

1. Fam. STROMBIDÆ.

1. *Strombus*, sp.
2. ,, ,,

2. Fam. MURICIDÆ.

3. *Murex branderis*, Brocc.
4. ,, *erinaceus*, Linn.
5. ,, *conglobatus*, Micht.
6. *Fusus rostratus*, Defr.
7. ,, *corneus*, Sow. = *F. gracilis.*
8. ,, sp.
9. *Ranella marginata*, Sow. or Brocc.

3. Fam. BUCCINIDÆ.

10. *Buccinium flexuosum*, Brocc.
11. *Cassis crumona*, Lam.
12. *Cassidaria echinata.*
13. *Columbella nassoides.*
14. *Nassa clathrata*, Defr.
15. ,, *megastoma*, Brocc.
16. *Terebra imbricaria.*
17. ,, near *T. plicaria.*
18. ,, sp.

4. Fam. CONIDÆ.

19. *Conus Noë*, Brocc.
20. ,, *deperditus*, Brig.
21. ,, sp.
22. *Pleurostoma monile*, Brocc.
23. ,, *cataphractra*, Brocc.
24. ,, *turricola*, Brocc.

5. Fam. VOLUTIDÆ.

25. *Mitra scrobiculata*, Defr. (Brocc).
26. ,, sp.
27. ,, sp.

Sec. B.—**Holostomata.**

6. Fam. CERITHIIDÆ.

28. *Aporrhais* (*Chenopus*) *pes-pelecani*, L.
29. *Cerithium vulgatum*, Brug.

7. Fam. NATICIDÆ.

30. *Natica*, sp.

8. Fam. LITTORMIDÆ.

31. *Phorus agglutinans*, Lam.

9. Fam. TURBINIDÆ.

32. *Turbo rugosus*, Lam.
33. ,, sp.
34. *Trochus patulus*, Brocc.

Class II.—*CONCHIFERA*, Lam.

Sec. A.—**Asiphonida.**

10. Fam. OSTREIDÆ.

35. *Ostrea*, sp.
36. *Spondylus crassicostata.*

11. Fam. PECTENIDÆ.

37. *Pecten*, sp. near *P. altoplicatus.*
38. ,, *jacobæus.*
39. ,, *opercularis*, L.
40. ,, *dubius*, Brocc.
41. ,, *janira*, near *quinquecostatus.*

12. Fam. ARCIDÆ.

42. *Arca polii.*
43. *Pectunculus*, sp.

Sec. B.—**Siphonida, Integro-pallialia.**

13. Fam. CHAMIDÆ.

44. *Chama squamosa*, Brand.

14. Fam. CARDIIDÆ.

45. *Cardium rusticum*, L.
46. ,, *echinatum*, L.
47. ,, *edule*, L.

15. Fam. LUCINIDÆ.

48. *Lucina borealis*, L.

Sec. C.—**Sinu-pallialia.**

16. Fam. VENORIDÆ.

49. *Venus fasciata*, Da Costa.
50. ,, (*Cytheræa*) *casina*, L.

Class III.-*BRACHIOPODA*, Cuv.

51. *Waldheimia complanata.*

It will be seen by this list that three classes, seventeen families, twenty-nine genera, and fifty-one species are represented.

Beside the above marine species, which are found more or less embedded in the soil, as well as on its surface, *Helix pomatia* is found in great profusion all over the surface.

Another species of *Helix*, closely allied to *H. lapicida*, and a species of *Clausilia*. No other terrestrial shells were collected in this region.

In addition to the above Mollusca I found a species of *Toxas-*

ter, species of *Dentalium Noë*, and specimens of *Serpula*, also a large shark's tooth belonging to the genus *Carcharodon*.

During the autumn of 1885 I visited Zante in the Austrian Lloyd's steamer from Trieste to Athens. As the steamer only anchored for a few hours, I had time only for a walk to the top of the hill overlooking the town. A chain of hills trending nearly north and south forms the backbone of the Island of Zante. At the latitude of the town of Zante this chain is broken by a strip of alluvial plain about 2 miles wide, stretching from the eastern to the western coast of the island. The Castle hill is a mass of Pliocene marl, rising about 300 feet above this plain at its eastern edge. The steep side of the hill is channeled with innumerable ravines and gullies, and of the same colour as the Pliocene beds of Lattakia. In coming down the hill, I observed in one locality, within a radius of 30 feet, the following species:—

Cerithium vulgatum, Brug.
Murex conglobatus.
Cardium edule, L.
Venus (*Cytheræa*) *casina*, L.
Ostrea, sp.

All of these were more or less embedded, or had been worked out by the rain, and lay at the bottom or sides of the gullies.

London, June 23. GEORGE E. POST.

The Perception of Colour.

I HAVE not yet heard it stated that our perception of colour is slower for the blue and violet rays than for the green, yellow, and red ones; and as I think that this subject has interest for many of your readers, they will perhaps carry out the following simple experiments on themselves and their friends.

A luminous object, such as a distant gas-lamp, an electric light, or the moon, is looked at through a direct-vision prism after it has been removed out of its case. The spectrum is of course a bad one, but brilliant. Now, if the prism is rolled backwards and forwards between the fingers, so that the spectrum oscillates through a small angle, it appears to bend like a riding-whip which is being flicked from side to side. The blue and violet parts of the spectrum always lag behind. In fact, as far as I could see, the spectrum, instead of being straight, seemed to be gently curved, but very sharply bent between the indigo and the violet part, which would show that the more refractive rays are seen by us very much later (even proportionately) than the others.

As everybody is not able to detect this bending of the spectrum, the following experiment should also be carried out. Instead of rolling the prism, it is passed between the eye and the object as quickly as possible, so that the spectrum is only seen for an instant; and it will be distinctly noticed that it seems to flash from the red end towards the violet—a sure sign that the red is seen first and the violet last. C. E. STROMEYER.

Strawberry Hill, July 5.

Breeding for Intelligence in Animals.

SEEING the results that have been attained by breeding for special qualities in dogs, why should not systematic efforts be made to breed for general intelligence? The correspondents who have from time to time furnished you with illustrations of canine sagacity must be sufficiently numerous to form an Association to promote the interbreeding of intelligent dogs, and the distribution of their offspring to those who would foster and cultivate their intellect. H. RAYNER.

June 27.

The Nephridia of *Lanice conchilega*.

SINCE my paper on the nephridia of *Lanice conchilega*, Malmgren, appeared in NATURE (June 16, p. 162), I have learned that the chief peculiarity to which I called attention in my description of the nephridial system had been observed and mentioned before. In the monograph on the Polycladen by Dr. Arnold Lang, published in 1884, and forming one of the series "Fauna und Flora des Golfes von Neapel," p. 677, occurs the sentence: "Bei *Lanice conchilega*, Pallas, hat neuerdings Ed. Meyer bei erwachsenen Thieren jederseits einen Längscanal aufgefunden, welcher alle Segmentalorgane mit einander verbindet, und nur an einer Stelle durch ein Dissepiment unterbrochen ist." Dr. Ed. Meyer has called my attention to this passage, and informed me that Dr. Lang received permission from him to make use of this and other observations which he (Dr. Meyer) had made in the course of his studies on Chætopoda. The sentence quoted has been also cited by Dr. R. S. Bergh in an article on "Die Excretionsorgane der Würmer," in *Kosmos*, 1885, Bd. ii. p. 115. That sentence is the only account yet published concerning Dr. Meyer's observations on the nephridia of the species in question. When my paper was printed I was unaware of the existence of the sentence in Dr. Lang's monograph, or of the reference to it made by Dr. Bergh. Unfortunately I had not had time to read the monograph through, and had not suspected that there was in it a mention of a novel fact concerning the anatomy of Chætopoda. My examination of *Lanice conchilega* was made in entire ignorance that Dr. Meyer had already investigated its anatomy; otherwise I should of course have mentioned his name in the summary I gave of previous work on the subject. J. T. CUNNINGHAM.

Edinburgh, June 30.

THE PARIETAL EYE IN FISHES.

THE discovery of the parietal eye in lizards by de Graaf and Spencer is so recent that it is hardly necessary to preface an account of the structure of that organ in another group with the history of their researches.

Its high development in some lizards, and, so far as we know, its rudimentary nature in all other existing groups of vertebrates, including fishes and Amphibia, and lastly its entire absence in Amphioxus, are, for those who see in the latter the "Urvater" of the Chordata, points which made it difficult to form any satisfactory morphological conception of its origin.

True, something that admitted of comparison with it could be found in larval Ascidians; and Spencer, at the end of his able paper, endeavoured to trace its "rise and fall" from its supposed homologue, the larval Tunicate eye.

With Wiedersheim and Carrière, I consider that Spencer has placed the eye of the larval Tunicate at the wrong end of the series—if it should come in at all; for, as experience has abundantly shown, it is very easy to compare organs of the higher vertebrates with what are supposed to be homologous organs in Amphioxus and the Tunicata, and at the same time to be entirely in error. I need hardly refer the reader to the instances in which such comparisons have been shown by Dohrn in his famous "Studien" to have been entirely wrong; and holding with him that Amphioxus and the Tunicata are very degenerate vertebrates, and that from them but little can be got for the elucidation of the problems of vertebrate morphology, I felt the necessity of looking elsewhere for the solution of that of the parietal eye in its relations to the paired eyes.

With these problems in view I began to study the development of the pineal eye, and also its structure in such fishes as might be expected to retain it in a more developed condition than most of those we know.

At Prof. Wiedersheim's suggestion I examined the structure of the "pineal gland" in Ammocœtes of *Petromyzon planeri*, in the hope that something more might come out beyond that which the able work of Ahlborn has already made known to us. The result was, in a sense, disappointing, but not unexpected, for, remembering Dohrn's researches, and bearing in mind that the paired eyes of Petromyzon are rudimentary in Ammocœtes, first becoming capable of vision in the adult, I had firm hope of good results from the examination of sexually mature animals.

In the adult the discoveries made exceeded my expectations; and after examining this animal I proceeded to make sections through the brain of Myxine. Here, again, the finds were important, and the research was extended to specimens of *Bdellostoma* and *Petromyzon marinus*, which I owe to the generosity of my former teacher, Prof. Howes.

Before giving the detailed account of my investigations, I may say that neither the anatomical nor developmental studies so far made, give any direct clue to the origin of the organ.

That which seems to me the most likely hypothesis I shall give at the end of this paper, and in its favour I can at least say that it is a morphological explanation of the evolution of the parietal eye, which, so far as I know, is not inconsistent with any known facts.

The epiphysis in Ammocœtes has been described by Ahlborn (*Zeitsch. f. wiss. Zool.*, Bd. xxxix.). His description is mainly correct, and but little can be added to it. The epiphysis itself is divided into a dorsal and a ventral vesicle, and as we are not concerned here with the ventral one, I shall ignore its existence.

In large Ammocœtes the dorsal vesicle lies deep under the skin, and far removed from the light; its position being marked externally by a clear white spot just behind the opening of the nose.

It is a simple closed sac, and retains its attachment to the brain. The dorsal wall is thinner than the ventral, and is made up of a layer of flattened cells, which are not modified to form a lens.

The ventral wall is a much more complicated structure. Towards the inside of the vesicle it presents a layer of rod-like cells, which are more like the rods of a retina than like anything else. Externally (with regard to the vesicle) to this layer are two or three irregular rows of nuclei. There is no lens and no pigment, except a few very minute dots.

In this stage the retina of the parietal eye of Ammocœtes somewhat resembles that of *Cyclodus*, figured by Spencer, but is somewhat better developed, and tends towards the condition found in *Varanus giganteus*.

Except in the presence of the minute dots of pigment, and in the fact that the dorsal wall of the vesicle is not connected by fine strands with the ventral wall, as Ahlborn supposed, there is nothing new in this description, and even now we cannot say that the parietal eye of fully-grown Ammocœtes is very highly developed.

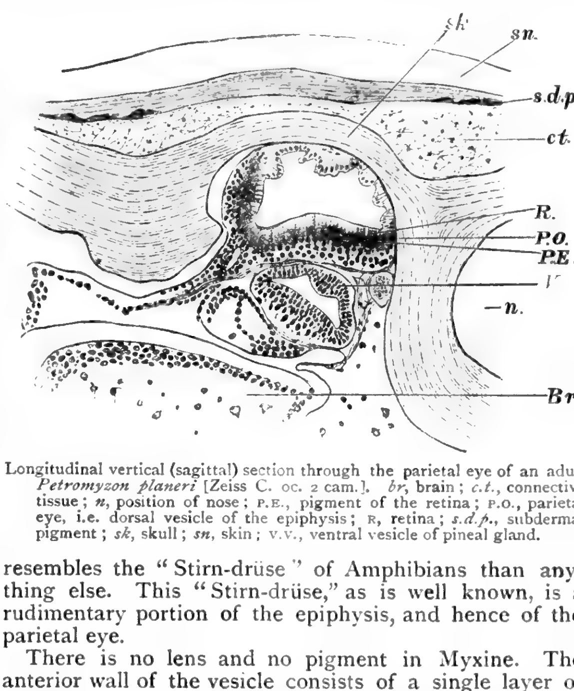
In the adult Petromyzon, just as the paired eyes are highly developed so also do we meet with an increased development of the parietal eye. As is well known since Wiedersheim's researches, the brain of the adult is much compressed in an antero-posterior direction. The dorsal vesicle of the pineal gland lies much further forwards, and more dorsally than in the larva, so that it comes to be nearer the external surface of the body, while it lies buried in the roof of the skull. Its posterior wall is densely pigmented, so much so that it is impossible by ordinary means to make anything out of the structure of the cells composing it. These points can be seen very plainly in longitudinal vertical sections through the brain and skull (see figure).

I ought to mention that the clear white patch of skin lying above the organ is much larger and more marked than in the Ammocœtes. It is, however, difficult to suppose that the white patch is here of much physiological importance, and it can only be referred back to a time when the eye in Petromyzon was of more use than at present. The anterior wall is composed of cells which are thrown into folds (possibly in part due to contraction) projecting into the cavity of the vesicle.

I mentioned above that in the full-grown Ammocœtes there are only a few minute dots of pigment present. So few and so small are these, that unless specially sought for they would be overlooked, as indeed they have been by previous observers. The state of things is much different in the young Ammocœtes of about 2 inches in length. There, as in the adult, the retina of the parietal eye contains a large deposit of pigment. This was first shown me by Dr. Schwarz (a pupil of Prof. Weismann's), who has made, for the study of the paire eyes, some very fine sections of very young Ammocœte at stages which I had failed to obtain. I shall figur these sections in the complete account I have in prepara tion. In the young Ammocœtes the parietal eye is large and exceeds in size either of the paired eyes. Its posterio wall is really a well-developed retina, with long rod-lik elements embedded in pigment, and a series of oute layers of spherical nucleated bodies. Its anterior wa consists of several layers of rounded cells, but it doe not form a lens.

In the specimen of *Petromyzon marinus* mentione before, owing to the soft state of the brain I could onl make out a very deep fossa in the skull in the position i which the "eye" is situated in *P. planeri*. The whit patch of skin is here very large indeed, and on the whol I am inclined to think that the parietal eye in *Petromyzo marinus* would well repay further investigation.

In Myxine the state of things is even more surprising Here the parietal eye is a large flattened vesicle lying o the brain and connected with it by a very short soli stalk. There is externally no white patch of skin, bu lying in the skin above the vesicle there is a flattene body, which, in structure and position, more nearl



Longitudinal vertical (sagittal) section through the parietal eye of an adul *Petromyzon planeri* [Zeiss C. oc. 2 cam.]. *br*, brain; *c.t.*, connectiv tissue; *n*, position of nose; P.E., pigment of the retina; P.O., parieta eye, i.e. dorsal vesicle of the epiphysis; R, retina; *s.d.p.*, subderma pigment; *sk*, skull; *sn*, skin; V.V., ventral vesicle of pineal gland.

resembles the "Stirn-drüse" of Amphibians than any thing else. This "Stirn-drüse," as is well known, is a rudimentary portion of the epiphysis, and hence of the parietal eye.

There is no lens and no pigment in Myxine. The anterior wall of the vesicle consists of a single layer o somewhat flattened cells.

The retina has essentially the structure of that of the parietal eye of Varanus, but it lacks the pigment which is there present (*vide* Spencer, "Pineal Eye in Lacertilia," *Q.J.M.S.*, vol. xxvii. Part 2, Plate XIV. Figs. 1 and 6).

Bdellostoma seems, in this, and, as was first shown by Johannes Müller, in other points in the structure of its brain, to resemble Myxine. Without discussing the matter at length, I may say that in the parietal eyes of Petromyzon and Myxine we have to deal with structures which are still well developed, and which were probably once much more developed than now. In this connexion the history of the changes in Ammocœtes is very interesting, and all the more so as confirming and extending Dohrn's opinion that the Cyclostomata have degenerated from highly developed fishes. The parietal eye in Ammocœtes, like many other of its organs, makes a good start, and only degenerates as the Ammocœte degenerates. When the Petromyzon state is reverted to, the parietal eye, like the animal in which it occurs, reverts towards an ancestral condition, and its doing so is an additional point in favour of Dohrn's opinion that the change to the adult Petromyzon is a sort of atavism.

Myxine, though in other respects more degenerate than the adult Petromyzon, retains the structure of the retina in a somewhat more specialized condition, one which most nearly recalls the highest parietal eye presented to us by the Lacertilia.

With regard to the development of the eye in lizards, the only point I will now mention is one which was to be expected to hold, viz. that the lens develops as a thickening of the anterior wall of the vesicle. I may add, however, that it shows signs of a tendency to involution.

And now, without discussing Spencer's speculations, I will briefly state my idea of the manner in which the parietal eye was evolved in connexion with the paired eyes.

From the start of my investigations I was fully convinced that the evolution of all three eyes must be viewed from one common starting-point. The fact that, as Wiedersheim states, even in man nerve-fibres have been traced from the optic thalami to the pineal gland, is sufficient evidence for this, even if we did not know that all three eyes arise in connexion with the same portion of the brain. The hypothesis is an extension of that given by Wiedersheim, Carrière, Dohrn, and others, to account for the evolution of the paired eyes.

The starting-point is a dorsal optic plate before the neural folds begin to form. This gives us a dorsal eye on the so-called invertebrate type. When the neural folds began to form so as to involute the brain and spinal cord, the optic plate was of course, being part of the brain, involved in the involution. With the progression of the latter it probably increased in size, and extended somewhat over the lateral margins of the neural folds.

When the neural folds closed and shut in that which forms the optic vesicles, part of the optic plate was left, forming the rudiment of the parietal eye. This, just as all known sense-organs tend to get involuted, got also secondarily involuted, and that but slowly, so that the outside wall of the involution had time to become a lens, an eye being thus formed on the invertebrate type. The parietal eye, being closely bound up with the paired eyes, got secondarily involuted with them; and, losing its primary mode of origin by delay in its development, it now appears as a secondary outgrowth of the brain, in which the lens is still formed from the outer wall. The lens, moreover, possibly retains traces of an involution.

Spencer has not attempted to grapple with the difficulty involved in the fact that the rods of the retina of the paired eyes are turned from the source of light, while in the parietal eye they are turned towards it.

The explanation given above is not in contradiction with this state of things; it, in fact, receives support from it.

In the complete paper I shall discuss the matter at length, and give ample illustrative figures.

J. BEARD.

Anatomisches Institut, Freiburg i/Br., June 21.

THE JUBILEE ANTICYCLONE.

"QUEEN'S WEATHER" has long been a familiar expression descriptive of the most desired weather for all open-air celebrations and enjoyments; and perhaps no June of the last fifty years has presented us with so many days of such choice weather as the June of 1887. In the language of modern meteorology this is due to the fact that the prevailing type of weather has been anticyclonic. From the middle of June to the beginning of July, thus including the time of Her Majesty's Jubilee, a very pronounced and remarkable anticyclone overspread the British Islands, with its usual attendants of bright weather, strong sunshine and heat during the day, clear and cool nights, and capriciously-distributed rainfall.

Taking June as a whole, temperature was most in excess of the average in the west and north-west of Ireland and over Central Scotland from Inverness to the Solway; the excess at Glencarron, in Ross-shire, being 5°·0, at Laing and Braemar 4°·5, and in many places in Scotland and the west of Ireland about 4°·0. The exceptional character of these temperatures will appear from the fact that during the present century they have only been exceeded in the north-east of Scotland in the Junes of 1818, 1826, and 1846. On the other hand, over England, to the east of a line drawn from Berwick to the Isle of Wight, and to the north of a line from Stornoway to Wick, temperature does not appear to have exceeded the mean of June more than a degree: whilst at Somerleyton in Suffolk, and North Unst in Shetland, the temperature fell fully a degree below the average. These differences were due to the general position of the centre of the anticyclone being well to westward of the British Islands, so that the northern islands and the south-east of England were within the eastern margin of the anticyclone, and hence exposed to the northerly winds and lower temperatures peculiar to that section of an anticyclone, as was pointed out in NATURE ten years ago in reviewing the American Weather Maps. During this anticyclonic weather there were two distinct sources of high temperature, viz. that due to the strong sunshine which found its most decided expression in the high temperatures of Central Scotland; and that due to the warm descending air-currents of the anticyclone, which being most marked at great heights was most strongly expressed at the Ben Nevis Observatory. At this Observatory the means, for the ten days ended June 26, of the daily maxima were 61°·8, and of the minima 50°·3, thus giving a mean temperature of 56°·0, and 11°·5 for the daily range. Quite different was the temperature during these ten days at low levels inland. At Pinmore, for example, in the deep valley of the Stinchar, Ayrshire, the mean temperature was 63°·4, and the daily range 33°·3, or three times greater than on the top of Ben Nevis. On June 21 the contrast was very striking, the minimum on Ben Nevis being 43°·0, whereas at Pinmore it fell to 34°·3, on which morning, as reported by Mr. Donald, the observer, it was freezing at the river side. During the night the high temperature was kept up on Ben Nevis by the descending air-currents of the anticyclone, but the cold currents generated by the night radiation concentrated on and filled the steep narrow valley of the Stinchar.

The frequent occurrence of 40°·0 and upwards between the daily maximum and minimum, so frequently observed over the country, was primarily dependent on the clear dry atmosphere and the strong solar and terrestrial radiation consequent thereon. These great and sudden changes of temperature were on occasions largely increased by the shiftings of the position of the anticyclone, by which a particular locality was at one time on its west side, and therefore in enjoyment of the high temperature peculiar to that position, but a few hours thereafter was within its eastern side and its low temperature.

So far as records have reached us, the rainfall was nowhere above its average, being, however, at or close to the average at Glenquoich and Glencarron, where it was respectively 5·53 and 4·19 inches. On Ben Nevis 7·51 inches fell, being only 0·66 inch less than the average. At Oxford, the deficiency from the monthly mean was only 15 per cent., and at Somerleyton 23 per cent. Generally, however, the deficiency was exceptionally great and widespread, being in nearly all parts of the British Islands from 50 to 95 per cent. less than the June average of the stations. Another feature of the weather was the sudden changes which occurred in the humidity of the air, which were perhaps most striking on June 18, on which day at many places a higher temperature was observed than has been noted for many years. On that day thunderstorms occurred over the greater part of the eastern districts of Scotland, accompanied with dense clouds and a close atmosphere. At a very large number of places not a drop of rain fell. At a few places a heavy, short-continued shower fell, but the air cleared and dried so suddenly that in three minutes all effects of the rain were gone; and everything looked as parched and dried up as before the rain. On the morning of this day the isobars for 9 a.m. revealed the existence of a local shallow depression extending from Ochtertyre, north-eastwards towards Aberdeen, where atmospheric pressure was lower than on either side of it. Here the thunderstorm was severest, and rain fell most generally. At Lednathie, Forfarshire, the storm and rainfall were all but unprecedented. The rain commenced at 12.50 p.m., and ceased at 1.30 p.m., and during these forty minutes there fell 2·24 inches. Mr. Morison, the observer, remarks that "the appearance of the rain while falling was like bright small streams falling straight down"—a description which will recall to some of our readers what they have often noticed during the torrential downpours of the tropics.

The state of many of our rivers attests only too strongly to the persistence and severity of the drought. On Sunday last the level of the Tay was fully half an inch beneath the deep cut made in the red sandstone rock at Perth on June 30, 1826, to mark the unprecedented lowness of the river at that time. The Thames in its upper reaches is covered with high grown rushes and great floating masses of weeds, and nearer London it is reported to be lower than it has been in the memory of the oldest boatman.

NO LANGUAGE WITHOUT REASON—NO REASON WITHOUT LANGUAGE.

As I found that you had already admitted no less than thirteen letters on my recent work "The Science of Thought," I hesitated for some time whether I ought to ask you to admit another communication on a subject which can be of interest to a very limited number of the readers of NATURE only. I have, indeed, from the very beginning of my philological labours, claimed for the science of language a place among the physical sciences, and, in one sense, I do the same for the science of thought. Nature that does not include human nature in all its various manifestations would seem to me like St. Peter's without its cupola. But this plea of mine has not as yet been generally admitted. The visible material frame of man, his sense-organs and their functions, his nerves and his brain, all this has been recognized as the rightful domain of physical science. But beyond this physical science was not to go. There was the old line of separation, a line drawn by mediæval students between man, on one side, and his works, on the other; between the sense-organs and their perceptions; between the brain and its outcome, or, as it has sometimes been called, its secretion—namely, thought. To attempt to obliterate that line between physical science, on one side, and moral science, as it used to be called, on the other, was represented as mere confusion of thought. Still, here as elsewhere, a perception of higher unity does not necessarily imply an ignoring of useful distinctions. To me it has always seemed that man's nature can never be fully understood except as one and indivisible. His highest and most abstract thoughts appear to me inseparable from the lowest material impacts made upon his bodily frame. And "if nothing was ever in the intellect except what was first in the senses," barring, of course, the intellect itself, it follows that we shall never understand the working of the intellect, unless we first try to understand the senses, their organs, their functions, and, in the end, their products. For practical purposes, no doubt, we may, nay we ought, to separate the two. Thus, in my own special subject, it is well to separate the treatment of phonetics and acoustics from higher linguistic researches. We may call phonetics and acoustics the ground floor, linguistics the first story. But as every building is one—the ground floor purposeless without the first story, the first story a mere castle in the air without the ground floor—the science of man also is one, and would, according to my opinion, be imperfect unless it included psychology, in the widest meaning of that term, as well as physiology; unless it claimed the science of language and of thought, no less than the science of the voice, the ear, the nerves, and the brain, as its obedient vassals. It was, therefore, a real satisfaction to me that it should have been NATURE where the questions raised in my "Science of Thought" excited the first interest, provoking strong opposition, and eliciting distinct approval, and I venture to crave your permission, on that ground, if on no other, for replying once more to the various arguments which some of your most eminent contributors have brought forward against the fundamental tenet of my work, the inseparableness of language and reason.

I may divide the letters published hitherto in NATURE into three classes, unanswerable, answered, and to be answered.

I class as unanswerable such letters as that of the Duke of Argyll. His Grace simply expresses his opinion, without assigning any reasons. I do not deny that to myself personally, and to many of your readers, it is of great importance to know what position a man of the Duke's wide experience and independence of thought takes with regard to the fundamental principle of all philosophy, the identity of language and thought, or even on a merely subsidiary question, such as the geneaological descent of man from any known or unknown kind of animal. But I must wait till the Duke controverts either the linguistic facts, or the philosophical lessons which I have read in them, before I can meet fact by fact, and argument by argument. I only note, as a very significant admission, one sentence of his letter, in which the Duke says: "Language seems to me to be necessary to *the progress of thought*, but not at all necessary to the mere act of thinking." This sentence may possibly concede all that I have been contending for, as we shall see by and by.

I class as letters that have been answered the very instructive communications from Mr. F. Galton, to which I replied in NATURE of June 2 (p. 101), as well as several notes contributed by correspondents who evidently had read my book either very rapidly, or not at all.

Thus, Mr. Hyde Clarke tells us that the mutes at Constantinople, and the deaf-mutes in general, communicate by signs, and not by words—the very fact on which I had laid great stress in several parts of my book. In the sign-language of the American Indians, in the hieroglyphic inscriptions of Egypt, and in Chinese and other languages which were originally written ideographically, we have irrefragable evidence that other signs, besides vocal signs or vocables, can be used for embodying thought. This, as I tried to show, confirms, and does not invalidate, my theory that we cannot think without words, if only it is remembered that words are the most usual and the most perfect, but by no means the only possible signs.

Another correspondent, "S. T. M. Q.", asks how I account for the early processes of thought in a deaf-mute. If he had looked at p. 63 of my book he would have found my answer. Following Prof. Huxley, I hold that deaf-mutes would be capable of few higher intellectual manifestations than an orang or chimpanzee, if they were confined to the society of dumb associates.

But, though holding this opinion, I do not venture to say that deaf-mutes, if left to themselves, may not act rationally, as little as I should take upon myself to assert that animals may not act rationally. I prefer indeed, as I have often said, to remain a perfect agnostic with regard to the inner life of animals, and, for that, of deaf-mutes also. But I should not contradict anybody who imagines that he has discovered traces of the highest intellectual and moral activity in deaf-mutes or animals. I read with the deepest interest the letter which Mr. Arthur Nicols addressed to you. I accept all he says about the sagacity of animals, and if I differ from him at all, I do so because I have even greater faith in animals than he has. I do not think, for instance, that animals, as he says, are much longer in arriving at a conclusion than we are. Their conclusions, so far as I have been able to watch them, seem to me far more rapid than our own, and almost instantaneous. Nor should I quarrel with Mr. Nicols if he likes to call the vocal expressions of pain, pleasure, anger, or warning, uttered by animals, language. It is a perfectly legitimate metaphor to call every kind of communication language. We may speak of the language of the eyes, and even of the eloquence of silence. But Mr. Nicols would probably be equally ready to admit that there is a difference between shouting "Oh!" and saying "I am surprised." An animal may say "Oh!" but it cannot say "I am surprised;" and it seems to me necessary, for the purpose of accurate reasoning, to be able to distinguish in our terminology between these two kinds of communication. On this point, too, I have so fully dwelt in my book that I ought not to encumber your pages by mere extracts.

I now come to the letters of Mr. Ebbels and Mr. Mellard Reade. They both seem to imagine that, because I deny the possibility of conceptual thought without language, I deny the possibility of every kind of thought without words. This objection, too, they will find so fully answered in my book, that I need not add anything here. I warned my readers again and again against the promiscuous use of the word "thought." I pointed out (p. 29) how, according to Descartes, any kind of inward activity, whether sensation, pain, pleasure, dreaming, or willing, may be called thought; but I stated on the very first page that, like Hobbes, I use thinking in the restricted sense of adding and subtracting. We do many things, perhaps our best things, without addition or subtraction. We have, as I pointed out on p. 20, sensations and percepts, as well as concepts and names. For ordinary purposes we should be perfectly correct in saying that we can "think in pictures." This, however, is more accurately called imagination, because we are then dealing with images, presentations (*Vorstellungen*), or, as I prefer to call them, percepts, and not yet with concepts and names. Whether in man, and particularly in the present stage of his intellectual life, imagination is possible without a slight admixture of conceptual thought and language, is a moot point; that it is possible in animals, more particularly in Sally, the black chimpanzee at the Zoological Gardens, I should be reluctant either to deny or to affirm. All I stand up for is that, if we use such words as thought, we ought to define them. Definition is the only panacea for all our philosophical misery, and I am utterly unable to enter into Mr. Ebbels's state of mind when he says: "This is a mere question of definition, not of actual fact."

When Mr. Ebbels adds that we cannot conceive the sudden appearance of the faculty of abstraction together with its ready-made signs or words, except by a miracle, he betrays at once that he has not read my last book, the very object of which is to show that we require no miracle at all, but that all which seemed miraculous in language is perfectly natural and intelligible. And if he adds that he has not been able to discover in my earlier works any account of the first beginnings of language, he has evidently overlooked the fact that in my lectures on the science of language I distinctly declined to commit myself to any theory on the origin of language, while the whole of my last book is devoted to the solution of that problem. My solution may be right or wrong, but it certainly does not appeal to any miraculous interference for the explanation of language and thought.

There now remain two letters only that have really to be answered, because they touch on some very important points, points which it is manifest I ought to have placed in a clearer light in my book. One is by Mr. Murphy, the other by Mr. Romanes. Both have evidently read my book, and read it carefully; and if they have not quite clearly seen the drift of my argument, I am afraid the fault is mine, and not theirs. I am quite aware that my "Science of Thought" is not an easy book to read and to understand. I warned my readers in the preface that they must not expect a popular book, nor a work systematically built up and complete in all its parts. My book was written, as I said, for myself and for a few friends, who knew beforehand the points which I wished to establish, and who would not expect me, for the mere sake of completeness, to repeat what was familiar to them, and could easily be found elsewhere. I felt certain that I should be understood by them, if I only indicated what I meant; nor did it ever enter into my mind to attempt to teach them, or to convince them against their will. I wrote as if in harmony with my readers, and moving on with them on a road which we had long recognized as the only safe one, and which I hoped that others also would follow, if they could once be made to see whence it started and whither it tended.

Mr. Murphy is one of those who agree with me that language is necessary to thought, and that, though it may be possible to think without words when the subjects of thought are visible things and their combinations, as in inventing machinery, the intellectual power that invents machinery has been matured by the use of language. Here Mr. Murphy comes very near to the remark made by the Duke of Argyll, that language seems necessary to the *progress of thought*, but not at all necessary to the mere act of thinking, whatever that may mean. But Mr. Murphy, while accepting my two positions—that thought is impossible without words, and that all words were in their origin abstract—blames me for not having explained more fully on what the power of abstraction really depends. So much has lately been written on abstraction, that I did not think it necessary to do more than indicate to which side I inclined. I quoted the opinions of Aristotle, Bacon, Locke, Berkeley, and Mill, and as for myself I stated in one short sentence that I should ascribe the power of abstraction, not so much to an effort of our will, or to our intellectual strength, but rather to our intellectual weakness. In forming abstractions our weakness seems to me our strength. Even in our first sensations it is impossible for for us to take in the whole of every impression, and in our first perceptions we cannot but drop a great deal of what is contained in our sensations. In this sense we learn to abstract, whether we like it or not; and though afterwards abstraction may proceed from an effort of the will, I still hold, as I said on p. 4, that though *attention* can be said to be at the root of all our knowledge, the power of abstraction may in the beginning not be very far removed from the weakness of distraction. If I had wished to write a practical text-book of the science of thought, I ought no doubt to have given more prominence to this view of the origin of abstraction, but as often in my book, so here too, I thought *sapienti sat*.

I now come to Mr. Romanes, to whom I feel truly grateful for the intrepid spirit with which he has waded through my book. One has no right in these days to expect many such readers, but one feels all the more grateful if one does find them. Mr. Romanes was at home in the whole subject, and with him what I endeavoured to prove by linguistic evidence—namely, that concepts are altogether impossible without names—formed part of the very A B C of his psychological creed. He is indeed almost too sanguine when he says that concerning this truth no difference of opinion is likely to arise. The columns of NATURE and the opinions quoted in my book tell a different tale. But for all that, I am as strongly convinced as he can be that no one who has once understood the true nature of words and concepts can possibly hold a different opinion from that which he holds as well as I.

It seems, therefore, all the more strange to me that Mr. Romanes should have suspected me of holding the opinion that we cannot think without pronouncing or silently rehearsing our thought-words. It is difficult to guard against misapprehensions which one can hardly realize. Without appealing, as he does, to sudden aphasia, how could I hold pronunciation necessary for thought when I am perfectly silent while I am writing and while I am reading? How could I believe in the necessity of a silent rehearsing of words when one such word as "therefore" may imply hundreds of words or pages, the rehearsing of which would require hours and days? Surely, as our memory enables us to see without eyes and to hear without ears, the same persistence of force allows us to speak without uttering words. Only, as we cannot remember or imagine without having first seen or heard something to remember, neither can we inwardly speak without having first named something that we can remember. There is an algebra of language far more wonderful than the algebra of mathematics. Mr. Romanes calls that algebra "ideation," a dangerous word, unless we first define its meaning and lay bare its substance. I call the same process addition and subtraction of half-vanished words, or, to use Hegel's terminology, *aufgehobene Worte;* and I still hold, as I said in my book, that it would be difficult to invent a better expression for thinking than that of the lowest barbarians, "speaking in the stomach." Thinking is nothing but speaking *minus* words. We do not begin with thinking or *ideation*, and then proceed to speaking, but we begin with naming, and then by a constant process of addition and subtraction, of widening and abbreviating, we arrive at what I call thought. Everybody admits that we cannot count—that is to say, add and subtract—unless we have first framed our numerals. Why should people hesitate to admit that we cannot possibly think, unless we have first framed our words? Did the Duke of Argyll mean this when he said that language seemed to him necessary for *the progress of thought*, but not at all for the mere act of thinking? How words are framed, the science of language has taught us; how they are reduced to mere shadows, to signs of signs, apparently to mere nothings, the science of thought will have to explain far more fully than I have been able to do. Mr. Romanes remarks that it is a pity that I should attempt to defend such a position as that chess cannot be played unless the player "deals all the time with thought-words and word-thoughts." I pity myself indeed that my language should be liable to such misapprehension. I thought that to move a "castle" according to the character and the rules originally assigned to it was to deal with a word-thought or thought-word. What is "castle" in chess, if not a word-thought or thought-word? I did not use the verb "to deal" in the sense of pronouncing, or rehearsing, or defining, but of handling or moving according to understood rules. That this dealing might become a mere habit I pointed out myself, and tried to illustrate by the even more wonderful playing of music. But, however automatic and almost unconscious such habits may become, we have only to make a wrong move with the "castle" and at once our antagonist will appeal to the original meaning of that thought-word, and remind as that we can move it in one direction only, but not in another. In the same manner, when Mr. Romanes takes me to task because I said that "no one truly thinks who does not speak, and that no one truly speaks who does not think," he had only to lay the accent on *truly*, and he would have understood what I meant—namely, that in the true sense of these words, as defined by myself, no one thinks who does not directly or indirectly speak, and that no one can be said to speak who does not at the same time think. We cannot be too charitable in the interpretation of language, and I often feel that I must claim that charity more than most writers in English. Still, I am always glad if such opponents as Mr. Romanes or Mr. F. Galton give me an opportunity of explaining more fully what I mean. We shall thus, I believe, arrive at the conviction that men who honestly care for truth, and for the progress of truth, must in the end arrive at the same conclusions, though they may express them each in his own dialect. That is the true meaning of the old dialectic process, to reason out things by words more and more adequate to their purpose. In that sense it is true also that no truth is entirely new, and that all we can aim at in philosophy is to find new and better expressions for old truths. The poet, as Mr. A. Grenfell has pointed out in his letter to NATURE (June 23, p. 173), often perceives and imagines what others have not yet conceived or named. In that sense I gladly call myself the interpreter of Wordsworth's prophecy, that "the word is not the dress of thought, but its very incarnation."

F. MAX MÜLLER.

The Molt, Salcombe, July 4.

ON THE PRESENCE OF BACTERIA IN THE LYMPH, ETC., OF LIVING FISH AND OTHER VERTEBRATES.[1]

I FIRST noticed bacteria in the blood of a roach (*Leuciscus rutilus*). This roach, for some hours before it was removed from the water, had been occasionally swimming on its side at the surface—an indication that it was in an exhausted condition. Immediately after the fish was killed, a drop of blood was taken from the heart by a sterilized pipette (with all the necessary precautions) and examined. The blood was found to contain a considerable number of slender motionless bacilli, measuring from 0·003-0·008 micromillimetres in length. On an average, four bacilli were visible in the field at a time, with Zeiss's F objective and No. 1 eye-piece. The peritoneal fluid which was next examined contained so many bacilli that it was impossible to count them; the bacilli were usually lying amongst large granular lymph-cells, and they were longer and more slender than those in the blood. Similar bacilli were found in the lymphatics, spleen, liver, and kidney, and they were abundant in the muscles in contact with the peritoneum, while very few were found in the muscles under the skin of the trunk, and still fewer in the muscles near the tail. The intestine was crowded with similar bacilli to those found in the body-cavity, and, in addition, there were a number of large and small bacteria and micrococci. Bacilli also were found in the walls of the intestine and in the bile-duct. Believing that there was some relation between the diminished vitality of the above roach and the numerous bacilli in the tissues, I examined a considerable number of healthy roach in the same way, and also other fresh-water fish, *e.g.* trout (*Salmo levenensis*), perch (*Perca fluviatilis*), carp (*Cyprinus auratus*), and eels (*Anguilla vulgaris*). In all the healthy specimens examined, with the exception of the trout, bacilli were found in the

[1] Abstract of Paper by Prof. J. C. Ewart, read before the Edinburgh Royal Society on June 6.

body-cavity. Bacilli were also present in the blood of the carp, and on one occasion four bacilli were detected in a drop of blood from what appeared to be a healthy roach. In some the peritoneal fluid contained numerous bacilli, while in others only a few were visible; generally there was a relation between the number in the body-cavity and the number in the intestine, and they were most abundant in fish which had lived for some time in aquaria without food; but in trout which had been fasting for at least ten days, no bacilli could be observed in the peritoneal fluid. The carp which had bacilli in their blood had been living for some months in a small glass aquarium.

The difference between the roach first examined and those examined subsequently led me to endeavour to ascertain whether a sudden change of temperature would produce any influence in the number and distribution of the bacilli. As I anticipated, a rapid change from a spring to a summer temperature (from 48° to 65° F.) greatly diminished the vitality of all the fish experimented with, except the carp; and, as the fish became more and more exhausted, the bacilli gradually increased. If the temperature was raised from 48° F. to 65° F. in two hours, the bacilli of the peritoneal fluid not only increased in the roach, perch, carp, and eel, but they made their appearance in considerable numbers in the body-cavity of the trout, and on one occasion a number of small bacilli were found in the blood of a trout. Although the carp seemed to enjoy the rise of temperature, they were not exempt from the increase of the bacteria in the blood as well as in the peritoneal fluid. In some specimens of blood as many as eight short slender bacilli were visible in the field of the microscope at one time, and the peritoneal fluid in some instances swarmed with long and short bacilli, some of which were motile.

The above observations were confirmed by cultivations in gelatine agar-agar, and in infusions of fish-muscles. In healthy active specimens of the roach and perch, cultivations were easily obtained of the peritoneal bacilli, and generally also from the muscular fibres lying near the peritoneum, but in no instance did I succeed in obtaining cultivations when the blood, or the muscles from immediately under the skin, were used for infecting the culture-media.

Of the sea fish examined I have found bacilli—sometimes long and slender, sometimes short and thick—in the peritoneal fluid and blood of the whiting (*Gadus merlangus*), haddock (*Gadus æglefinus*), cod (*Gadus morrhua*), herring (*Clupea harengus*); and in the peritoneal fluid only of the flounder (*Platessa flessus*), plaice (*Platessa vulgaris*), and lumpsucker (*Cyclopterus lumpus*). I have not hitherto succeeded in demonstrating the existence of bacteria in either the peritoneal fluid or blood of the skate (*Raia batis*), dogfish (*Acanthias vulgaris*), or fishing frog (*Lophius piscatorius*).

There can be no doubt that the bacteria enter the body-cavity by penetrating the walls of the intestine; neither can there be any doubt that, having once established themselves in the peritoneal fluid, they do their utmost to find their way into the blood and tissues. Notwithstanding the presence of active bacteria in the intestinal canal, and the bile and pancreatic ducts, I have failed to discover either bacilli or micrococci in the body-cavity of either amphibia, reptiles, birds, or mammals, when in a healthy condition. Hence it may be taken for granted that, in the higher vertebrates, under ordinary circumstances, either (1) the walls of the intestine form an effective filter or screen, which prevents the passage of the bacteria into the body-cavity; or (2) that the living cells of the mucous and other layers so act on the bacteria that they are destroyed before they reach the body-cavity; or (3) that the cells of the peritoneal fluid effectively sterilize the bacteria which succeed in entering; or (4) that the bacteria are destroyed as they pass along the lymphatics towards the general circulation. Most fish seem capable of tolerating the presence of one or more kinds of bacteria in the peritoneal fluid, whilst others can even tolerate considerable numbers in their blood. It seems, however, that there is a limit to this toleration; for when the equilibrium is disturbed, when by a change of the surroundings the vitality of the tissues is diminished, the bacteria rapidly increase, and unless the tissues as rapidly recover, the bacteria may directly or indirectly cause death.

From the observations made, it appears that bacteria travel most easily along the lymphatic canals and spaces, the lymph-cells being apparently less able to arrest their progress than the blood-corpuscles.

As to the nature of the bacilli found in fish nothing has hitherto been determined. Olivier and Richet seem to think they are neither specific nor putrefactive. At first I thought they were putrefactive, but not specific. Having made some further experiments, I am now inclined to consider them specific and not putrefactive. It has been asserted by previous writers that bacteria are always present in the living tissues of fish, but this conclusion should be accepted with some reserve. For example, trout, roach, and eels, which were gutted immediately after death, and introduced for a short time into a 5 per cent. solution of phenol, and then transferred into sterilized water, remained unchanged for weeks. When examined, dead bacteria were found on the surface of the skin and in the peritoneal lining of the body-cavity, but no living bacteria could be detected in the muscles, nor did they appear in cultivations into which fragments of muscle had been introduced. As was anticipated, when the fish were placed in ordinary water, putrefaction at once set in. Hence, in the meantime, it may be taken for granted that while bacteria exist in the tissues of some fish even at comparatively low temperatures, they are not always, if ever, present in the tissues of others.

THE PROGRESS OF SCOTCH UNIVERSITIES.

THE following three diagrams are meant to convey an idea of the progress of the Scotch Universities—Edinburgh, Glasgow, Aberdeen, and St. Andrews—in recent

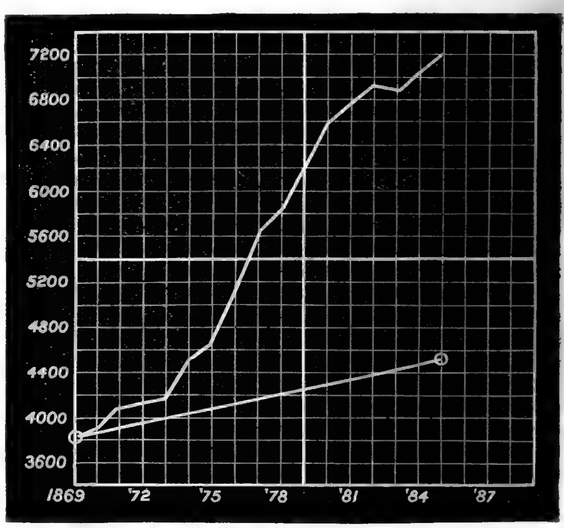


Fig. 1.—Total number of students at the four Scotch Universities (with line of population).

years. The first shows the total number of students each year from 1869 to 1885, and it appears that, with an increase of population of about 18 per cent. in that period, the total attendance has grown over 90 per cent. (The straight line indicates what the growth would have been at the population-rate.) The growth in Edinburgh is greatest, and the other Universities follow in the above order. Nos. 2 and 3 indicate how the students have been distributed among the different Faculties. The preponderance of arts students in Glasgow, and of medical in Edinburgh, will be noted.[1] As regards theology,

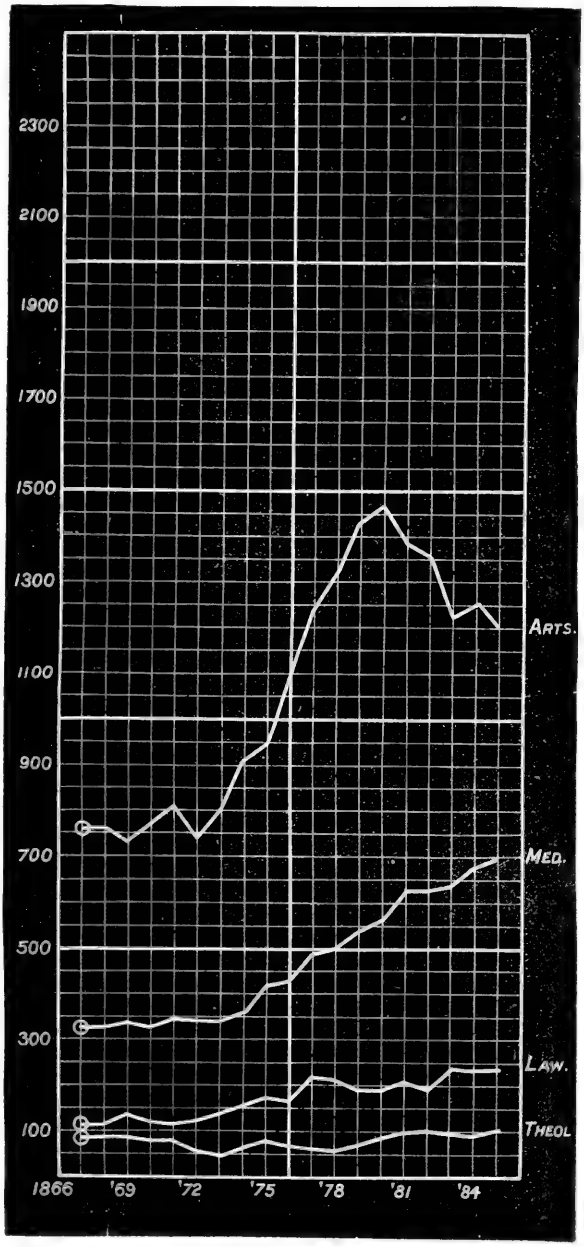


Fig. 2.—Glasgow University. Students in different Faculties (19 years).

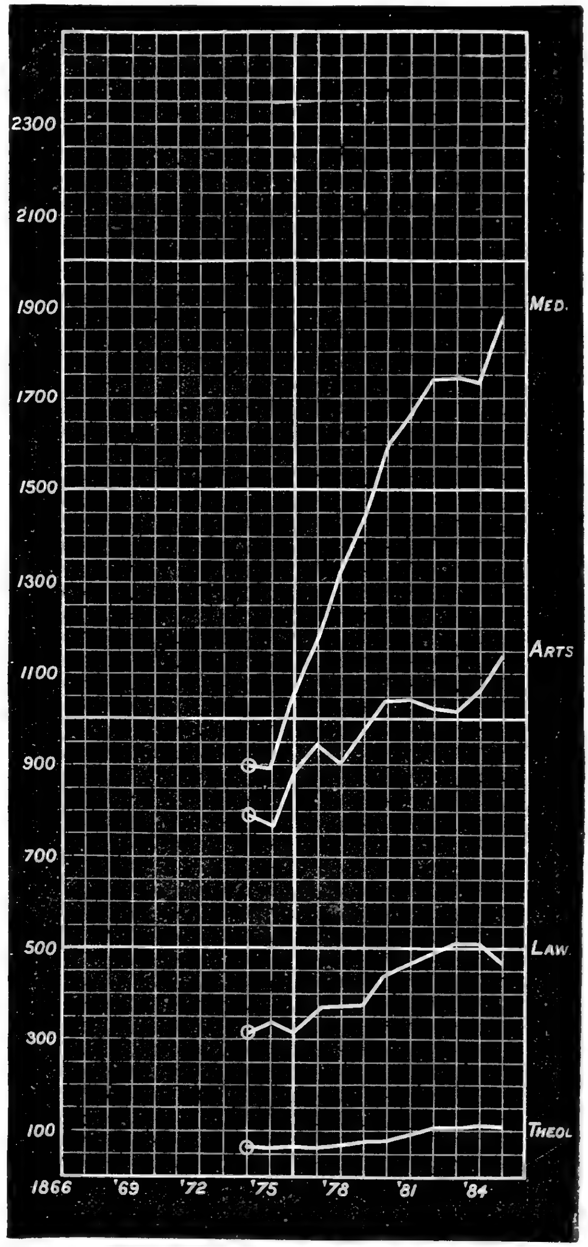


Fig. 3.—Edinburgh University. Students in different Faculties (12 years).

it is to be remembered that the students are only those of the Established Church; the two other large Presbyterian bodies having their own theological schools. (The statistics are taken from Oliver and Boyd's "New Edinburgh Almanac," and the numbers of students at each University include those of the summer as well as the winter session.)

A. B. M.

[1] It is right to state that in the recent classification of Glasgow students a small proportion are given as "Arts and Medicine," "Arts and Law," &c. These we have included as "Arts" students only.

NOTES.

THE Admiralty has, we believe, specially set apart the *Triton* for the use of the scientific branch of the Navy and of men of science at the approaching naval review. It was obviously right that this arrangement should be made, and the Admiralty is to be congratulated on having declined to follow the bad example set by the Lord Chamberlain in connexion with the ceremony in Westminster Abbey.

M. PASTEUR having consented to become a candidate for the office of Perpetual Secretary of the Paris Academy of Sciences, the other candidates have withdrawn their applications, and he will of course be elected unanimously.

THE Committee for the erection of a statue to François Arago, in Paris, on the Place St. Jacques, near the Observatory, held a meeting the other day at the Observatory, Admiral Mouchez in the chair. It was decided that the subscription should be closed on December 31 next. Although the majority of the lists have not yet been returned, it is already known that not less than £700 has been collected. An appeal will be addressed by M. Mouchez to admirers of the celebrated astronomer.

THE Congress of the International Astronomical Society will be held at Kiel from August 29 to September 1.

AT the half-yearly general meeting of the Scottish Meteorological Society, held on Monday, it was intimated that the subscriptions obtained for the Ben Nevis Observatory since the beginning of January last now amount to £1115.

MR. H. H. JOHNSTON, H.B.M. Consul for the Cameroons district of West Africa, has sent home to this country, through the Foreign Office, the collections of natural history objects made during his recent excursion into the Rio del Rey district, a swampy region lying near the base of the Cameroons Mountains, in which it was at one time reported that Mr. Johnston had been taken prisoner and held in captivity by the natives. The collections have been placed by Mr. Sclater in the hands of various specialists to be reported upon. They are not very numerous, and will not probably contain many novelties, as much of the surrounding district has been well explored. But Capt. Shelley has already discovered amongst the birds two examples of a fine new species of plover, which will be described in the next number of the *Ibis* as *Sarciophorus seebohmi*. This plover is remarkable for its rufous forehead, black crown, and chocolate-coloured crop, which render it easily distinguishable from its congeners. There is likewise among the mammals an example of a small shrew new to science, which Mr. Dobson will describe at the next meeting of the Zoological Society, and dedicate to its discoverer.

AN important botanical periodical is about to be issued by the Delegates of the Clarendon Press. It will be entitled *Annals of Botany*, and will be edited by Prof. Bayley Balfour, of the University of Oxford; by Dr. Vines, Reader in Botany in the University of Cambridge; and by Prof. W. G. Farlow, of Harvard University, Massachusetts, U.S.A. The papers, adequately illustrated, will be on subjects pertaining to all branches of botanical science, including morphology, histology, physiology, palæobotany, pathology, geographical distribution, economic botany, and systematic botany and classification. There will also be articles on the history of botany, reviews and criticisms of botanical works, reports of progress in the different departments of the science, short notes, and letters. A record of botanical works in the English language will be a special feature. With regard to the last point, the editors direct attention to the fact that many important contributions to botanical science are not at present brought before the botanical world with that promptitude which their merit deserves, and many are frequently entirely overlooked, owing to the fact that the periodical in which they appear is not readily accessible to botanists generally. An attempt will be made in the *Annals of Botany* to remedy this state of affairs; and it is hoped that it may be possible to make the record fairly complete, embracing works published not only in Great Britain and Ireland, but also in India and the colonies, and in America. To enable them to carry out this intention, the editors appeal to the Secretaries of local Scientific Institutions, Societies, and Clubs, in all parts of the world, to send them early information of the publication of papers relating to botany in any of its branches.

DURING the months of March, April, and May, Prof. Paul Ascherson, of the Berlin University, carried on botanical researches on the coast of Egypt. He has found a surprising number of plants that were formerly unknown. Special interest attaches to the results obtained by him on the coast between the Suez Canal and the Syrian frontier.

THE Berlin Academy of Sciences has granted 900 marks (£45) to Dr. Ravitz (Naples) for the continuation of his researches on the central nervous system of Acephala; 3000 marks (£150) to Prof. Nussbaum (Bonn) for a zoological expedition to San Francisco and investigations on the division of organisms; 600 marks (£30) to Dr. Otto Zacharias (Hirschberg) for the continuation of his studies on the fauna of the North German lakes; and 1200 marks (£60) to Dr. Karl Schmidt for a geological expedition to the Pyrenees.

WE are glad to notice that the Council of the Recreative Evening Schools' Association, through one of their Committees, are organizing a system of elementary instruction in many branches of natural science, to come into operation next season in the London Board schools. These evening classes are held from September or October until April or May, and last winter the Association carried on its operations in eighty of the London Board schools, and hopes to do so in at least 100 next winter. The classes are intended for the continuation of the training of young people between the ages of fourteen and eighteen, who have left the day Board schools. About 80,000 such leave the London schools alone every year, and hitherto not 5 per cent. of them have continued their education. The science teaching is intended to be carried out mainly by lantern demonstrations, and the Committee (among whom we observe the names of Prof. Jeffrey Bell, Mr. F. W. Rudler, Mr. W. Lant Carpenter, Mr. J. Harris Teall, and others) are anxious to obtain the voluntary services of young scientific men, who may be willing to help so good a cause by undertaking to give courses of ten or twelve very elementary lectures, of a thoroughly popular but educational kind, on various scientific subjects. The expenses of lantern, slides, &c., will be borne by the Association. It is intended that the lectures shall be once a week, and shall not exceed forty-five minutes in length. Circulars have been freely sent to the various centres of scientific teaching in London inviting the co-operation of students and others, and further information can be obtained from any member of the "Science Committee" of the Association, or from its Secretary, Mr. J. E. Flower, 37 Norfolk Street, Strand. We are also informed that the Gilchrist Trustees have generously expressed their intention of spending £100 on lanterns and slides for the use of the Association.

A WELL-EQUIPPED technical school for Preston and the neighbourhood is about to be erected and endowed. A grant of £30,000 has been made for the purpose to the Council of the Harris Institute, Preston, by the trustees under the will of the late Mr. E. R. Harris, who left nearly half a million sterling for philanthropic objects in Preston. The site for the school has been given by the Preston Corporation. In the prospectus just issued by the Council of the Institute it is estimated that the cost of the building, furniture, and fittings will not be less than £17,000, of which they are allowed to provide £10,000 out of the grant, the remaining £20,000 being held as an endowment fund. The Council intend that instruction shall be given in all the branches of cotton-spinning, weaving, and designing, mechanical engineering, and the building-trades in general, both in day and night classes. The school will be called the Victoria Jubilee Technical School.

THE Chair of Civil Engineering in the University of Dublin has been filled by the appointment of Prof. Thomas Alexander, who recently returned from Japan after having held the Professorship of Civil Engineering in the Imperial College of Engineering, Tokio, for seven years. Mr. Alexander is the author of several valuable papers on engineering subjects, and has given some new theorems in graphic statics which have been adopted both in English and Continental works on that subject. He is also, jointly with Mr. Arthur W. Thomson, author of a work on elementary applied mechanics.

IN the latest number of the *Zeitschrift für physikalische Chemie* will be found complete details of the classical work of Drs. Krüss and Nilson, briefly announced three weeks ago in the *Berichte*, upon the vapour-density of thorium chloride, which finally sets at rest the controversy as to the valency and position in the natural system of thorium. From a consideration of the physical constants, and the fact that the oxide is isomorphous with the oxides of titanium, zirconium, and tin, thorium was generally supposed to be tetravalent, forming an oxide, ThO_2, and a chloride, $ThCl_4$; moreover, an element of atomic weight 232, having these properties and belonging to the tetravalent series, was required from theoretical considerations based on the assumption of the truth of the periodic law. But, unfortunately, confusion was introduced into all this harmony by the matter-of-fact announcement by no less an authority than Troost that the vapour-density of the chloride had been determined by him to correspond to the formula $ThCl_2$. This, however, meant a divalent thorium of atomic weight 116, for which no place exists in the periodic table. Krüss and Nilson, in their endeavours to get at the truth of this matter, have utilized a quantity of pure thorium, which they had prepared for atomic-weight determinations, by converting it into the chloride, pure colourless prisms of which were eventually obtained by resublimation in a platinum tube. The determination of its vapour-density was then carried out in a platinum vessel and in an atmosphere of carbon dioxide, with the satisfactory result that at temperatures varying from 1102° to 1140°, just above the point of volatilization, the vapour-density corresponds to a formula of $ThCl_4$, while above this temperature the chloride dissociates into free chlorine and a lower chloride. This proves decisively that thorium is tetravalent, and demonstrates the accuracy of results deduced from physical constants. To complete this splendid work, which bears great similarity to the famous work of Nilson and Pettersson on beryllium, the Swedish chemists have redetermined the atomic weight of thorium, which, in the light of their vapour-density determinations, they find to be 231·87.

THE Annual Report of the Chief Signal Officer of the United States Army for the year 1885 has now reached this country. It consists of two volumes: the first contains the usual meteorological results and notices of the works in progress; the second part, a volume of 440 pages (Washington, 1886), is a treatise by Dr. W. Ferrel on the recent advances in meteorology. As might be expected from Dr. Ferrel's works on the "Mechanics and General Motions of the Atmosphere," the subject is not treated in a very elementary manner. In fact, it is stated in the preface that the object has been to select from the material on hand some of the more important principles, methods, and results arrived at, mostly during the last quarter of a century, and to present them in the form of a text-book of the higher meteorology. No descriptions of meteorological instruments are given, as the Report states that this subject will be treated of in a separate work, by Prof. Cleveland Abbe. We refrain from making any comments here on Dr. Ferrel's treatise, further than that it supplies a want that has been much felt by students who have mastered the usual elementary text-books.

VOLUMES 28–30 of the miscellaneous collections published by the Smithsonian Institution (Washington, 1887) contain much valuable matter which should be widely known, viz.:—(1) A fourth edition of Dr. Guyot's meteorological and physical tables, the third edition of which was published more than a quarter of a century ago. Many useful tables have been added, mostly geographical and miscellaneous, but the meteorological tables have generally been reprinted unchanged, and are much behind the present requirements of the science. (2) A catalogue of the principal independent scientific and technical periodicals published in all countries from the earliest times to the close of the year 1882, giving full titles, sequence of series, and other bibliographical details. (3) The scientific writings of the late Joseph Henry, formerly Secretary of the Smithsonian Institution, including his contributions to various Societies and some previously unpublished papers, embracing a period of fifty-five years. This work is divided into two parts, the first containing miscellaneous, and the second meteorological papers.

PROF. SHALER'S article on tornadoes and cyclones in *Scribner's Magazine* for August will contain reproductions of two instantaneous photographs of a tornado which passed over Jamestown, Dakota, on June 6, 1887. The publishers had made a special search for negatives of storms, and given notice of it to many Western photographers. This fortunate opportunity occurred after the article was already in type.

THE "Admiralty Manual of Scientific Inquiry" is such a well-known book, that we need only state the names of the eminent men who have brought the fifth edition, which we have just received, up to date. Astronomy by Sir G. B. Airy, K.C.B., ex-Astronomer-Royal; Hydrography by Capt. W. J. L. Wharton, R.N., Hydrographer of the Admiralty; Tides by Prof. George H. Darwin; Terrestrial Magnetism by Prof. George F. Fitzgerald, assisted by Staff-Commander Creak, R.N., and Mr. G. M. Whipple, Superintendent of the Observatory; Meteorology by Mr. Robert H. Scott, Secretary of the Meteorological Council; Geography by General Sir Henry Lefroy, R.A.; Anthropology by Mr. E. B. Tylor; Statistics by Prof. C. F. Bastable; Medical Statistics by Mr. William Aitken; Geology by Prof. Arch. Geikie; Mineralogy by Prof. W. J. Sollas; Seismology by Mr. Thomas Gray; Zoology by Prof. H. N. Moseley; Botany by Sir J. D. Hooker. About half of the book has been entirely re-written; the arrangement of the present edition being substantially the same as that of former ones. No doubt our men-of-war will by-and-by be used very much more as floating laboratories and observatories than they are at present. When this is done both science and the naval service will be great gainers, and we know of no better means towards such an end than the efficient use of this magnificent compendium published in accordance with the laws of the Admiralty.

"PIONEERING in New Guinea," by the Rev. James Chalmers, contains some very valuable sketches of travels and labours in New Guinea during the years 1878–86. Mr. Chalmers explains that "his hand takes more readily to the tiller than to the pen." Hence he has made no effort to "work up" the contents of his journals into "a finished book," but has been content for the most part to present them exactly as they were written. The book is all the more likely to be appreciated on that account, for it has a freshness and vividness which it could scarcely have possessed if it had sprung less directly from the author's experience. Mr. Chalmers points out that succeeding missionaries and observers can never see the people of New Guinea in the stage of savagery in which he found them when he first went to the island. This gives, of course, a peculiar interest to the record of his impressions. The work contains a map and illustrations, and is published by the Religious Tract Society.

We have received the first eight numbers of "British Dogs" by H. Dalziel (Upcott Gill). The book will supply admirers of the dog with a trustworthy guide, and it provides in an accessible form much information that will be of service to professionals, as well as to amateurs. The descriptions and plates, with slight exceptions, are very good.

Prof. Ayrton's "Practical Electricity" is being translated into the German and Spanish languages.

The tenth volume, lately published, of the series entitled "Monographs of the United States Geological Survey," contains a full account, by Prof. O. C. Marsh, of the Dinocerata, an extinct order of gigantic mammals discovered in the Eocene deposits of Wyoming Territory. The work is admirably illustrated.

The New York Industrial Education Association will begin in the autumn the publication of a series of educational monographs under the editorship of the President of the Association, Dr. Butler. According to *Science*, the papers will treat of various educational topics, historically and critically; and some of the most influential educators, both in America and in Europe, have promised contributions. It is expected that the first monograph will be from the pen of President Gilman, of the Johns Hopkins University. The arguments in favour of industrial education and statements as to its proper organization and development will occupy a prominent place in the series, but not at all to the exclusion of other topics.

On Friday, the 15th inst., a students' *conversazione* will be held at the Technical College, Finsbury. There will be a concert and exhibition, and lectures on "Church Bells" and "Spectrum Analysis" will be delivered, the former by Prof. Ayrton, F.R.S., the latter by Prof. Meldola, F.R.S. A demonstration on "The Use of the Secohmmeter" will be given by Mr. W. E. Sumpner.

In 1880 the Midland Union of Natural History and other Scientific Societies founded the Darwin Medal for the purpose of encouraging original research by members of the Societies forming the Union. The medal is a handsome one, the dies for which were engraved by Mr. Joseph Moore, of Birmingham. On the obverse is the bust of Darwin, and on the reverse a branch of coral, commemorative of one of the most famous of his researches. The subjects for which the medal is awarded are geology, zoology, botany, and archæology. This year it was set apart for archæology, and at the annual meeting of the Midland Union of Natural History Societies, held last week at Malvern, it was awarded to Mr. Edward W. Badger, of King Edward's High School, Birmingham, for a paper on "The Monumental Brasses of Warwickshire."

The second German Fishery Meeting will be held at Freiburg in Baden on July 29 and 30. An excursion to the Imperial Piscicultural Establishment at Hüningen (Alsace) will be made. All inquiries are to be directed to the German Fishery Society, Leipzigerplatz 9, Berlin.

The Deutsche Seewarte has issued a second edition of its ice chart (see Nature, vol. xxxvi. p. 41) compiled from the semi-weekly Atlantic Ice Report, by F. Wyneken, of New York, and from its own observations. The chart shows that the state of the drift ice in April and May was nearly the same as in February and March. Between 48° and 51° W., and north of 42° N., icebergs were frequently met with, but there were very few to the south of this. It is not supposed that the ice will disappear during July, so that vessels cannot yet safely take a more northerly route.

Towards the end of June very remarkable weather prevailed in certain parts of Scandinavia. At Röros, in Central Norway, for instance, it snowed so heavily that sledges might easily have been used. Just before, the weather had been very warm for a long while. In Sweden, on the other hand, several provinces were visited by terrific cyclones, which tore up hundreds of trees by the roots, and unroofed many houses.

At the annual meeting of the Victoria Institute, to be held at the Society of Arts House on Tuesday, July 19, at 8 o'clock, an address will be delivered by the President of the Royal Society.

The total value of the fish landed on the coasts of Scotland during the six months ended June 1887 was £556,058, being a decrease under the corresponding period of last year of £38,332, a decrease under the corresponding month of last year of £34,219, and an increase over last month of £9043.

The additions to the Zoological Society's Gardens during the past week include an Entellus Monkey (*Semnopithecus entellus*, ♀) from India, presented by Capt. W. L. Prentice; a Grey Squirrel (*Sciurus cinereus*) from North America, presented by Mr. Percival Farrer; two Weasels (*Mustela vulgaris* ♂ ♀) from Sussex, presented by Mr. Clement Wykeham Archer; two Blue-headed Pigeons (*Starnœnas cyanocephala*) from Cuba, presented by Mr. John Marshall; two Common Gulls (*Larus canus*) from Scotland, presented by Mr. T. A. Cotton; two Lapwings (*Vanellus vulgaris*) from Essex, presented by Mr. Gervase F. Mathew; an Alligator Terrapin (*Chelydra serpentina*) from North America, presented by Prof. Agassiz; a Speckled Terrapin (*Clemmys guttata*), an American Black Snake (*Coluber constrictor*), from North America, presented by Mr. Samuel Garman; a White-fronted Capuchin (*Cebus albifrons*) from Brazil, a Dingo (*Canis dingo* ♀) from Australia, deposited; two Gluttons (*Gulo luscus*) from Russia, a Redshank (*Totanus calidris*), two Lapwings (*Vanellus vulgaris*) from Suffolk, purchased; a Mandarin Duck (*Æx galericulata*), two Red-crested Pochards (*Fuligula rufina*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

Researches on the Diameter of the Sun.—In continuation of his investigations on the supposed changes in the sun's diameter from year to year (Nature, vol. xxxv. p. 496), Prof. Auwers publishes in the *Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin*, 1887, No. xxviii., the result of his researches on the yearly inequality of the diameter. The existence of such an inequality has been pointed out by Lindenau in his discussion of Maskelyne's observations; by Cesaris, Carlini, and Rosa in the Milan observations; and by Struve in the Dorpat observations. More recently Rosa has discussed extensive series of Greenwich observations of the sun, and also Madras observations; Newcomb and Holden have discussed Greenwich and Washington observations; and Hilfiker has discussed transits of the sun's diameter obtained at Neuchâtel. To these must now be added Prof. Auwers' careful discussion of the Greenwich transit-circle observations, both of horizontal and vertical diameter, obtained during the years 1851–83 inclusive, as well as of the extensive series of Washington and Oxford observations collected in his former paper, referred to above. These discussions all show the existence of apparent inequalities in the sun's diameter during the year, but do not appear to be at all conclusive as to the reality of such variations in the sun itself. In Prof. Auwers' opinion they are due to the effect of temperature on the instrument, or to the effect of difference in the telescopic image of the sun as observed at opposite seasons of the year. Thus a most remarkable inconsistency appears in the results obtained from the Greenwich observations, both of horizontal and vertical diameter, 1851–83, and from the Neuchâtel observations, of horizontal diameter only, for 1862–83. The following table shows the discordances from the mean for each month of the year for the two series:—

Month.	Greenwich.	Neuchâtel.
	"	"
January	−0·36	+0·66
February	−0·24	+0·54
March	−0·03	+0·24
April	+0·22	−0·51
May	+0·25	−0·54
June	+0·08	−0·34
July	+0·08	−0·33
August	+0·01	−0·54
September	−0·06	−0·19
October	−0·10	+0·38
November	−0·22	+0·23
December	−0·35	+0·41

It appears obvious that these results must be attributed to other causes than physical changes in the sun's diameter.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *JULY* 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 July 17

Sun rises, 4h. 4m.; souths, 12h. 5m. 50·5s.; sets, 20h. 7m.; decl. on meridian, 21° 13′ N.: Sidereal Time at Sunset, 15h. 48m.

Moon (New on July 20) rises, 1h. 19m.; souths, 9h. 0m.; sets, 16h. 50m.; decl. on meridian, 17° 15′ N.

Planet.	Rises.	Souths.	Sets.	Decl. on meridian.
	h. m.	h. m.	h. m.	° ′
Mercury	5 59	13 14	20 29	13 45 N.
Venus	8 23	15 6	21 49	7 53 N.
Mars	2 14	10 35	18 56	24 1 N.
Jupiter	12 43	18 0	23 17	9 15 S.
Saturn	4 11	12 12	20 13	21 6 N.

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.	° °
17	85 Tauri	6	1 5	1 45	30 286
17	Aldebaran	1	3 16	3 33	139 172
18	115 Tauri	6	1 50	2 40	73 237

July.	h.	
19	4	Saturn in conjunction with the Sun.
21	17	Mercury in conjunction with and 3° 40′ south of the Moon.

Variable Stars.

Star.	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei	0 52·3	81 16 N.	July 17,	22 32 *m*
			,, 22,	22 11 *m*
R Corvi	12 13·8	18 38 S.	,, 20,	*M*
δ Libræ	14 54·9	8 4 S.	,, 22,	23 16 *m*
U Coronæ	15 13·6	32 4 N.	,, 22,	20 54 *m*
U Ophiuchi	17 10·8	1 20 N.	,, 21,	2 31 *m*
				22 38 *m*
W Sagittarii	17 57·8	29 35 S.	,, 17,	21 0 *m*
			,, 21,	1 0 *M*
U Sagittarii	18 25·2	19 12 S.	,, 22,	1 0 *m*
R Scuti	18 41·5	5 50 S.	,, 23,	*M*
S Sagittæ	19 50·9	16 20 N.	,, 20,	2 0 *m*
			,, 23,	2 0 *M*
S Aquarii	22 51·1	20 57 S.	,, 18,	*M*
S Pegasi	23 14·8	8 18 N.	,, 18,	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
	°	°	
Near ο Cassiopeiæ	11	+48	Very swift. Streaks.
,, 63 Cygni	315	47	Swift. Short.
From Cassiopeia	350	52	Very swift.

GEOGRAPHICAL NOTES.

THE July number of the Proceedings of the Royal Geographical Society contains a detailed report of the paper read by Dr. Junker on his explorations in Central Africa. Mr. Delmar Morgan contributes, from Russian sources, a long account of Russian geographical work in 1886, which contains much that is interesting. One of the most important Expeditions was that under J. V. Ignatieff, to explore the magnificent Khan Tengri group of mountains in the Thian Shan, whose summits soar to a height of 22,000 to 24,000 feet. The botanist of the Expedition, A. N. Krasnof, made some extremely important investigations, with especial reference to the flora of the high snow and ice regions of the Thian Shan, as compared with that of the Polar regions recently worked up by Wittrock. M. Krasnof is of opinion that the valley of the Ili once had an entirely different vegetation to that possessed by it now, and that this early plant-life has completely perished owing to the desiccation of Central Asia and the consequent change in its climate. Formerly, M. Krasnof says, the whole flora of the Ili valley was similar to that still preserved at the foot of the snowy mountains, resembling that of Central Russia. At present all the lower chains are deprived of the moisture they derived from melting ice-fields, and have changed their flora in the most radical way, having now only Central Asian forms. M. Krasnof's general conclusions are that formerly the Thian Shan flora was intermediate between the Altai and the Alpine, and resembled more closely that of the Central and Northern Caucasus. The process of desiccation began in the south, and showed itself by the formation of detritus, retreat of the glaciers, and disappearance of lakes. It caused the formation of loess deposits, sand, and pebble-strewn plains, while it diminished the areas of marshes and black-earth deposits. All plants common to Polar and Alpine regions disappeared from the southern slopes and syrts, while coniferous and deciduous arborescent vegetation also vanished from all waterless slopes. Wherever the snow has ceased to lie, the ancient flora has also perished, only a few species having adapted themselves to a continental climate and assumed an Asiatic character.

THE current number of *Petermann's Mitteilungen* contains several papers of special scientific interest. M. Yokoyama contributes an account of a paper by J. Tanaka, on the vegetation zones of Japan, while Herr Ernst Hartert describes the botanical results of his journey along with Herr Standinger in the Western Soudan. Dr. Supan's paper on the climate of Europe, as regards the duration of a certain mean temperature in different areas, will be found of great value in working out the physical geography of Europe. Dr. Supan's object is to show the length of time (the number of months) a mean temperature—low, temperate, or high—prevails in a European area, and to mark off on maps the areas in which the temperature endures, the number of months being expressed by colours. Many geographical and biological considerations depend on such general facts of climate as Dr. Supan is endeavouring to work out. He divides temperature into three classes: (1) 0° Cent. and under, which he calls the "Frost Period"; (2) 10° to 20° C., the "Warm Period"; (3) 20° C. and over, or the "Hot Period." The duration of these temperatures he has noted at 471 different stations in Europe and the countries round the Mediterranean. The results, which he has represented cartographically by areas of colour, may be briefly summarized thus:—The lines of equal duration of the "Frost Period" run similarly to the winter isothermal lines, changing from a southerly direction in the West of Europe to a south-easterly and then east-south-easterly in the East of Europe. As regards the "Warm Period," it is only on the Atlantic side of Europe that the lines of equal duration run distinctly south-east, elsewhere on the Continent they approximate very nearly to the parallels of latitude; while for the "Hot Period" they show a north-easterly direction. Thus in all three maps the contrast between the oceanic west and the continental east comes out very sharply. A glance at Maps 1 and 2 explains why the Norwegian highland was in the Glacial epoch the birth-place of North European land-ice; the reason is not to be found in the extraordinarily low temperatures, but in the duration of the cold and warm periods. In all districts where a coast range of mountains interposes between the interior and the sea, or where the hills rise abruptly from the sea, the lines of equal duration press closely together, notably in Norway and the Alps. Dr. Supan emphasizes the importance, in considering the climate of Europe of such regions of depression as the valleys of the Rhone and Rhine, the low-lying region of Hungary, and the plain of Poland.

NEWS of the African traveller, Herr Gottlob Ad. Krause, has been received at Berlin through the missionary Steiner from Christiansborg on the Gold Coast. On April 16, Herr Krause arrived at Salaga, north of the Appanti kingdom; proceeding in a northerly direction he succeeded in reaching the vicinity of Timbuctoo. At present, most likely, he has arrived at the Togo coast.

THE PROGRESS OF GEOGRAPHY.

AT the anniversary meeting of the Royal Geographical Society, held on May 23, 1887, General R. Strachey, the Vice-President, delivered an address, from which we take the following extracts:—

The attention of geographers during the year, as far as regards Africa, has been chiefly directed to the basin of the Congo, where many explorers, of various nationalities, have been employed in exploring and surveying the numerous streams which combine to make the Congo one of the greatest fluvial systems of the world. Other explorers have been engaged in the same region in examining into its economical and prospective commercial resources, but at present without definite results. An excellent summary of the geographical work done in the Congo region up to the middle of last year was given to the Society in this hall, in June last, by Sir Francis de Winton, who had then recently returned from his two years' administration of the country. The most important of the new explorations he described was that of Lieut. Wissmann and his party, who had embarked on the upper waters of the Kassai River, near the part made known to us by Livingstone and Cameron, and navigated it to its junction with the Congo. Since then Dr. Wolff, one of Wissmann's companions, has explored the Sankuru, a large northern tributary of the Kassai, and found it navigable for a long distance. One result of this latter exploration is to show that another navigable river of the far interior, the Lomami, enters the Sankuru from the north-east, and that it is a distinct river from the Lomami of Cameron, recently ascended by Grenfell, which enters the Congo near Stanley Falls.

The direction which the Kassai takes—in a long curve, from south-east to west-north-west—causes it to be the recipient of nearly all the drainage of the southern half of the Congo basin, and near its junction with the main stream it adds to its volume the waters of another great tributary, the Quango, besides the Mfini from a chain of great lakes further north. The united waters are poured into the Congo through the Kwa, which, according to Mr. Grenfell's measurement, is contracted in its passage through a range of low hills, and at its mouth is only 700 yards wide (a little higher up only 450 yards); the depth of the swiftly-flowing stream Mr. Grenfell was unable to ascertain, as no bottom was touched with a line 120 feet long.

The prospective value to the Congo State of the Kassai, with its immense mileage of navigable waters flowing through fertile plains, is acknowledged on all hands. Already stations have been founded on its banks, and Portuguese traders are choosing the newly-discovered river route in preference to their old inland road into the interior from Loanda. It has been during the past few months repeatedly reascended by river steamers, once by Sir Francis de Winton himself.

Equal in importance and extent have been the explorations and surveys along the main river and many of its tributaries carried out by Mr. Grenfell. The chief of these explorations were noticed by the Marquis of Lorne in the Address of last year; and a brief general account of his surveys was given, together with a reduction of his admirable series of river charts, in the October number of our Proceedings. Since then Mr. Grenfell has added to his achievements the ascent of the unknown portion of the Quango between its junction with the Kassai (or Kwa) and the Falls of Kikunji, which latter was the farthest point, coming down river, reached by a former traveller, Von Mechow.

Other considerable additions have been made to our knowledge of the Congo region, by Lieuts. Kund and Tappenbeck, members of a scientific Expedition sent out in 1884 by the German African Association. These two courageous travellers, instead of following the courses of the rivers like others, and gleaning information only of the country and people along the banks, struck across the country, first from Stanley Pool to the south, and thence towards the east, crossing in succession all the southern tributaries, from the Quango inclusive to the Lukenye, beyond the Kassai; a toilsome and dangerous march of about 600 miles. Another member of the same Expedition. Dr. Büttner, made also a land journey, of less extent, but not less interest. Starting from San Salvador, the old capital of the Congo, he travelled eastward and crossed the Quango, reaching the capital of a Negro potentate named Kasongo, whence he struck northward to the main Congo above Stanley Pool. Much valuable information regarding the configuration of the country and the ethnology and products of the interior was obtained on these two journeys. We learn, for example, that the whole western section, to a distance some 400 miles inland, is a hilly country cut up by deep valleys, to which succeeds, further inland, a wide stretch of undulating plains, wooded only along the courses of streams, and that it is only when the eastern side of the Kassai is reached that continuous tropical forest is met with.

North of the Congo the French have been active both in completing the pioneer exploration of their new possessions and in laying down with scientific precision large tracts of country imperfectly known. The most important work of the latter kind is that of Capt. Rouvier, the representative of France on the joint Commission for laying down the boundary between the Congo State and the French possessions. This accomplished surveyor fixed numerous positions by a long series of observations both for longitude and latitude, and his Report, which will be accompanied by an atlas of thirty-eight maps on various scales, will form a solid contribution to our geographical knowledge of the region. An important pioneer exploration, about the same time, was made by M. Jacques de Brazza, brother of the eminent traveller, to the north and east of the French stations on the River Ogowé, undertaken soon after Mr. Grenfell's discovery of the magnitude of the Mobangi, and apparently with the object of ascertaining whether that great river flowed within the French boundary as fixed at the Berlin Conference. After a journey of a month's duration through dense forests, M. de Brazza emerged on an open plain, through which flowed, not the Mobangi, but the Sekoli, an independent tributary of the Congo lying far to the westward. After a fruitless attempt had been made to penetrate beyond this river, his party built canoes and descended the Sekoli to its mouth. It has been recently announced that by arbitration the French boundary has been extended a little farther to the east than fixed by the Berlin Conference, so as to include the right bank of the Mobangi. A complete and very useful *résumé* of all the geographical work accomplished by recent French explorers in the Ogowé-Congo region, by Major de Lannoy de Bissy, was contributed to our Proceedings for December last, illustrated by a map reduced from the French surveys.

Public interest has recently been directed towards the region north of the Congo, and the practicable routes it may offer to the Niam-Niam countries and the Egyptian Soudan, in consequence of the despatch of the Expedition under Mr. Stanley, for the relief and rescue of Emin Bey, which has adopted the Congo route to the Upper Nile in preference to the more direct and shorter route inland from Zanzibar. A paper giving a *résumé* of all published information regarding this region was recently read in this hall by our accomplished young colleague, Mr. J. T. Wills. Since then, you have had before you the greatest of all travellers in this little-known region, Dr. Junker, and heard his own account of his six years' explorations. The wide open plain country lying between the Congo and the Nile, which Dr. Junker described to us, is watered by numerous streams, the chief of which, the Welle-Makua, flows westerly in the direction of the Upper Mobangi, and, judging from Dr. Junker's maps, it is difficult to dispute his conclusion, in which Mr. Wills agrees, that the two rivers are the same. Other geographers believe that the Welle-Makua belongs to the Shari system and flows into Lake Chad. The alternative offers one of those problems in which speculative geographers seem to delight; but in this case it will not be long before a solution is arrived at in the only satisfactory way—namely, by actual exploration. Meantime we learn, by the latest news from the Congo, that Mr. Stanley has chosen to adopt a somewhat more direct route to Emin Pasha than that first proposed—namely, from the Congo near Stanley Falls by land to the shores of the Albert Nyanza.

Two more journeys across the continent have been brought to a successful conclusion during the past year. One by M. Gleerup, a Swedish officer, formerly in the service of the Congo State, who crossed from Stanley Falls to Zanzibar, and the other by the experienced traveller and geologist, Dr. Oscar Lenz, who undertook, in 1885, an expedition for the purpose of reaching Dr. Junker and Emin Pasha *viâ* the Congo. Reaching Stanley Falls in February 1886, Dr. Lenz was unable to obtain men from the Arab traders there to accompany him on the march through the unknown country between that point and the Upper Nile, and proceeded to Ujiji in the hope of meeting with better success there, and advancing northwards along the eastern side of Lake Tanganyika. The disturbed state of the country and the excitement in Uganda made this impossible, and he took the Tanganyika and Nyassa route to the Indian Ocean, emerging at the Portuguese settlement of Quillimane.

Further south, Dr. Hans Schinz, a learned botanist and ethnologist, has been exploring with fruitful results the region between the Kunene and Lake Ngami.

On the eastern side of the continent our Society is especially interested in the expedition of Mr. J. T. Last, who was commissioned by us in the summer of 1885 to proceed to the region between the Rovuma and the Zambesi, and follow up the work of Mr. O'Neill by exploring the Namuli Hills and the Lukugu Valley. We hear by recent telegram of his safe arrival at Zanzibar, and may shortly expect him home to give us in person an account of his journey. The letters which we have received from him from time to time have informed us that he has carried out his programme, though he found the summit of the Namuli Hills inaccessible, and, in addition, traversed the whole region a second time, striking into the interior from Quillimane, and emerging at Ibo on the Mozambique coast.

Count Pfeil, one of the most active of the pioneers in the newly-acquired German Protectorate of Eastern Tropical Africa, published last year an account of his two journeys in Khutu and in the neighbouring region, a country previously known to us only through Thomson's expedition to the Central African Lakes. Some additions to our knowledge of the geography of this part of the African interior have resulted from Count Pfeil's labours, the most interesting of which is the discovery of the main stream of the Ulanga, or upper course of the Rufigi, a river which this explorer claims to be of some importance, and which he navigated in a boat for upwards of 150 miles.

The unsuccessful attempt of the experienced African traveller Dr. Fischer to carry succour to Dr. Junker in 1885–86, a mission with which he was charged by that traveller's family, would have excited great interest in the earlier days—not long past—of Central African travel. The route he took led for several hundred miles through a totally unexplored country, namely, from the Pangani westward across the region which still remains a great blank on our maps to the caravan route between Unyanyembe and Victoria Nyanza. He reached the southern shores of the Victoria in January 1886, but found it impossible to obtain leave to pass through the territory of the fanatical king of Uganda. Turning backward he made a valiant attempt to reach the Upper Nile by the eastern side of the great lake, but his supplies failed him by the time he arrived at Lake Bahringo, and he returned with a sorrowful heart to the coast. He did not long survive the fatigues of this arduous journey, but died soon after his return to Europe, in November last.

In the continent of Asia the most important addition to our accurate geographical knowledge of the interior is no doubt that gained by the joint Russian and British Commission, which has been engaged on the survey of the northern frontier of Afghanistan from the borders of Persia to the Upper Oxus, but pending the diplomatic settlement of disputed points this information has not been made public, though it will doubtless soon become available. A brief note of a portion of this work, describing surveys made by Capts. Maitland and Talbot, between the Hari-rud and Bamian, connecting Herat with the last-named place, and also with points north of the Oxus, and the neighbourhood of Kunduz, has appeared in our Proceedings. The total area surveyed amounts to about 120,000 square miles, mapped on the scale of ¼ inch to the mile, in 60 sheets. These brilliant results are believed to be unique in the annals of surveying. The chief of the British topographical staff, by whom these surveys were undertaken, was Colonel Holditch, to whom one of the Gold Medals has now been awarded, in recognition of the valuable services to geography rendered by him in this and other similar expeditions.

Much valuable geographical work has also been accomplished by Mr. Ney Elias, the Gold Medallist in 1873, who was despatched from Ladakh on a mission to Chinese Turkistan, and diverging westward at Yengi-Hissar, traversed the Pamir Plateau for a distance of 360 miles, to the Khanat of Shignan. The details of this journey have not yet been made known by the Indian authorities, but Sir Henry Rawlinson has communicated to our Proceedings a note in which he points out that his former suggestion that this route, first brought to notice by Major Trotter, was probably that by which caravans of Rome passed from Bactria, and had been used as a military road in comparatively modern times, is confirmed by the additional light now thrown on the subject; and he identifies the lake *Rang-Kul*, visited and described by Mr. Elias, as the famous Dragon Lake of Buddhist cosmogony, and as answering very closely to the description given by the Chinese traveller Hwang-tsang in the seventh century.

Mr. A. D. Carey, a gentleman in the Indian Civil Service, has in a most enterprising manner devoted a period of leave of absence to a very remarkable journey in Eastern Turkistan and Tibet, and has traversed a large part of those central regions which have lately been made known by General Prejevalsky, and of which a brief *résumé* was given in the last Presidential Address. Accompanied by Mr. Dalgleish, an enterprising trader, who had previously visited Eastern Turkistan, he started from Ladakh in the summer of 1885, taking a route which had never before been trodden by a European, from Leh eastward across the high Tibetan plateau, and descending to Kiria by an extremely difficult and rugged defile *viâ* Polu. After a short stay here, he traversed the desert northward, along the course of the Khotan River, and arriving at the Tarim, crossed that river to Shah-yar and Kuchar. At the end of the year he tracked the Tarim to Lake Lob, and proceeded thence in a southward direction to the foot of the great escarpment which in this meridian forms the northern limit of the Tibetan highlands, where he wintered, and made a fresh start across the Altyn Tagh in the spring of 1886. No news having been received of him for many months, his friends had begun to fear for his safety, but all anxiety has been set at rest by recent telegrams from India announcing his safe arrival at Ladakh at the end of the winter. Considering that Mr. Carey travelled without escort and unarmed, and that his journey has been performed on slender means through vast unknown tracts peopled by tribes supposed to be of hostile and fanatical temper, his exploit is one of the most remarkable in the recent annals of adventurous travel.

Northwards of Khatmandu, the capital of Nepal, about 400 miles of entirely new traverse in Nepal and Tibet has been contributed by a native explorer, surnamed M—H., besides a confirmation of the details of a hundred miles of ground previously travelled over. It is regretted that the explorer brought back no determinations of heights, which would have been most interesting, for he crossed the main ridge of the Himalayas by one of the highest passes (the Pangu-la), and approached within 15 miles of Mount Everest. Another native surveyor, R—N., who accompanied Colonel Tanner in his explorations on the Tibetan border in the autumn of 1884, was despatched across Bhutan and the mountains to the east to reach Gyala Sindong, the lowest point yet reached on the Sanpo, and, starting from the left bank of the river, to find his way back to India by *any* practicable route, without recrossing the river. The object was to set at rest the vexed question of the connexion between the Brahmaputra and the Sanpo on the one hand, and the Irawadi on the other. The explorer met with bad luck at the outset, from the fact of there being hostility between Tibet and Bhutan, a state of things which had closed all the passes into Tibet. He therefore had to find his way back to India down the Hachhu and Wongchu Rivers to Baxa, having been detained and kept under surveillance for ten days by the *jongpon* of Chukhajong. His next attempt was made from Dewangiri, whence he proceeded by a pretty direct route to the Monlakachung Pass, and thence to the vicinity of Seh, a very large monastery on the Lhobra River, the position of which had been previously obtained from the north by Lama U—G.'s traverse of 1883. Here, in consequence of the rumours regarding the advance of the Tibet Mission from the south, and of a party of Russians from the north, the officials absolutely stopped his further progress, and kept him in custody for nine days, and then conveyed his party under escort to Seh. From here he escaped with his party by night, and, keeping away from the beaten tracks, found his way to Menchuna (lat. 28° N., long. 92° E.), and thence, *viâ* Tawang, to Odalguri, along the route formerly traversed by Pundit Nain Singh. His work furnishes about 280 miles of new route survey, and throws light on the general geography of Bhutan, besides forming a connexion with the work of Pemberton (1838) from the south, and of the Pundit and the Lama from the north.

Another journey carried out by three English gentlemen through the heart of Manchuria, from south to north from the shores of the Yellow Sea, and from west to east to the Russian settlement of Vladivostock on the Pacific coast, also calls for notice. The party consisted of Mr. H. E. M. James, of the Indian Civil Service; Mr. F. E. Younghusband, of the King's Dragoon Guards; and Mr. H. Fulford, of the Chinese Consular Service. We have received at present brief accounts only of this meritorious achievement; but they are sufficient to show that the travellers made excellent use of their opportunities of gaining accurate information regarding the country, its inhabitants, and products. One of their objects was to ascend the Pei-shan or White Mountain, the highest mountain in the country, which they accomplished, and fixed its altitude by boiling-point and aneroid at 7525 feet, the estimates previously given in books making it 10,000 or 12,000 feet. A very good map of their route was plotted and a copy obligingly communicated to the Society. Mr. James has just arrived in England, and we may hope to have an early opportunity of hearing from his own lips an account of his journey.

The recent addition of Upper Burmah to the territories administered by the Viceroy of India, makes it certain that before long the various questions that have till now puzzled geographers in relation to the course of the rivers that rise in Tibet and flow from that country, will be finally cleared up, and a staff of surveyors under Capt. Hobday is already at work in this country. The sources of the Brahmaputra have already been clearly designated; but doubts still surround the origins of the Irawadi, which actual surveys will, it is to be hoped, before long dispel.

The expectations entertained of the opening up of the still unknown interior of New Guinea, from the southern or British portion of the island, by the expedition of Mr. H. O. Forbes, have, unfortunately, not been fulfilled. Mr. Forbes spent the rainy season in the early part of 1886 in camp, at a short distance inland from Port Moresby, profiting by the enforced inactivity, in cultivating friendly relations with the tribes, learning the languages, and making botanical collections. The remainder of his resources during these months was exhausted, and when at the commencement of the fine season, in April, he made a bold attempt with the great advantage of the companionship of the Rev. J. Chalmers, to reach the summit of the Owen Stanley Range, the term of service of his Amboynese escort had expired, and he could do no more than make a few observations in the rugged country at the foot of the mountains, 75 miles distant from the coast. Since then, he has not been enabled to renew his explorations. We learn, however, that the Government of Victoria has taken the matter in hand, and that a well-equipped Expedition is in preparation for the exploration of the interior, the leadership of which is to be offered to Mr. Chalmers, whose account of his varied explorations along the south-eastern coast-region, given at one of our evening meetings during this session, will be fresh in your memories. The great influence which this experienced missionary pioneer has obtained over the natives, and his knowledge of their habits, inspire us with great hopes in the success of this enterprise, which so much depends on the willingness and fidelity of native followers. Several minor excursions have since been made by various travellers, but very little has been added to our knowledge of the southern portion of the island. Capt. Everill's larger Expedition, fitted out in New South Wales, succeeded in ascending the Fly River and penetrating for some distance up an eastern arm or tributary named the Strickland, which is said to flow in the rear of the range of coast hills, but the map of the parts explored has not yet reached us.

In German New Guinea the discovery of the important river, named after the Empress Augusta, was confirmed by Capt. Dallmann, who in April 1886 ascended it in a small steamer for a distance of 40 miles, and it has since been further navigated by Admiral Von Schleinitz and Dr. Schrader in the steamer *Otilie*, which reached a distance of 224 miles from the mouth, the ship's steam launch ascending 112 miles further, finding still sufficient water, but being obliged to return for want of fuel.

The progress made in the great continent of America, which still offers wide fields for the explorer, and still wider and more productive fields for the physical geographer, remains now to be briefly noticed. As a contribution to physical geography, Mr. John Ball's recently published volume on his voyage round South America and various short journeys inland at various points, merits special mention. It is a model of what serious books of travel that aim at conveying accurate knowledge of the countries visited ought to be.

In Central America, our colleague, Mr. A. P. Maudslay, continues his explorations of the sites and his studies of ruined cities, having returned to Yucatan and Guatemala after reading to us in June last the results of his second and third visits to Central America. His work has great geographical and ethnological as well as antiquarian interest, and his excavations at Copan show that the ruins are those of a city, and not simply of a group of sacred edifices, and that the course of the Copan River has changed somewhat since the remote time at which the massive walls of the buildings had been erected. He believes that he has good ground for concluding that Copan and other cities were abandoned before the Spanish discovery of America in 1492.

Lastly, there remains to notice an admirable labour of exploration in the interior of Brazil by a private scientific Expedition consisting of Dr. Karl von den Steinen, Herr W. von den Steinen, and Dr. Otto Claus. These gentlemen set themselves the task of exploring the course of the Xingu, one of the great southern tributaries of the Amazons. The work was accomplished in 1884, but the first detailed accounts of it were published only in May and June last year. The party proceeded in the first place overland to Cuyaba in the far interior, and, organizing there their caravan, proceeded to the sources of the great river, and descending along the banks of the principal stream, through wild Indian territory, to the point where it becomes navigable, built bark canoes, and paddled down the river a distance of about 1000 miles to its junction with the Amazons. Throughout the journey, in addition to the geographical survey, physical, biological, and anthropological observations were made with the usual thoroughness of German travellers.

It will not be out of place at the present time, when our countrymen are celebrating in all parts of the globe the fiftieth year of the reign of Her Majesty Queen Victoria, to look back on the progress that has been made in geographical knowledge since the commencement of that reign, which dates seven years after the foundation of our Society. The time at my disposal will only admit of an extremely brief review, and I would refer you for more ample details to the valuable memoir drawn up by our esteemed Secretary, Mr. Clements Markham, and published by the Society a few years back, under the title of "Fifty Years' Work of the Royal Geographical Society." A comparison of the maps of fifty years ago with those of the present day shows how great have been the additions made to our knowledge during this period. Foremost, in this respect, must be placed the maps of Africa, the interior of which has been transformed from an almost complete blank, containing little more than hypothetical geographical features derived from the reports of native traders some of which had been handed down to us from the time of Ptolemy, to trustworthy representations, based on precise data, of a vast system of rivers, lakes, and mountains, the existence of which had been wholly unknown to the civilized world. This continent has at length been traversed and re-traversed in all directions, and what remains unknown, consists of details needed to fill in well-ascertained large outlines, rather than essential features still to be discovered. Closely following the progress of geographical research, some of the latest fruits of which it has been my agreeable duty to recognize to-day, when presenting one of the Gold Medals of the Society to Mr. Grenfell, the advance of commercial enterprise is already carrying the pioneers of civilization, recruited from all the principal States of Europe, into the heart of what may without exaggeration be called a newly-found quarter of the globe.

The additions to our knowledge of the great insular continent of Australia have been hardly less remarkable; and the difficulties that have been overcome, and the enterprise and endurance displayed in the investigation of its geography, have never been surpassed in the history of the earth's exploration. Here, too, hand in hand with the advance of geographical knowledge, the domain of civilization has been extended, and the Australian colonies have started into existence fully armed as it were from their birth for the battle of national life. Our fellow-subjects in those distant countries have already displayed their complete fitness to undertake the task of further geographical investigation in that quarter, and to them we may now confidently leave it, assuring them of the continued sympathy and interest with which their labours will be regarded by this Society.

During the period to which I am referring, much also has been done to add to our knowledge of the formerly little understood geography of Central Asia. The Russian geographers on the north, and our own surveyors on the south, have now almost entirely cleared away the darkness that shrouded this part of the earth's surface. The limits and the nature of the central plain lying between the mountains of Siberia and of Tibet have been at length satisfactorily ascertained. The long-discussed problem of the true source of the Brahmaputra has been finally solved. The remarkable plateau of Tibet has been crossed in many directions, and important parts of it have been accurately surveyed, so that here also what remains to be done is rather to complete the delineation of details than to enter upon altogether new investigations.

The large geodetic and topographical operations in connexion with the international demarcation of the northern boundary of Afghanistan will supply all that seems still required to complete the maps of Western Asia between the Indus and the Caspian.

Turning to the American continent, we find a measure of progress which, to say the least of it, quite equals that obtained elsewhere. The exploration of the vast tract lying between the valley of the Mississippi and the Pacific has been carried out by the United States Government with a degree of completeness, both in respect to its topographical representation and its physical characteristics, that has probably never been approached elsewhere, and the whole country has thus been thrown open to the enterprise of the energetic citizens of the United States, who have not been slow to possess themselves of its natural wealth.

In British North America, under less favourable conditions for the prosecution of such systematic surveys as those carried out in the territories of the United States, much has still been done, and the recent opening of the railway connecting Columbia on the Pacific with the eastern Canadian States, and the establishment of another through route to Eastern Asia, will doubtless before long lead to the thorough exploration of the countries through which the railway passes.

The Arctic voyages which had been originally commenced with the hope of finding a practically useful north-west passage to Asia, have long ceased to be animated by such an expectation, and their repetition has been undertaken in the cause of geographical exploration alone.

The results of the numerous expeditions undertaken during the last fifty years, combined with those obtained by land journeys directed from British North America, have very completely defined the southern border of the Polar Sea between Behring Strait and Greenland, and have secured the precise delineation of the somewhat complicated system of channels by which the northern border of the American continent is intersected, and of the islands formed by them, along the Arctic circle. In like manner the boundary of this sea has been determined by voyages directed to the north-east along the northern border of Asia.

The highest latitude reached hitherto is rather less than 83½° N.—that is, within 500 miles of the Pole. The further extension of the exploration of the north of Greenland and of Franz-Josef Land may still be possible, and it is by journeys in this direction that any closer approach to the North Pole will probably be most readily attainable.

I should not omit mention of the memorable voyage to the Antarctic Circle under the most experienced of the Arctic naval commanders of his time, the results of which were of the greatest scientific value, though the difficulties arising from climate that stand in the way of a near approach to the South Pole prevented the Expedition reaching a higher latitude than 78° 11′ S.

Lastly, I may notice the remarkable additions that have been made during this epoch to our knowledge of the ocean, its depths, its temperature, the winds and climates that prevail over its various portions, its currents, and the life with which it abounds. Much of the knowledge thus acquired has supplied completely new and wholly unexpected data with which to deal in our endeavours to interpret the earth's history, and to understand the phenomena it presents to us.

It has been in connexion with the extension of geographical discovery, both that to which I have thus more specially referred, and other similar explorations to which specific reference has not been possible, that there has been accumulated a great mass of knowledge which has had a most important place among the causes which justify our assigning to this epoch its conspicuous character of deserving to be recorded in the history of the present times as the age of scientific progress. There is no room to doubt that it was only by aid of the accumulation of a knowledge of numerous forms of life from various countries, developed under different conditions, that the remarkable generalizations of Darwin and Wallace as to the origin and distribution of species became possible; and that in this sense those great conceptions of the signification of the wonderful variety in the forms of animal and vegetable life, and of the remarkable manner in which they are found associated in various parts of the earth, which it has truly been said are worthy of being classed with the sublime discoveries of Newton, may be regarded as consequences of geographical exploration and discovery. In a somewhat similar manner the progress of geology follows that of geography, and the same may be said of almost all the natural sciences.

In some branches of science the student is able to submit his conclusions to the test of experiment, to vary the conditions of his investigation at his pleasure, and to draw his inferences from the varying results under the changed conditions. In the great laboratory of Nature no such control of conditions is within our power. But by suitable variation of our geographical position, we are able to observe the effects that the physical forces of Nature have produced under varied conditions, and it thus becomes possible to some extent to obtain a substitute for the power of direct experiment.

Properly to estimate the relation between geographical conditions and any observed effect, it is obviously necessary to possess a sound knowledge of the physical forces that may be called into operation in producing that effect, and consequently such a knowledge is of essential importance to every geographer.

I shall not detain you to say anything more on the much-discussed subject of geographical education. I desire to point out, however, that, for such reasons as I have briefly indicated, it is hardly possible to over-estimate the value of exact and scientific geographical research, and that this can only be attained by those who have been properly prepared by previous training. Such a training, it is hoped, may be provided by the instruction which it has been the earnest desire of the Society to see imparted at our chief Universities, and which I trust may not only add to the number of our scientific travellers, but serve generally to throw on many other branches of study that light which an intelligent knowledge of geography alone can supply.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE following is the list of Scholarships, Prizes, Associateships, &c., awarded at the Normal School of Science and Royal School of Mines, South Kensington, for the Session 1886–87:—

First Year's Scholarships—Samuel B. Asher-Aron, William Tate, James A. Schofield, Savannah J. Speak. Second Year's Scholarships—William Blackmore, Henry Sowerbutts.

Edward Forbes Medal and Prize of Books for Biology—Miss Agnes Calvert. Murchison Medal and Prize of Books for Geology—Thomas H. Holland. Tyndall Prize of Books for Physics, Part I.—James W. Rodger. De la Beche Medal for Mining—John W. Sharwood. Bessemer Medal with Prize of Books from Prof. Roberts-Austen for Metallurgy—John Richards. Hodgkinson Prizes for Chemistry—1st Prize, Books, John T. Hewitt; 2nd Prize, Book, William E. Hotson. Frank Hatton Prize for Organic Chemistry—John T. Hewitt.

Associateships (Normal School of Science)—Mechanics (1st Class): Albert Griffiths, Ernest A. Hamilton-Gordon. Physics (1st Class): Arthur T. Simmons. Chemistry (1st Class): John H. Powell, John T. Hewitt; (2nd Class): William R. Bower, Herbert Anderson, Walter D. Severn, Ernest H. Smith, Frank Belcher. Geology (1st Class): Walter G. Ridewood, William F. Hume.

Associateships (Royal School of Mines)—Metallurgy (1st Class): John Richards, André P. Griffiths, James A. Gilmour, Arthur E. Cattermole, Andrew McWilliam; (2nd Class): Sidney Allingham, Hugh Barbour, Arthur M. M. Cooke, George W. Card. Mining (1st Class): John W. Sharwood, Arthur M. M. Cooke. (2nd Class): Cæsar Bello, John Leechman, Andres Franchy, John H. Grant.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. ix. No. 4 (Baltimore, June 1887).—The number opens with a further instalment of Prof. Sylvester's lectures on the "Theory of Reciprocants" (pp. 297–352), which grow in interest as we approach their close—promised in a subsequent number. Lectures xxv. to xxxii. are reported as before by Mr. Hammond, and are accompanied by the lecturer's notes.—M. Maurice d'Ocagne (pp. 354–80) in a paper "Sur une Classe de Nombres remarquables," discusses properties of the numbers symbolically represented by K_m^p. Form a table of squares, as in the case of Pascal's arithmetical triangle, putting in the top left corner K, and in the vertical and horizontal lines the successive numbers 1, 2, 3 . . . The K-numbers will then be, first row 1, second row, 1 1, third row 1 3 1, fourth row, 1 7 6 1, fifth row, 1 15 25 10 1, and so on; the law of formation being, "Multiply the number of the pth column of the qth row by the number of the column, and add to the result the number in the $p-1$th column of the qth row to get the number in the pth column of the $q+1$th row": thus, in the above, $15 = 2 \cdot 7 + 1$, $25 = 3 \cdot 6 + 7$, $10 = 4 \cdot 1 + 6$. These numbers, like those of Bernoulli and Euler, frequently occur in analysis. Many curious results are obtained.—We next have "Extraits de Deux Lettres addressées à M. Craig par M. Hermite" (pp. 381–88). These notes are upon a definite integral formula of Fourier, upon a formula due to Gauss, and upon a formula first given by Weierstrass (an expression for the sine by a product of prime factors).—The volume closes with a notelet by Prof. Franklin, entitled "Two Proofs of Cauchy's Theorem."

Rivista Scientifico-Industriale, April 30.—Recent progress in the theory of the microscope, by Dr. Aser Poli. Reference is made more especially to the labours of Abbe, Helmholtz, Crisp, and others, which have been either originally published or reproduced in the Journal of the London Royal Microscopical Society during the last ten years.—On the electric conductivity of gases and vapours, by Prof. Giovanni Luvini. This is a reply to Prof. Edlund, of Stockholm, who has recently urged several arguments against the author's views regarding the non-conductivity of gases and vapours. These arguments are examined in detail, and it is shown generally that, being mainly based on theoretic grounds or gratuitous assertions, they cannot affect the conclusions to which the author has been led by carefully conducted experiments.—Celestine of Montecchio Maggiore, by G. Bettanini. Preparatory to a complete study of this mineral, a brief description is here given of its crystalline forms and general physical properties. Its specific gravity is shown to be 3·965 at a temperature of 14° C.

Bulletin de l'Académie Royale de Belgique, May.—A new reptile discovered in the Aix-la-Chapelle district, by the Abbé G. Smets. Considerable interest attaches to this discovery recently made in a sandpit at Moresnet, a comparison with the Dinosaurians brought to light in the chalk formations of the New World showing that it is a carapaced Hadrosaurian, the first representative of this family yet found in the eastern hemisphere.—On the electrical phenomena of the excitatory process in the heart of the dog, by Léon Fredericq. This elaborate paper is introduced by an historical summary, from the discovery of the negative variation of the heart of the frog by Kölliker and H. Müller down to the recent studies of Sanderson and Page, with an account of the stroboscopic method employed by Martins to demonstrate the simple nature of the electric variation of the heart in the dog and rabbit. This is followed by a full description of the apparatus employed and experiments made by the author, who has investigated the subject by means of an electrometer modelled on that described by Lovén. A detailed account is added of the results of these researches, illustrated by a series of photographic diagrams.—The solar eclipse of October 29, 1886, observed on the Congo, by A. Merlon. These observations were taken with great care in 3° 7′ S. latitude above the Congo-Kassai confluence to the north of Kwamouth. By means of the data obtained and here supplied, the longitude of the point of observation may now be accurately determined. The instruments used were Abbadie's theodolite, Leroy's chronometer, and Fortin's barometer.

Rendiconti del Reale Istituto Lombardo, June.—On the sulphate of copper, as a remedy against the mildew of the grape-vine, by Prof. E. Pollacci. A crucial chemical experiment is described, showing that the sulphate of copper cannot pass from the grape to the wine except in the minutest quantities. Some critical remarks are added on various other remedies recently proposed against diseases of the vine.

SOCIETIES AND ACADEMIES.

London.

Royal Society, June 16.—"The Electromotive Properties of the Electrical Organ of *Torpedo marmorata*." By Francis Gotch, B.A., B.Sc. London, M.A. Oxon. Communicated by Prof. Burdon Sanderson, F.R.S.

After an introduction, in which the author sets forth the present state of knowledge with reference to the electromotive properties of the electrical organ of Torpedo, he gives an account of his own experimental investigations in three sections.

The first section relates to the nature of the changes produced in the electrical organ by mechanical injury and by heat, and the relation of these changes to those which manifest themselves under similar conditions in muscle and nerve, a subject which has not hitherto been inquired into.

In the second, the duration and the character of the response of the electrical organ to stimulation of its nerve are investigated for the first time by means of the rheotome and galvanometer.

In the experiments which are recorded in the third section, the author has entered on the examination of the after-effects which are produced in the organ by the passage through it of voltaic or induction currents, a subject which has been recently investigated by Du Bois-Reymond.

The author is led by his experiments to believe that the physiological effects produced in the organ by injury, by the passage of currents, and by the stimulation of the electrical nerve, are, notwithstanding that they differ so widely from each other in distribution, duration, and intensity, all phenomena of excitation.

Physical Society, June 25.—Mr. Shelford Bidwell, F.R.S., Vice-President, in the chair.—The following communications were read:—Note on magnetic resistance, by Prof. W. E. Ayrton, F.R.S. and Prof. J. Perry, F.R.S. In the spring o 1886 the authors made experiments on the magnetic inductior through horse-shoe electro-magnets when excited by constan currents. The inductions through different armatures and ai spaces were also measured. The results show that for smal exciting powers the law of parallel resistances is true fo magnetism, taking leakage into account. From experiment made with two electro-magnets, the poles of which were placec at different distances apart, the authors conclude that the mag netic resistance of air is proportional to length, or to length plu a constant. A note on magnetic resistance was read befor the Society on March 12, 1887, by the same authors, describin experiments on two iron rings, one whole and the other divide by a radial saw-cut. Since then the experiments have been re peated with great care by Colonel Swinton and Mr. Sörenson, c the Central Institution. The resulting curves agree with thos previously obtained. On measuring the air space it was foun considerably less than estimated, and the magnetic resistance c air relative to iron (assuming no "surface resistance") come about 1500. Experiments made with different air spaces tc gether with the above seem to show a considerable "surfac resistance." Prof. S. P. Thompson thought dynamo-make had evidence of such "surface resistance" from the care exe cised in avoiding joints in the magnetic circuit wherever possibl and Mr. Bosanquet mentioned some experiments he had recent made on the resistance of joints during the various stages fitting. The changes of resistance are very large, and I concludes that, however good the fit, it is not possible reduce the surface resistance to a negligible quantity.—C sounding coils, by Prof. W. Stroud and Mr. J. Wertheime The paper describes experiments on coils and helices wire which emit sounds when variable electric currents a passed through them. The pitch depends on the frequen of the current variations. The authors believe the soun due to the attractions of adjacent parts of the wire whi cause shortenings and lengthenings as the current increases decreases. To prove this, two identical coils were made, a one of them embedded in plaster of Paris. This gave no sound when the variable current was passed, whilst the other emitted the usual note. It was also found that no sound could be got from a single turn of wire, whilst one and a quarter turns gave an audible sound under the same conditions.—On comparing capacities, by Mr. E. C. Rimington. This is an investigation of the conditions under which the integral current through a galvanometer in a balanced Wheatstone's bridge is zero, when the battery circuit is broken; two adjacent arms, A and D, of the bridge being shunted by condensers of capacities K_1 and K_2. It is shown that $\frac{K_1}{K_2} = \frac{C}{B}$, where C and B are the resistances of the arms opposite to A and D respectively. If A and D be made infinite, the necessity of balancing for steady currents is obviated; but if either of the condensers has an appreciable leakage, corrections are required. The best resistance to give to the galvanometer is shown to be $G = \frac{B(C + D)}{B + C}$, and the conditions under which a telephone may replace the galvanometer are $\frac{K_1}{K_2} = \frac{C}{B}$. The case where all the arms have self-inductions is investigated.—On the effects of change of temperature in twisting or untwisting wires which have suffered permanent torsion, by Mr. Herbert Tomlinson. The author's attention was re-directed to the subject by the note read by Mr. Bosanquet on May 14. Some eight years ago he made experiments on such wires, and upon the effects due to changes produced in the thermal expansibility of the metals, by permanent elongation or compression. Thus if a small square be drawn on the surface of a wire, and the wire subjected to permanent torsion, the square becomes a rhombus, the longer diagonal of which suffers permanent extension, and the shorter diagonal permanent compression. If permanent extension causes an increase in thermal expansibility, and compression a decrease, then a rise of temperature will cause the wire to twist more, and *vice versâ*. With annealed iron wires which have suffered permanent torsion, remarkable effects take place at about a red heat. On heating such a wire, it untwists slightly until a bright red heat is attained, when a sudden twist takes place. On cooling, a sudden untwist occurs at about the same temperature. These effects have been previously observed by Prof. Barrett, who believes them to be connected with the sudden changes in the magnetic properties of iron, and to take place at the same temperature. This latter conclusion was found to be erroneous, for the author exhibited experiments showing that the magnetic change takes place at a temperature decidedly lower than that at which the jerks above referred to, occur.—On permanent magnet ammeters and voltmeters of invariable sensibility, by Prof. W. E. Ayrton, F.R.S., and Prof. J. Perry, F.R.S. The sensibility of ordinary permanent magnet ammeters and voltmeters increases as the strength of the magnet decreases, whereas in those of the Deprety-D'Arsonval type (in which a suspended coil controlled by torsion swings between the poles of a permanent magnet) the reverse effect takes place. By combining the two systems, the authors have devised instruments whose sensibility is unaltered by changes in the strength of the magnet. The torsional control of the D'Arsonval is removed, and a small permanent magnet attached to the swinging coil. As the large permanent magnet changes, the controlling and deflecting forces change in the same proportion, and the deflection for a given current remains unaltered.

Zoological Society, June 23.—Prof. W. H. Flower, F.R.S., President, in the chair.—Mr. Sclater exhibited the skin of a White-nosed Monkey of the genus Cercopithecus, lately living in the Society's Gardens, which appeared to be the *C. ascanias* of Schlegel. It had been obtained by the Rev. W. C. Willoughby from the west shore of Lake Tanganyika, East Africa.—Mr. Sclater also exhibited and made remarks on a specimen of the Pheasant from Northern Afghanistan lately described by him as *Phasianus principalis*.—An extract was read from a letter addressed to the Secretary by Mr. A. H. Everett, of Labuan, reporting the return of Mr. John Whitehead from his expedition to Kina-Balu Mountain, in Northern Borneo, with specimens of some fine new birds, mammals, and other objects of natural history.—Dr. Günther, F.R.S., exhibited and made remarks on a hybrid Pheasant, between a male Golden Pheasant (*Thaumalea picta*) and a female Reeves's Pheasant (*Phasianus reevesi*). Dr. Günther also exhibited a living hybrid Pigeon, produced by a male white Fantail Pigeon and a female Collared Dove (*Turtur risorius*).—Dr. Günther, F.R.S., read a report on the zoological collections made by Capt. Maclear and the other officers of H.M.S. *Flying-Fish* during a short visit to Christmas Island. This island is situated in the middle of the Indian Ocean, south of Java, and had never been before visited by naturalists. The collection, which had been worked out by the staff of the British Museum, consisted of ninety-five specimens, amongst which were examples of two mammals, two birds, two reptiles, two mollusks, two Coleoptera, two Lepidoptera, and a Sponge, new to science.—Mr. F. E. Beddard read a paper on *Myrmecobius fasciatus*, in which he described a remarkable glandular structure stretched across the anterior region of the thorax of this marsupial.—Prof. F. Jeffrey Bell read the sixth of a series of studies on the Holothuridea. The present paper contained descriptions of several new species belonging to the genera *Cucumaria*, *Bohadschia*, and *Holothuria*.—Mr. A. Smith-Woodward read a paper on the fossil teleostean genus *Rhacolepis*. The author gave a detailed description of this Brazilian fossil fish, which had been named and briefly noticed by Agassiz. Three species were defined, and the author showed that the genus had hitherto been erroneously associated with the Percoids and Berycoids. He considered it an Elopine Clupeoid.—A communication was read from Mr. James W. Davis containing a note on a fossil species of *Chlamydoselachus*. The author pointed out that some teeth from the Pliocene of Orciano, Tuscany, figured and described by R. Lawley in 1876, were referable to this newly-discovered genus of Sharks. He named the fossil species *C. lawleyi*.—Mr. Frank E. Beddard read the fourth of a series of notes on the anatomy of Earthworms. The present communication treated of the structure of *Cryptodrilus fletcheri*, a new species from Queensland.—A communication was read from Mr. Roland Trimen, containing observations on *Bipalium kewense*, of which worm he had obtained many specimens from gardens at the Cape.—Dr. Günther gave the description of two new species of fishes from the Mauritius, proposed to be named *Platycephalus subfasciatus* and *Latilus fronticinctus*.—Mr. Sclater read a note on the Wild Goats of the Caucasus, in which he pointed out the distinctions between *Capra caucasica* and *C. pallasi*, which had been until recently confounded together.—Mr. G. Boulenger made remarks on the skull and cervical vertebræ of *Meiolania*, Owen (*Ceratochelys*, Huxley), and expressed the opinion that these remains indicated a Pleurodiran Chelonian of terrestrial and herbivorous habits. The peculiar structure of the tail pointed to a distinct family (*Meiolaniidæ*).—A second paper by Mr. Boulenger contained remarks on a rare American fresh-water Tortoise, *Emys blandingii*, Holbrook, which was shown to be a close ally of *Emys orbicularis* of European fresh waters, but to present distinct differential characters.—Mr. A. Dendy read a paper on the West Indian Sponges of the family Chelininæ, and gave descriptions of some new species.—Mr. H. Seebohm gave the description of a new species of Thrush, from Southern Brazil, proposed to be called *Merula subalaris*.—A communication was read from Mr. R. Bowdler Sharpe, containing the description of a new species of the genus *Calyptomena*, lately discovered by Mr. John Whitehead on the mountain of Kina-Balu, in Borneo, which he proposed to name *C. whiteheadi*.

PARIS.

Academy of Sciences, July 4.—M. Janssen in the chair.—Inauguration of the statue to Nicolas Leblanc, by M. Eug. Peligot. It was stated that this bronze statue, erected to the memory of the illustrious chemist, inventor of artificial soda, was unveiled on June 28 in the court of the Conservatoire des Arts et Métiers.—Note accompanying the presentation of the Report of the English Commission appointed to inquire into M. Pasteur's treatment of rabies, by M. Pasteur. While expressing his great satisfaction at the general tenor and conclusions of this Report, the author referred in feeling terms to the premature death of his distinguished fellow-worker, M. Vulpian, who had not lived to receive this high testimony to the efficacy of the method of cure in which he had taken so much interest.—Note on the first labours of the Observatory of Nice, by M. Faye. After passing in rapid review the services already rendered to science during the construction of the works at this important astronomical station, the author stated that these works are now completed by the erection of the great 0·76 m. telescope, constructed by the brothers Henry, and mounted in Eiffel's wonderful revolving dome, whose diameter exceeds that of the Pantheon at Rome. He added that the International

Geodetic Association has decided to hold the next session of its Permanent Commission in October of this year at the Observatory of Nice.—General method for determining the constant of aberration, by M. Lœwy. In this concluding paper the particular process is described by means of which the research may be made independent of the errors due to the action of the screw in the apparatus already described.—On some double phosphates of thorium and sodium, or of zirconium and sodium, by MM. L. Troost and L. Ouvrard. After examining the action of the metaphosphate, of the pyrophosphate, and orthophosphate of potassa on thorine, zircon, and their salts, the authors here describe the action of the meta-, pyro-, and orthophosphate of soda under analogous conditions. From the study of the double phosphates formed by these bases with soda and phosphoric acid, they are unable to derive any argument in support of the theory that has been advanced on the relation of zircon to thorine in order to justify the formula of a bioxide given to the latter substance. In a future communication the reactions will be described which separate both of these compounds from each other, and bring thorine more into relationship with the protoxides.—Remarks accompanying the presentation of two works on subterranean waters in the present and former geological epochs, by M. Daubrée. In the first of these works, relating to the present epoch, the author describes the manifold action of water in its passage through the rock on the constitution of the terrestrial crust. The underground waters are studied from the several stand-points of their *régime*, their temperature, and their composition. The second work, dealing with past epochs, studies the action of these waters in modifying the original substance of the crust of the earth, and especially in connexion with the distribution of minerals. It is shown generally that the superheated water, whose presence is betrayed by thermal springs and igneous exhalations, slowly and silently brings about great and permanent effects in the interior of the globe, at all times giving rise to mineral deposits of all kinds. By its incessant subterranean circulation, and especially by its chemical work, it accomplishes a sort of vital action, which is perpetuated from age to age.—On an atlas of marine meteorology presented to the Academy by M. Mascart. A limited number of copies of this work have been issued by the Central Meteorological Bureau in connexion with the Exhibition at Havre, and at the expense of a person who desires to remain anonymous. It has been prepared by M. Léon Teisserenc de Bort, and comprises thirty-two charts based on the best published and inedited materials. The first series deals with the mean distribution of pressure, and of the prevailing winds during the different seasons on the surface of the globe. The second is more especially devoted to the study of the Atlantic Ocean, indicating the atmospheric systems, the temperature of the sea, the position of the Arctic and Antarctic floating ice, the line of equal declination, &c. According to the donor's intention the work will be distributed gratuitously to all captains of the mercantile marine who have by their personal observations contributed in any way to the progress of meteorological studies.—Fluorescences of manganese and bismuth (continued), by M. Lecoq de Boisbaudran. In this paper the author deals (1) with two solid solvents, one of which, in the presence of the other, plays the part of a moderately active body, and an active substance fluorescing energetically with one only of these solvents; (2) with two solid solvents, the first of which (α) plays the part of a moderately active body and two active substances fluorescing energetically, one with the two solvents α and β, the other with β alone.—Elements and ephemeris of the planet 267, by M. Charlois. These elements have been calculated by three equatorial observations made at the Observatory of Nice on May 27 and June 9 and 27, 1887. At the instant of opposition on June 5 the planet was of magnitude 13·5.—On the position of the foci in a tangential bundle of plane curves, by M. G. Humbert. From various considerations deduced from Leguerre's theorem, the author arrives at the general proposition that the poles of any three series are the foci of three algebraic curves of the same class, belonging to the same tangential bundle; inversely the real foci at a finite distance from a curve of this bundle constitute a system of poles. —On the synthesis of pilocarpine, by MM. Hardy and Calmels. The synthesis of this substance has been obtained by means of β-pyridino α-lactic acid. It takes place in two phases: (1) transformation of this acid into pilocarpidine; (2) transformation of pilocarpidine into pilocarpine.—On the origin of the striated Bilobites, by M. Ed. Bureau. These tracings, occurring on certain sandstones, are referred to the footprints of some Crustacean of the order of Phyllopods, which cannot at present be more accurately determined.—Observations on the meteor of June 17, 1887, by MM. Waltner and Didier. This meteor, seen at an altitude of about 45° above the horizon near the Mont Parnasse railway-station at 7.45 p.m., was especially remarkable for its extraordinary brilliancy. It disappeared in about five seconds, without any noise or explosion, before reaching the top of the houses.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Matter and Energy (Kegan Paul).—My Microscope: by a Quekett Club Man (Roper and Drowley).—Exercises in Practical Chemistry vol. i. 4th edition: Harcourt and Madan (Clarendon Press).—Bibliographie Générale de Astronomie, vol. i.: Houzeau and Lancaster (Hayez, Brussels). Four-Figure Mathematical Tables: J. T. Bottomley (Macmillan).—Handbook of Fern Allies: J. G. Baker (Bell).—Jahrbuch der Meteorologischen Beobachtungen der Wetterwarte der Magdeburgischen Zeitung, Jahrgang iv. 1885 (Magdeburg).—Actes de la Société Helvétique des Sciences Naturelles Compte Rendu, 1885-86 (Genève).—Compte Rendu des Travaux de la Société Helvétique des Sciences Naturelles, 1886 (Genève).—Mittheilungen der Naturforschenden Gesellschaft in Bern ans dem Jahr 1886 (Bern).—Foods and Food Adulterations: part 1, Dairy Products (Washington).—Journal of Anatomy and Physiology, July (Williams and Norgate).—Mind July (Williams and Norgate).—Journal of the Society of Telegraph-Engineers and Electricians, No. 67, vol. xvi. (Spon).—Folk-Lore Journal, vol. v part 3 (Stock).—Zeitschrift für wissenschaftliche Zoologie, xlv. Band, 3 Heft (Engelmann, Leipzig).—Botanische Jahrbücher für Systematik, Pflanzengeschichte, und Pflanzengeographie, Achter Band, v Heft (Engelmann Leipzig).—The Indian Forester, April, May, and June 1887 (Roorkee).

CONTENTS.

THURSDAY, JULY 21, 1887.

THE MINING INDUSTRY OF NEW ZEALAND.

Report on the Mining Industry of New Zealand. (Papers laid before Parliament, Session 1886.) 8vo, pp. 334. (Wellington, New Zealand, 1887.)

The Hand-book of New Zealand Mines. With Maps and Illustrations. 8vo, pp. 519. (Wellington, New Zealand, 1887.)

THESE volumes, which cover the same ground, and to some extent reproduce the same information, are in great part the result of a personal investigation of the mining districts of our great antipodean colony, made by the Hon. Mr. W. J. M. Larnach, C.M.G., the Minister of Mines. From the Report, which is about six months older than the Hand-book, we learn that the latter has been compiled by the officers of the Mining Department, under the direction of the Minister, in order to furnish systematic information as to the area of mining claims, and as to other particulars concerning the working of mines, which has not hitherto been available. This result has been fairly well attained in the volume before us, which is a valuable summary, arranged topographically, of the condition of the mines actually at work, the description of each district being preceded by an historical sketch of the early explorations. Among these, that describing the progress of discovery on the west coast of the Middle Island is especially interesting, as it goes back as far as 1836, when an early settler, named Toms, "on one occasion was caught and thrown down by a large seal, receiving a severe bite on the thigh, but he escaped death by dealing it some hard blows with his fist on the nose." Other and more serious difficulties were encountered from the opposition of the native inhabitants, whose interests were finally purchased by Sir George Grey and the successive Governors, subject to certain reserves, which at the present time produce an income of about £4000 per annum, and as there are only about a hundred natives on the west coast, they are comfortably fed, housed, and clad, peaceable and sober, and generally respected by their European neighbours. From this part of the colony gold was exported of the value of nearly £12,000,000 sterling between 1864 and 1873, and the yield, though diminished, still continues, with the prospect that the product of alluvial rocks will be more than eclipsed by that of the quartz reefs, some of which have been proved to be extraordinarily rich. The total produce of gold in New Zealand between 1853 and the end of 1885, according to the Report, is 10,789,560 ounces, valued at £42,327,907 sterling, and the Hand-book gives the area of country proved to be auriferous in the three islands as about 21,000 square miles.

The product next in importance to gold, although perhaps it is scarcely to be classed as a mineral, is kauri gum, which is produced at the rate of about 6000 tons annually from deposits in the North Island, which have already yielded upwards of £3,500,000 sterling to the wealth of the colony. The prosperity of Auckland has been largely aided by its kauri gum fields, and the valuable kauri tree, which is only found in the northern forests of the North Island.

The coal of New Zealand seems to be largely of the character of lignite, though some portion is of a more highly carbonaceous character. The output at present is little in excess of 500,000 tons, which suffices for about three-quarters of the consumption of the colony. Several other minerals have been produced in small quantities, but their aggregate value is insignificant when compared with that of the three staples noticed above.

In going over the detailed accounts of the different gold-mines, given in both volumes, we cannot but be struck by the great diversity of the character of the deposits, and this, as might be expected, has led to several interesting modifications in the method of working. Among the more remarkable of these are, the use of a steam dredger for working auriferous alluvial gravels in the channel of the Molyneux River, and a method of lifting similar materials by a water-jet aspirator applied at Gabriel's gully in the Tuapeka district. These are described at some length, but the descriptions and illustrations are not as full and precise as they might be, considering the interest of the subjects. Another novelty is the use of electricity on the large scale for driving a stamping mill at the Phenix Mine, in Otago. The current produced by a pair of turbines of about 100 horse-power and two Brush dynamos is transmitted to a distance of about two miles to the crushing battery, which contains thirty heads of stamps and is driven by a Victoria electromotor and a Leffel turbine conjointly. This is probably the largest application of electric power to mining purposes that has yet been made.

Mining in New Zealand appears to receive greater support from the State than is customary in most other countries, as not only are large sums devoted to the opening up of roads and pack trails through the country, but contributions are made towards the construction of water races and channels for tailings, and subsidies are paid towards prospecting in different localities. These grants are made contingently upon much larger sums being furnished by local or individual effort, and, according to the testimony of the Reports, have been of great value in encouraging discoverers.

A point of interest in connexion with the economics of New Zealand mining is the general establishment of local schools of mines, or, as they are called in some localities, chemistry clubs, in the different mining centres. These are organized apparently on a system somewhat similar to that of the science classes of the Science and Art Department, the instruction being given to the members by means of a staff of seven teachers under the charge of Prof. J. G. Black, of the University of Otago, who travels through the different districts giving lectures and laboratory demonstrations, for periods varying from two to five months at each, according to its size and importance. The course of instruction includes mineral chemistry and assaying, mineralogy and metallurgy, and provision is being made for the addition of the subjects of mining engineering and surveying. The results expected from the scheme are set forth in full, from which it appears that miners will be able to assay ores and metals of every kind, be able to assay their own bullion, and become generally familiar with the metallurgy of the precious metals. Such results will probably not be realized in their entirety, neither is it desirable that they should be, as the presence of a well-educated specialist, an assayer or smelter, for example, may often be of more permanent value to a district than the necessarily superficial knowledge of subjects not immediately connected with their own occupation that the local miners are likely to acquire under the scheme; but there can be no doubt that great good will result from giving them an intelligent interest in mineralogy, and the observation of the phenomena brought under their notice when at their own particular work.

The Hand-book concludes with a description of the principal forest trees of New Zealand, taken from Dr. Hector's "Hand-book of New Zealand." It has also several maps, supplied by Dr. Hector and Mr. Gordon, of the Mines Department. The greater part of the material has been collected by Mr. Patrick Galvin, of Wellington.

We are sorry to see that in the final paragraphs of the preface, Mr. Larnach appeals to the honourable gentleman who may succeed him to improve the work in a second edition; from which we infer that the author has fallen a victim to a Ministerial crisis. If it be so, we have to thank him for what he has done, but if not, we hope that he may have the opportunity of extending and improving the work which he has so worthily begun, instead of leaving it to his successor. H. B.

A CENTURY OF ELECTRICITY.

A Century of Electricity. By T. C. Mendenhall. (London: Macmillan and Co., 1887.)

IN this readable little work, Prof. Mendenhall has striven to depict the origin and growth of many of the modern electric appliances—the telegraph, the dynamo, the telephone, and the electric lamp. He opens with a felicitous quotation from Benjamin Franklin describing with characteristic humour a proposal to hold an electrical party of pleasure on the banks of the Skuylkil, when the healths of all the famous electricians in England, Holland, France, and Germany are to be "drank" in electrified bumpers, under the discharge of guns from the electrical battery. This is followed by a very interesting account of the early development of the experimental science, and in particular of the work of Gilbert and of Franklin. It is satisfactory to note that for once Gilbert's just fame as the creator of the double science of electricity and magnetism is recognized, and his pre-Baconian use and development of the experimental and deductive methods of philosophizing acknowledged. The discoveries of Galvani, Volta, Oersted, and Ampère are set forth in a style which, while losing nothing in accuracy of description, is enlivened by pleasant biographical touches. Speaking of the week during which Ampère wrought out to such brilliant conclusions the train of ideas suggested by Oersted's discovery of the electric deflexion of the magnet, Prof. Mendenhall observes: "It is safe to say that the science has at no other time advanced with such tremendous strides as during that memorable week." The work of Sturgeon in inventing, and of Henry in perfecting, the electro-magnet is duly noted; but we miss, in connexion with electro-magnetic subjects, the name of Prof. Cumming, who did so much to expand and define the growing science.

The vexed question, Who invented the electric telegraph? is here reached, and is very carefully handled. Prof. Mendenhall's frank impartiality in touching on this and sundry other delicate topics of contested priority is worthy of praise. *A propos* of the part taken by Henry in the invention of the electric telegraph, the author gives a sketch of Henry's arrangement of a bell for receiving electric signals, with a polarized lever to strike the bell, as it was exhibited in Albany in 1832. The most technical part of the work is that dealing with duplex and multiplex telegraphy, which is very fully treated, though here we miss the name of La Cour, who preceded Delany in the synchronous distribution of currents. Sir William Thomson's labours in submarine telegraphy, and those of Gaston Planté on accumulators, are emphasized, but not unduly. Respecting the telephone, after noting the early work of Page and the similarity between Reis's telephone transmitter and those used to-day, the author turns to the work of Elisha Gray and Graham Bell in the following terms:—"By a curious coincidence Mr. Gray deposited his specifications and drawings for a speaking-telephone in the United States Patent Office, in the form of a caveat, on February 14, 1876; and on the same day Mr. Bell filed his application for a patent, the latter being received a few hours earlier than the former. The coincidence becomes more interesting when it is remembered that it was also on February 14, 1867, that Wheatstone and Siemens simultaneously presented to the Royal Society their independent discovery of the important fact that dynamo-electric machines could be constructed and operated without the use of permanent magnets." The double coincidence of dates is certainly curious; but the significance of it is marred when we remember, first, that both Wheatstone and Siemens must yield priority of date to Varley, who patented the same discovery on December 24, 1866; and, secondly, that the apparatus described by Bell in the patent application of February 14, 1876, was one in which a separate instrument was employed for every pitch, "each instrument being capable of transmitting or receiving but a single note," and therefore did not describe a speaking-telephone at all. Bell's patent for "the transmission by the same means of articulate speech" was only applied for some ten months later. Due credit is given to Hughes for his well-known research on the microphone, to Edison for his button of lampblack, and to Dolbear for the invention of the electrostatic receiver. The chapter on the electric light is all too short, and might with advantage be expanded. Faraday's splendid discovery of magneto-electric induction, leading to the invention of the dynamo, is admirably recounted, and the important part played by modern American constructors of powerful machines is modestly narrated. A similar remark will apply to the paragraphs upon electric motors, a department of electro-technics which America is likely to make peculiarly her own.

When we reflect that the rapid introduction into British industries of the gas-engine is slow compared with the tremendous rate at which electric motors are being everywhere brought into use in the States, we think that Prof. Mendenhall has under-rated rather than overrated the importance of this item in his account of the developments of the century. Strangely enough, there is in the whole work no mention of that most widely-spread of all electric inventions, the domestic electric bell, nor of its almost forgotten inventor, John Mirand. Prof. Mendenhall has added to the interest of his sketch by supplying a number of illustrative cuts of objects of historic interest, such as Faraday's first magneto-electric machine, and his first transformer or induction-coil. We should have welcomed some account of the great theorists, Coulomb, Laplace, and Weber, who, with Sir William Thomson and Clerk Maxwell, have, by their calculations and mathematical developments, played so leading a part in the progress of the century; but the author would probably have found it impracticable with the plan of his sketch to deal with the labours of these intellectual giants. In his less ambitious aim of popularizing the experimental development of the subject he has succeeded admirably.

OUR BOOK SHELF.

The Fungus Hunter's Guide and Field Memorandum Book, with Analytical Keys to the Orders and Genera, illustrated, and Notes of Important Species. By W. Delisle Hay, F.R.G.S. (London: Swan Sonnenschein, Lowrey, and Co., 1887.)

A FIELD guide and mentor is a welcome companion for the practical botanist, provided it is so compiled as to meet all the requirements of field work, otherwise it is merely "a delusion and a snare." This little volume, unfortunately, belongs to the "otherwise," for it is insufficient, antiquated, and misguiding: insufficient, because it includes only a few species under each genus or sub-genus, and these have been selected without manifest reason; antiquated, because, although dated 1887, it is based upon the state of this branch of science in 1871, and might have been published at that date, for all internal evidence to the contrary; and misguiding, because the errors of 1871 are not corrected, the illustrative figures are entirely without names of the species intended to be represented, and more important or essential species are excluded than many of those included in the lists.

Under each genus or sub-genus in the volume a list is given of "common or notable species,"—each with its scientific name (but without the authority for the specific name, which any botanist would regard as essential); an imaginary popular name, which is useless because imaginary and not real; a short description, rarely sufficient; and letters indicating esculent or poisonous qualities. As only one or two species are given under a genus or sub-genus which has a dozen or more other British representatives, it should have been stated distinctly that there are so many more species which are not named, any of which the collector might meet with in his rambles. Unfortunately the selection of the species favoured with a place has been made with very little judgment. Some are included which are so rare that they have only been found once or twice in this country, whilst others are excluded which are almost sure to be met with in any moderately successful ramble. The fact is patent that the "Hand-book" issued sixteen years ago is accepted as the authorized record for to-day, whereas it is absolutely out of date, and all the great advances made during the intervening period are studiously ignored. The volume is interleaved with ruled paper for notes and memoranda, and we venture to affirm that this is the only useful and unexceptionable portion of the work. The purchaser must judge whether it would not have been more economical to secure a blank memorandum book, since the numerous figures are valueless without names, and the analytical keys ought to have been more accurate and better constructed. M. C. C.

My Hundred Swiss Flowers: with a Short Account of Swiss Ferns. By Mary A. Pratten. (London: W. H Allen and Co., 1887.)

THIS is a very unpretending book, and should be of considerable service to beginners in botany who may wish to carry on botanical studies among the Alps during the month of July or early in August. The writer has selected those Swiss flowers which seem to her "most remarkable, most characteristic of the country, or most commonly seen," and she is, of course, right in thinking that a great many of them will be new to such as make a first visit to the Alps. Her descriptions are clear and sufficiently full, and the illustrations are very good.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Carnatic Rainfall.

MR. H. BLANFORD's authority is so deservedly high, that I have had some hesitation in writing to controvert the conclusions he has adopted in the paper published in NATURE of July 7 (p. 227), entitled "The Eleven-Year Periodical Fluctuation of the Carnatic Rainfall"; and to state my reasons for thinking that there is no real validity in the arguments he uses in favour of "the very high probability that the apparent undecennial fluctuation is no chance phenomenon."

Mr. Blanford brings forward a series of figures which show the mean annual variation of the rainfall during twenty-two years, at a number of stations in that part of Southern India locally known as the Carnatic, from the mean annual rainfall for the Carnatic generally. From these figures he has inferred the appearance of two complete cycles of eleven years, with a dominant periodical fluctuation.

To test the character of this apparent periodicity he obtains from these figures the two first terms of an harmonic expression that shall represent the observed facts for an assumed eleven-year period; and he finds the mean difference between the observed values and those calculated from the adopted harmonic expression to be $\pm 3{\cdot}5$ inches, from which the mean probable error of any of the calculated periodical values is found to be $\pm 0{\cdot}70$ inch.

Now it is apparent that such a series of calculated values has no physical signification whatever. The greater or less degree of difference between the observed and calculated quantities only indicates how far the sums of the terms of the harmonic series employed coincide with the series of observed quantities which the calculated series was designed to represent. It is also obvious that by a sufficiently extended series of terms the calculated quantities might be brought to agree, within any desired degree of approximation, with those observed. No conclusion whatever, therefore, can be based on the amount of the differences above alluded to, so far as any question of periodicity is concerned, and the so-called "probable error" is merely an arithmetical result of the particular form of calculation adopted.

Mr. Blanford goes on to remark that the mean difference between the observed series of values of the annual variation of rainfall and the mean of the whole of them, is $\pm 5{\cdot}2$ inches, with a probable error of the general average of $\pm 0{\cdot}94$ inch.

And here again I am unable to see that any weight can be attached to these figures in connexion with the main point at issue. The mean variation of the series of observed values, from the mean of all of them, will of course be greater than the mean variation of those observed values from a series deliberately calculated so as to correspond with them, such as that obtained by aid of the harmonic series. The introduction of the expression "probable error" of the general average is also likely to be misleading. This too only represents an arithmetical result, and signifies that as in the series of twenty-two observations there is an average departure of $\pm 5\cdot2$ inches from the mean of all the measurements, the probability is that this mean will be within $\pm$ 0·94 inch of the truth, so far as those measurements are to be trusted.

For these reasons I am quite unable to follow the arguments by which it is sought to connect the amounts of these two "probable errors," or to see how they can in any way indicate "the relative probability of this particular variation being the result of a periodic law, and of its being a mere fortuitous series of variations from a constant average."

Neither does there appear to be any justification for assuming that the relative probability of the truth of two hypotheses is represented by the inverse ratios of the probable errors of results derived from them. Still less is there any ground for saying that because the particular series of quantities under discussion relates to a period of twenty-two years, the relative probability just alluded to is thereby increased to the twenty-second power of that ratio, or from about $1\frac{1}{3}$ to 1, to 655 to 1. It is no doubt true that if the probability of an event occurring once be represented by the fraction $\frac{1}{x}$, the probability of its recurring n times in succession will be represented by $\left(\frac{1}{x}\right)^n$; but I fail to see how this affects the question at issue.

RICHARD STRACHEY.

July 11, 1887.

Is Cold the Cause of Anticyclones?

IN a review of Loomis's papers in this volume of NATURE, p. 2, occur the following sentences:—"While all, or nearly all, of the high pressure of anticyclones may be accounted for by the very low temperatures which overspread the same region at the same time along with the resulting upper currents concentrating upon them from adjoining cyclonic regions, it is quite different with the low pressures of cyclones. In the case of cyclones the problem is complicated by the strong winds, the copious precipitation, and the ascending currents, which affect the results in ways which no physicist has yet been able to explain."

This induction of Loomis's, that anticyclones are largely the result of cold, which the reviewer here repeats, is in entire opposition to the deductive views of Ferrel, and I think the discrepancy is to be found in the method used by Loomis in drawing his inductions. In order to investigate the cause of anticyclones, Loomis selected only decided areas of high pressure, and as a consequence his data were almost entirely confined to the winter months, when the temperature accompanying anticyclones is always low. If, however, he had selected more moderate anticyclones, he would have found that in summer anticyclones in the United States are sometimes accompanied by intense heat (90° F. or more). This is especially so in periods of drought. Under these conditions the approach of a cyclone with rain brings a most refreshing cooling. Furthermore, Hahn, attacking the problem by a different method, has obtained results apparently directly opposed to this induction of Loomis. Hahn made a careful study and comparison of the observations obtained last autumn and winter on the Sonnblick and at adjacent mountain and valley stations (see *Meteorologische Zeitschrift*, February and April 1887). One of the most marked results found was that at heights exceeding 1000 metres above sea-level there was an increase of temperature during anticyclones, while a decreased temperature was only found in valleys and near the general level of the earth's surface. Hahn's average results show that the temperature on the Sonnblick, height 3090 metres, rose from an average of $-16°\cdot4$ C. at the average barometric pressure of 509·1 mm. to $-7°\cdot7$ C. at the barometric pressure of 529·3 mm.; while at the same time the average temperature at Schafberg, height 1776 metres, rose from $-9°\cdot0$ C. to $+1°\cdot4$ C.; but on the contrary at Zell-a-See, height 754 metres, the average temperature fell from $-5°\cdot9$ C. to $-8°\cdot9$ C. These results, which show that the larger portion of the atmosphere is warmed instead of cooled within the area of an anticyclone, seems entirely destructive of Loomis's hypothesis that the cooling of the air near the earth's surface is the chief cause of the anticyclone. Hahn's results, however, indicate that the cooling of the air near the earth's surface does increase the pressure somewhat. Thus in October 1886 the barometric minimum occurred at all the stations, both mountain and valley, on the 17th; while the barometric maximum occurred at all of the stations on the 30th. When the difference in pressure between the mountain stations at the time of barometric minimum was compared with the difference in pressure between the same stations at the time of barometric maximum, it was found almost exactly the same; but the difference in pressure between the valley stations and the mountain stations was about 5 mm. greater at the time of maximum pressure than at the time of minimum pressure. Hahn refers this greater range of pressure at the valley stations to the decreased temperature at valley stations during anticyclones, but this still leaves a range of pressure of nearly 20 mm., which the decreased temperature entirely fails to explain. These results of Hahn are in entire accord with the results obtained at Blue Hill Observatory (640 feet above sea), as compared with Mount Washington and with stations near sea-level, which indicate that the cooling in anticyclones is almost entirely confined to within a few hundred feet of the earth's surface (see *Science*, vol. viii. pp. 233 and 281).

In the light of these facts, it seems more reasonable to assume that the warmth found on mountains and the cold in valleys, accompanying anticyclones, are the result, rather than the cause, of anticyclones. Such researches as those of Langley on the solar heat, and of Hahn on the distribution of temperature pressure, &c., in different planes of the atmosphere, indicate that the effect of the sun's heat on the atmosphere is far more complex than some of our text-books on meteorology would have us believe; and instead of the cause of anticyclones being as simple as the reviewer of Loomis's article states, it seems probable that we shall understand the phenomenon of the anticyclone only when we master the problem of the cyclone.

H. HELM CLAYTON.

Blue Hill Observatory, Boston, Mass., U.S., June 28.

Physiological Selection.

I AM perhaps in a position to contribute something practical to the discussion upon Mr. G. J. Romanes's proposal of physiological selection as an improvement upon Darwin's natural selection.

I failed to meet with Mr. Romanes's paper in the Journal of the Linnean Society, and I confess that I did not gain a very clear idea of what he meant by physiological selection, until I read his article in the *Nineteenth Century* for January. His main difficulty appears to be the intercrossing with parent stock, which he thinks would prevent the survival of any varieties naturally selected to become species. Now, my Australian bush experience of the habits of animals and birds satisfies me that this difficulty is mainly, if not entirely, imaginary, and that Nature amply provides against the supposed intercrossing. Any person who has observed the habits of semi-domesticated stock, such as horses and cattle, can scarcely fail to know that migration is a general practice of one sex, and a frequent one of the other. The old always hunt the young males and strangers entirely away to form herds and families of their own, and thus the supposed intercrossing is by one sex at least effectually obviated. But it is also frequently the case that young fillies and heifers, at the same season, take to wandering, for less evident reasons, far from their accustomed haunts, scores of miles, after which they will stop, and attach themselves to another herd and locality as tenaciously as their parents remain in theirs. This of course further tends to prevent intercrossing with parent stocks.

I cannot but think that Mr. Romanes's anxiety to find a solution of his difficulty has led him into serious mistakes, which vitiate his treatment of the subject. For instance, he says (page 59, *Nineteenth Century* for January), "The hypothesis of physiological selection sets out with an attempted proof of the inadequacy of natural selection, *considered as a theory of the origin of species*." I was out walking yesterday when I read this, and I wrote in the margin, "The theory of natural selection is one, not of the origin of species at all, but of the *preservation* of *particular varieties*." On reaching home, I referred to the "Origin of Species" (4th edition, 1866), and was certainly pleased to find that I had adopted Darwin's precise words repeated in several places (see pages 71, 91, 123, &c.). At page 91 he says:—"Some writers have misapprehended or objected to the term natural selection. Some have imagined that natural selection induces variability; whereas it implies only the *preservation* of *such varieties as occur*, and are beneficial under the conditions of life." Was not this a prophet? Yea, I say unto you and more than a prophet! Of course if the conditions of life are unfavourable, the incipient variations cannot become species. But surely it is obvious that in *variation* is the real *origin* of species. Variations must occur before the selection of some of them in preference to others. To consider the theory of natural selection as a theory of the origin of species, is therefore clearly an error. In his "Origin of Species" Darwin certainly expounded variation, and I might have ventured to think that as the book deals more largely with the subsequent selection of a few varieties to survive as species at the expense of many extinguished, a more exact title for it would have been "The Evolution of Species." But what says the great master? See page 71:—"Owing to this struggle for life, any *variation*, however slight . . . will tend to the *preservation* of *that individual*, and will generally be inherited by its offspring. . . . I have called this principle by which each slight *variation*, if useful, is *preserved*, by the term natural selection, in order to mark its relation to man's power of selection." And who will not recognize the wisdom of his selection of the term? It has been before observed that the "*Ascent* of Man" would seem a more accurate title than the "*Descent* of Man." But I have no doubt that his reasons for preferring the latter were equally cogent.

But Mr. Romanes proceeds:—"This proof is drawn from three distinct heads of evidence. (1) The inutility to species of a large proportional number of their specific characters. (2) The general fact of sterility between allied species, which admittedly cannot be explained by natural selection, and therefore has hitherto never been explained. (3) The swamping influence of even useful variations of free intercrossing with the parent form." I have advanced, I think, ample reasons why No. 3 may be regarded as imaginary, and which therefore reduce the value of No. 2 to a minimum. No. 1 depends entirely upon the definition of "*utility*." Has this word any real significance outside human interests and considerations? The idea of utility, if extended to Nature's operations, may, it seems to me, apply to the interests of any other variation than the one whose specific characters are in question, which may therefore be, without compunction or regret, sacrificed to the most fit, as we know that innumerable species have been extinguished in the interest of those that supplanted them. But utility to Nature may be the extinction of one variation and the preservation of another. As Mr. Romanes's whole paper is built upon what I have already quoted from it, I need scarcely follow it any further. With your permission, however, I have another remark to make.

Mr. Romanes seems to me to have been much exercised by the consideration of the intercrossing with parent forms, and, not knowing of the simple solution given above, to have cleverly invented his physiological selection to escape from the dilemma. Of course Nature is not clever, but simple in its operations. I was always much impressed with what appeared to me a greater difficulty, which might be thought to have a clearer title to be called "physiological selection." I allude to a general tendency in the (human at least) sexes to prefer a mate with opposite characteristics, with the apparent result of insuring mediocrity in the progeny. Thus, as a general rule, the tall prefer the short; the dark, the fair; the wise, the silly; &c., and *vice versâ*. Variation is, on the other hand, apparently insured to a large extent by the differences between parents, but still it would seem that the tendency should, *cæteris paribus*, be inevitably towards a mean in the progeny. The general migration, however, as above indicated, of young males and females, gives plainly ample opportunity for the preservation of viable variations, besides others which experience and care will doubtless discover.

Melbourne, April 11. H. K. RUSDEN.

Weight, Mass, and Force.

APPLICATIONS of the data previously given, in the extract from the American journal, to the dynamical principles of varied motion are easily provided for Mr. Hayward. Take the following: "Determine the weight of the greatest train the Strong locomotive can take up a 96-feet grade from rest at one station to stop at the next station a mile off in four minutes, taking the brake power as a resistance of 400 lbs. to a ton."

The main points at issue, however, are whether the language of the engineer, and in fact the usage of our own and other languages, is scientifically correct or incorrect in its use of the words *weight* and *weighing*; and whether the mathematician is to be allowed to restrict the word weight to the subsidiary sense of force of attraction by the earth.

It is of great importance that this question of dynamical terminology should be thoroughly thrashed out now, before Mr. Hayward's Committee on Dynamics, of the Association for the Improvement of Geometrical Teaching, prepare their final report on the subject. A. G. GREENHILL.

Woolwich, July 11.

The Sky-coloured Clouds.

ON the evenings of June 14, 18, and 19 there was a feeble reappearance in Sark of the sky-coloured clouds, as I may call them in default of a better name, which were so brilliant in the twilights of the last two summers. Though the display this month has been comparatively faint, it has been unmistakably of the same character. I have seen nothing of these clouds since the 19th in travelling in the Channel Islands and through France.

Geneva, June 29. T. W. BACKHOUSE.

P.S.—*Chamounix, July* 13.—I have seen one more display—a brilliant one seen from this neighbourhood on the 6th inst.—T. W. B.

The Migrations of Pre-Glacial Man.

THE question raised by "Glaciator" has been treated by me in a paper entitled "The Faunas of the Ffynnon Beuno Caves and of the Norfolk Forest Bed" in the *Geological Magazine* for March 1887. I there stated that, "Although man probably reached this country from the east, it seems to me equally clear that he must also have arrived here with the reindeer from some northern source during the advance of glacial conditions." Though the Norfolk Forest Bed fauna contains abundant remains of deer and of other animals suitable as food for man, it is curious that so far no implements or other traces of man have been found there. The Forest Bed contains in the main the fauna of an eastern area, as the river on the banks of which the animals roamed flowed from the south-east. If pre-glacial man arrived in this country from the east or south, we should therefore expect to find evidences of this in the Forest Bed. On the other hand, wherever the remains of northern animals, such as the reindeer, mammoth, and rhinoceros, occur in any abundance, there we almost invariably find traces of man. Now that we know that man arrived in this country before the climax of the Ice age, as proved by the explorations carried on for several years at the Ffynnon Beuno Caves (amply confirmed also by this year's researches), it seems but natural to infer that man arrived in this country with the northern animals as they were compelled to migrate southwards by the gradually advancing glacial conditions, and that he kept mainly with the reindeer near the edge of the advancing ice. HENRY HICKS.

ABSTRACT OF THE RESULTS OF THE INVESTIGATION OF THE CHARLESTON EARTHQUAKE.[1]

I.

THE amount of information now in possession of the United States Geological Survey, relating to the Charleston earthquake, is probably larger than any of similar nature ever before collected relating to any one earthquake. The number of localities reported exceeds 1600. The sources of information are as follow: (1) we are deeply indebted to the U.S. Signal Service for furnishing us the reports of their observers; and (2) equally so to the Lighthouse Board, which has obtained and forwarded to us the reports of keepers of all lighthouses from Massachusetts to Louisiana, and upon the great lakes; (3) to the Western Union Telegraph Company, which instructed its Division superintendents to collate and transmit many valuable reports; (4) to the associated Press, which has given us access to the full despatches (with transcripts thereof) which were sent over the wires

[1] Paper read before the National Academy of Sciences at Washington, on April 19, 1887, by C. E. Dutton, U.S.A., and Everett Hayden, U.S.N., U.S. Geological Survey.

centering at Washington during the week following the earthquake; (5) to geologists and weather bureaus of several States, who have kindly exerted themselves in this matter and collected much important information; (6) to a considerable number of scientific gentlemen who have distributed for us our circular letters of inquiry in special districts,—notably, Profs. W. M. Davis, C. G. Rockwood, J.P. Lesley, T. C. Mendenhall, and Messrs. W. R. Barnes, of Kentucky, and Earle Sloan, of South Carolina; (7) to a large number of postmasters in the Eastern, Central, and Southern States; and, finally, to hundreds of miscellaneous correspondents throughout the country.

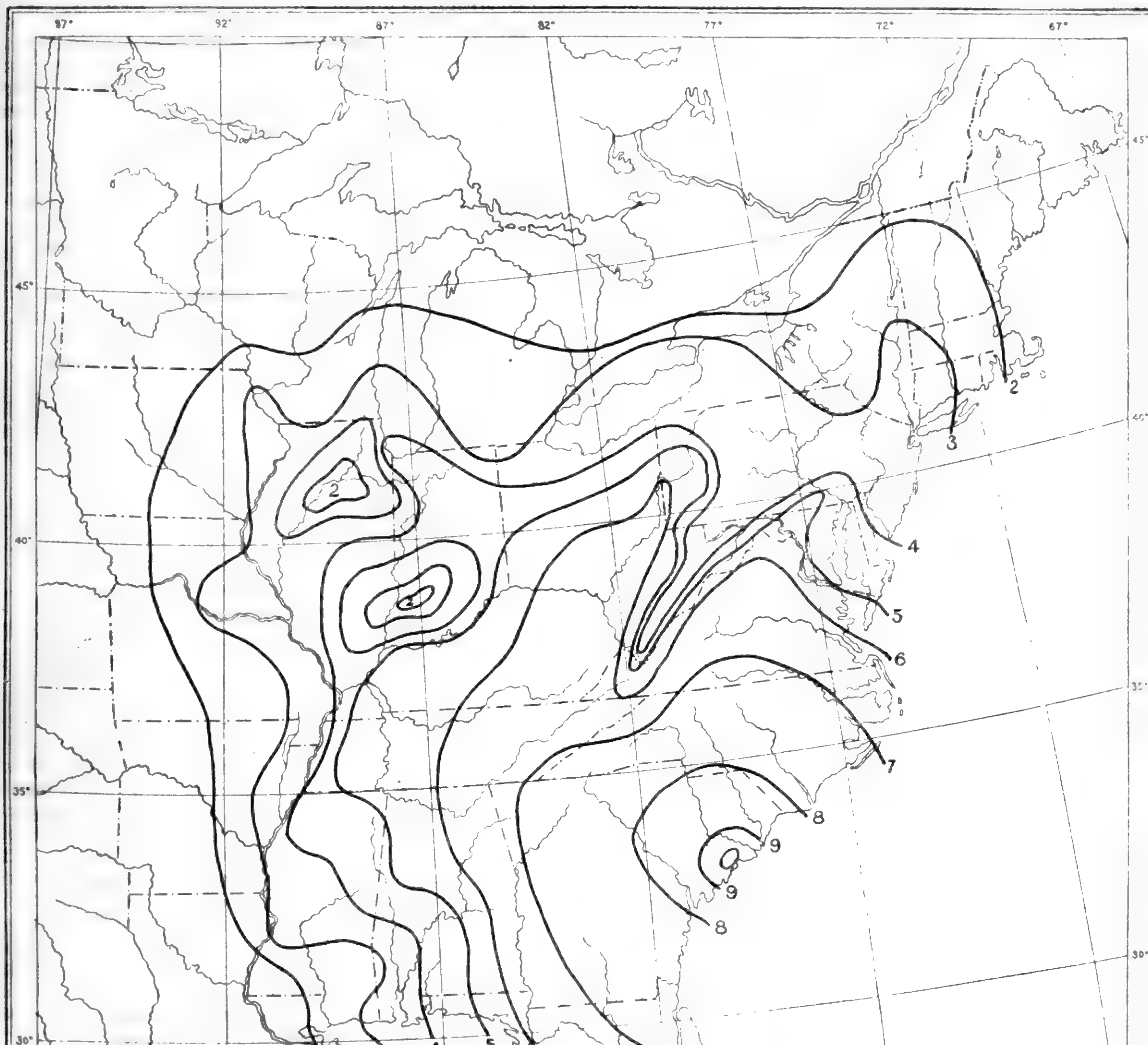
In collecting this information, a printed list of questions was prepared. This practice has been resorted to in Europe and in Japan with considerable success, and the questions which have been devised for distribution in those countries have been prepared with great skill by some of the ablest investigators of earthquakes. Prof. C. G. Rockwood, of Princeton, has also been in the habit of distributing formal questions of this character in this country whenever apprised by the newspapers of a notable shock. Availing ourselves of his advice and assistance, questions prepared by him were printed and widely distributed. They were much fewer and more simple than those employed in Europe, because European investigators depend almost wholly upon the educated classes to answer them, while in this country the uneducated but intelligent and practical classes of the people must be the main reliance. These questions were designed to elicit information: (1) as to whether the earthquake was felt, (2) the time of its occurrence, (3) how long it continued, (4) whether accompanied by sounds, (5) the number of shocks, (6) general characteristics which would serve as a measure of its intensity and indicate the kind and direction of motion.

It is to be observed that the only information to be hoped for which can have even a roughly approximate accuracy is the time of transit of the shock. The degree of approximation in the time data actually obtained will be adverted to later. Special effort was made to obtain information as to the relative intensity of the shocks in all parts of the country. At the very outset a serious difficulty presents itself. In the estimates of intensities there is no absolute measure. What is really desired is some reliable indication which shall serve as a measure of the amount of energy in any given portion of the wave of disturbance as it passes each locality. The means of reaching even a provisional judgment are very indirect, and qualified by a considerable amount of uncertainty. To estimate the force of a shock, we have no better means than by examining its effects upon buildings, upon the soil, upon all kinds of loose objects, and upon the fears, actions, and sensations of people who feel it. In view of the precise methods which modern science brings to bear upon other lines of physical research, all this seems crude and barbarous to the last degree. But we have no other resource. Even if it were possible to obtain strictly comparative results from such facts, and decide with confidence the relative measure of intensity which should be assigned to each locality, we should have gained measures only of a series of local surface intensities and not of the real energy of the deeply-seated wave which is the proximate cause of the surface phenomena. Notwithstanding the indirect bearing of the facts upon the real quantities we seek to ascertain, and their apparently confused and distantly related character, they give better results than might have been supposed. When taken in large groups, they give some broad indications of a highly suggestive character, and though affected with great inequalities which for the time being seem to be anomalous, these anomalies are as instructive as the main facts themselves.

We have given the preliminary plotting of the intensities in the map before you. The first point to which we shall invite attention is the magnitude of the area affected by the shocks. It was sensibly felt in Boston, which is the most distant point on the Atlantic coast from which affirmative reports have been received. From Maine the answers are all negative. Most of those from New Hampshire are negative, but two or three positive ones show clearly that it was felt in sensitive spots. In Vermont, affirmative reports come from St. Johnsbury and Burlington on Lake Champlain. No positive reports come from the province of Quebec. In New York State it was felt in the vicinity of Lake George, and at Lake Placid and Blue Mountain Lake in the Adirondacks. In Ontario, it was quite noticeable in several localities, though the great majority of reports from that place are negative. In Michigan, it was noted in several places, and at Manistee Lighthouse, on Lake Michigan, the trembling was strongly marked. In Winconsin, though most of the reports are negative, it was felt quite strongly at Milwaukee, and was also noticed at Green Bay, and at La Crosse on the Mississippi, 967 miles from Charleston, the remotest point in the United States which gives a positive answer. In Central Iowa and Central Missouri, it was unmistakably felt. In Arkansas, the eastern portion of the State, from sixty to seventy-five miles west of the Mississippi, gives numerous favourable reports. In Louisiana, the reports are mostly negative, but numerous persons in New Orleans felt the shocks and recognized their nature. In Florida, it was universally felt, and in the northern part of the State was severe and alarming. From the Everglade region, of course, no reports have been received, as it is uninhabited; but in some of the Florida Keys it was felt in notable force. From Cuba a few reports have come, and the most distant point in that island which was shaken was Sagua la Grande, where the vibration was very decided. Lastly, a report comes from Bermuda, 1000 miles distant from Charleston, which leaves little doubt that the tremors were sensible there.

The area within which the motion was sufficient to attract the attention of the unexpectant observer would be somewhat more than circumscribed by a circle of 1000 miles' radius, and the area of markedly sensible shaking would, including the oceanic area, be somewhere between two and a half and three million square miles. In this estimate, however, only well-defined seismic movement of notable force is considered. There are reasons for believing that by proper instrumental observation the movement could have been detected over a much greater area. In the first place it is to be noted that the peripheral portions of the observed area lie in districts which are rather thinly populated, sometimes, also, in districts which from the nature of the ground do not disclose forcibly the passing shock. Furthermore, the passing wave in the outer portions of the area was almost everywhere of an undulatory character and of great wave-length, and while still retaining a large amount of energy, did not often dissipate itself into those smaller and shorter tremors which are very much more likely to attract attention, though really possessing very much less energy. Six hundred miles from the origin the long swaying motion was felt, and was often sufficient to produce sea-sickness, yet was unaccompanied by sound or by the tremulous motion due to short waves.

It will be observed upon the map that there are several large tracts which show a comparatively feeble intensity, while completely surrounding them is the general area of greater intensity. The most conspicuous of these areas of silence is the Appalachian region. The facts here are extremely interesting and suggestive. It has been generally supposed that a mountain-range serves as a barrier to the propagation of earthquakes—not from any known relation of cause and effect, but merely as the result of observation. In Japan it is universal testimony that the central range of the island marks the dividing line between earthquake and no earthquake. The shocks so frequent there are seldom or never felt beyond the mountains. A similar conclusion has been drawn from South American earthquakes, and also from those which have visited Southern Italy. As soon as the data in the earlier stages of the inquiry began to indicate insulated areas of minimum action, they were completely investigated, and every effort has been made to secure full data from them. The result has been to show satisfactorily that such was the case. The Appalachian belt south of Middle Pennsylvania disclosed a few spots where the shaking was considerable, but in the main it was but slightly affected



ISOSEISMALS OF THE CHARLESTON EARTHQUAKE ROSSI-FOREL SCALE

until we reach the extreme southern portion of this range, where the shocks begin to be somewhat vigorous, even in the mountains. West and north-west of the range, however, the force of the undulations resumes even more than its normal vigour. In Eastern Kentucky and South-Eastern Ohio, the force of the shocks was very considerable, causing general alarm. Chimneys and bricks were shaken down, and the oscillation of the houses was strongly felt. In South-Eastern Ohio, nearly every theatre, lodge, and prayer-meeting, was broken up in confusion. It does not appear that the Appalachians offered any sensible barrier to the progress of the deeper waves, but it does appear that they affected in a conspicuous degree the manner in which the energy of the waves was dissipated at the surface. Another minimum area was found in Southern Indiana and Illinois, and also in Southern Alabama and Mississippi. There is a curious circumstance connected with the minimum area in Indiana and Illinois. On February 6 last, an earthquake of notable force occurred in just this locality. Circulars were sent out at once, and on plotting the isoseismals they showed a singular coincidence in almost exactly filling the vacancy or defects of intensity of the Charleston earthquake. At present there is nothing to indicate whether this coincidence is accidental or whether there is some hidden relation.

Where the waves passed into the newer delta region of the lower Mississippi, the surface intensity of the shocks rapidly declined. This is indicated in the map by the compression of the isoseismals in those localities. We incline to the opinion that this sudden diminution of the intensity is due to the dissipation of the energy of the waves in a very great thickness of feebly elastic, imperfectly consolidated, superficial deposits. It is a matter of common observation in all great earthquakes that the passage of the principal shocks from rigid and firm rocks into gravels, sands, and clays is, under certain circumstances, attended with a local increase in the amplitudes of the oscillations and in the apparent local intensity and destructiveness, and the reason for it is intelligible. But where such looser materials are of very great thickness and great horizontal extent the reverse should be expected. For when a wave passes from a solid and highly elastic medium into a less solid and imperfectly elastic one, the amplitude may be suddenly increased at the instant of entering; but so rapid is the extinction, that, if the new medium be very extensive, the impulse is soon dissipated.

Many reports throughout the Central States indicate localities of silence which are not expressed upon the map. The reason for omitting them is that it has been impracticable to secure a sufficient density of observation (*i.e.* a sufficient number of reports per unit area) to enable us to mark out and define these smaller areas with very great precision. To do this for the whole country would require some tens of thousands of observations and the expenditure of tens of thousands of dollars to systematize and discuss the data. A map shaded to show the varying intensity by varying the depth of the shading would have a mottled appearance, in which the mottling would be most pronounced in the areas of a little below the mean intensity, say between the isoseismals 3 and 5. This fact is of great importance in the interpretation of the isoseismals, for the omission to consider it results in giving to the middle isoseismals too high a value. In any isoseismal zone, what we should like to ascertain is the mean intensity of the whole area included within that zone. As a matter of fact, the data we possess consist more largely of maximum than of minimum or average intensities, and therefore tend to considerably augment the mean derived intensity above the true mean. This will become apparent by an inspection of the map where the zones of 5, 6, and 7 intensity are disproportionately broad, while those of 3 and 4 are disproportionately narrow. We have not attempted to allow for this source of error, though fully aware of it, because we had no means of determining what allowance to make. We have drawn the lines wholly upon the face of the returns, and the investigators who may attempt to utilize our results must grapple with the corrections as best they may.

Throughout the States of North Carolina, South Carolina, Georgia, and North-Eastern Florida, and in general anywhere within about 250 miles of the centre, the energy of the shocks was very great. At Columbia, Augusta, Raleigh, Atlanta, and Savannah, the consternation of all the people was universal. The negroes and many of the poor whites were for a week or two not exactly demoralized, but intensely moralized, giving themselves to religious exercises of a highly emotional character, the stronger and deeper natures among them being impressed with a feeling of awe, the weaker natures with a feeling of terror. And this was general throughout the large region just specified. In all of the large towns within 200 miles of Charleston more or less damage was suffered by houses and other structures. Walls were cracked to such an extent as to necessitate important repairs; dams were broken, chimneys were overthrown, plastering shaken from ceilings, lamps overturned, water thrown out of tanks, cars set in motion on side tracks, animals filled with terror, fowls shaken from their roosts, loose objects thrown from mantels, chairs and beds moved horizontally upon the floor, pictures banged against the walls, trees visibly swayed and their leaves agitated and rustled as if by a wind. These occurrences were general, and were more strongly marked until they became terrifying and disastrous as the centre of the disturbance was approached. At Augusta, 110 miles distant from the epicentrum, the damage to buildings was considerable; and at the arsenal in that place the commanding officer's residence was so badly cracked and shattered as to necessitate practical reconstruction. In Columbia, 100 miles distant, the shock was very injurious to buildings and appalling to the people, but no substantial structures were actually shaken down. In Atlanta, 250 miles distant, there was no worse injury than falling chimneys and some slight cracks in the walls, but the houses were instantly abandoned in great alarm and confusion by their occupants, and many preferred passing the night in the streets to re-entering their dwellings. At Asheville, N.C., 230 miles distant, and at Raleigh, 215 miles distant, the shocks were quite as vigorous as at Atlanta.

Coming nearer the seismic centre we find the intensity increasing on all sides as we approach it. The region immediately about the epicentrum in a great earthquake always discloses phenomena strikingly different from those at a distance from it, and the differences are not merely in degree but also in kind. The phenomena characteristic of the epicentral area cease with something like abruptness as we radiate away from the epicentrum. The central phenomena are those produced by shocks in which the principal component of the motion of the earth is vertical. Proceeding outwards, these predominating vertical motions pass, by a very rapid transition, into movements of which the horizontal component is the greater, and in which the undulatory motion becomes pronounced. The epicentrum, and the zone immediately surrounding it, is the portion of the disturbed tract which merits the closest attention, for it is here that we may find the greatest amount of information concerning the origin and nature of the earthquake. To appreciate this we will venture to offer some theoretical considerations.

Allusion has already been made to the indefinite character of the data used for estimating the intensity of the shock. There is no unit of intensity which is at present available. In selecting certain effects of an earthquake to characterize varying degrees of intensity, the most that can be hoped for is a means for discriminating whether the relative energy of a shock is greater or less in one locality than in another. But how much greater and how much less—in conformity with what law—is a problem which remains to be solved. An earthquake impulse, however, is a form of energy transmitted as an elastic wave through the deeply-seated rocks, and its propagation and varying intensity are subject to the laws of wave-motion There must be, therefore, some typical law governing the rate at which such a wave diminishes the intensity of its effects as it moves onward. To anticipate the objection that this typical law would apply only to a medium which is perfectly elastic, homogeneous, and isotropic, while the rocks are far from being so, we reply that we have investigated the objection, and are satisfied that while it has some validity, the effect of these inequalities is not great enough to seriously impair the applicability of the law, nor to vitiate greatly the results to be deduced from it. The analysis we offer is a novel one. We attach considerable importance to it, and the consequences which flow from it are somewhat remarkable.

(*To be continued.*)

EXPERIMENTS ON THE SENSE OF SMELL IN DOGS.[1]

I ONCE tried an experiment with a terrier of my own, which shows, better than anything that I have ever read, the almost supernatural capabilities of smell in dogs. On a Bank holiday, when the Broad Walk in Regent's Park was swarming with people of all kinds, walking in all directions, I took my terrier (which I knew had a splendid nose, and could track me for miles) along the walk, and, when his attention was diverted by a strange dog, I suddenly made a number of zigzags across the Broad Walk, then stood on a seat, and watched the terrier. Finding I had not continued in the direction I was going when he left me, he went to the place where he had last seen me, and there, picking up my scent, tracked my footsteps over all the zigzags I had made, until he found me. Now, in order to do this, he had to distinguish my trail from at least a hundred others quite as fresh, and many thousands of others not so fresh, crossing it at all angles.[2]

The object of the experiments about to be described was that of ascertaining whether a dog, when thus distinguishing his master's trail, is guided by some distinctive smell attaching to his master's shoes, to any distinctive smell of his master's feet, or to both these differences combined.

I have a setter-bitch, over which I have shot for eight years. Having a very good nose, she can track me over immense distances, and her devotion to me being very exclusive, she constituted an admirable subject for my experiments.

These consisted in allowing the bitch to be taken out of the kennel by someone to whom she was indifferent, who then led her to a pre-arranged spot from which the tracking was to begin. Of course this spot was always to leeward of the kennel, and the person who was to be tracked always walked so as to keep more or less to leeward of the starting-point. The district—park-lands surrounding a house—was an open one, presenting, however, numerous trees, shrubberies, walls, &c., behind which I could hide at a distance from the starting-point, and so observe the animal during the whole course of each experiment. Sundry other precautions, which I need not wait to mention, were taken in order to insure that the bitch should have to depend on her sense of smell alone, and the following are the experiments which were tried :—

(1) I walked the grass-lands for about a mile in my ordinary shooting-boots. The instant she came to the starting-point, the bitch broke away at her full speed, and, faithfully following my track, overtook me in a few minutes.

(2) I set a man who was a stranger about the place to walk the park. Although repeatedly put upon his trail by my servant, the bitch showed no disposition to follow it.

(3) I had the bitch taken into the gun-room, where she saw me ready to start for shooting. I then left the gun-room and went to another part of the house, while my gamekeeper left the house by the back door, walked a certain distance to leeward in the direction of some partridge-ground, and then concealed himself. The bitch, who was now howling to follow me, was led to the back door by another servant. Quickly finding the trail of the gamekeeper, she tracked it for a few yards; but, finding that I had not been with him, she left his trail, and hunted about in all directions for mine, which, of course, was nowhere to be found.

(4) I collected all the men about the place, and directed them to walk close behind one another in Indian file, each man taking care to place his feet in the footprints of his predecessor. In this procession, numbering twelve in all, I took the lead, while the gamekeeper brought up the rear. When we had walked two hundred yards, I turned to the right, followed by five of the men; and at the point where I had turned to the right, the seventh man turned to the left, followed by all the remainder. The two parties thus formed, after having walked in opposite directions for a considerable distance, concealed themselves, and the bitch was put upon the common track of the whole party before the point of divergence. Following this common track with rapidity, she at first overshot the point of divergence; but, quickly recovering it, without any hesitation chose the track which turned to the right. Yet in this case my footprints in the common track were overlaid by eleven others, and in the track to the right by five others. Moreover, as it was the gamekeeper who brought up the rear, and as in the absence of my trail she would always follow his, the fact of his scent being, so to speak, uppermost in the series, was shown in no way to disconcert the animal when following another familiar scent lowermost in the series.

(5) I requested the stranger before mentioned to wear my shooting-boots, and in them to walk the park to leeward of the kennel. When the bitch was led to this trail, she followed it with the eagerness wherewith she always followed mine.

(6) I wore this stranger's boots, and walked the park as he had done. On being taken to this trail, the bitch could not be induced to follow it.

(7) The stranger walked the park in bare feet; the bitch would not follow the trail.

(8) I walked the park in bare feet: the bitch followed my trail; but in quite a different manner from that which she displayed when following the trail of my shooting-boots. She was so much less eager, and therefore so much less rapid, that her manner was suggestive of great uncertainty whether or not she was on my track.

(9) I walked the park in new shooting-boots, which had never been worn by anyone. The bitch wholly refused to take this trail.

(10) I walked the park in my old shooting-boots, but having one layer of brown paper glued to their soles and sides. The bitch was led along my track, but paid no attention to it till she came to a place where, as I had previously observed, a small portion of the brown paper first became worn away at one of my heels. Here she immediately recognized my trail, and speedily followed it up, although the surface of shoe-leather which touched the ground was not more than a few square millimetres.

(11) I walked in my stocking-soles, trying first with new cotton socks. The bitch lazily followed the trail a short distance and then gave it up. I next tried woollen socks which I had worn all day, but the result was the same, and therefore quite different from that yielded by my shooting-boots, while more resembling that which was yielded by my bare feet.

(12) I began to walk in my ordinary shooting-boots, and when I had gone fifty yards, I kicked them off and carried them with me, while I continued to walk another three hundred yards in my stocking-soles; then I took off

[1] Paper read by Mr. George J. Romanes, before the Linnean Society, on December 16, 1886. Reprinted from the Linnean Society's Journal—Zoology, vol. xx.

[2] "Mental Evolution in Animals," pp. 92-93; where also see for additional remarks of a general kind on the sense of smell in different animals.

my stockings, and walked another three hundred yards on my bare feet. On being taken to the beginning of this trail, or where I had started in my shooting-boots, the bitch as usual set off upon it at full speed, nor did she abate this speed throughout the whole distance. In other words, having been once started upon the familiar scent of my shooting-boots, she seemed to entertain no doubt that the scent of the stocking-soles and of the bare feet belonged to me; although she did not clearly recognize them as belonging to me when they were not continuations of a track made by my shooting-boots (10 and 11).

(13) I requested a gentleman who was calling at the house, and whom the bitch had never before seen, to accompany me in a conveyance along one of the carriage-drives. At a distance of several hundred yards from the house I alighted in my shooting-boots, walked fifty yards beside the carriage, again entered it while my friend alighted and walked two hundred yards still further along the drive. The bitch ran the whole 250 yards at her full speed, without making any pause at the place where the scent changed. This experiment was subsequently repeated with other strangers, and with the same result.

(14) I walked in my ordinary shooting-boots, having previously soaked them in oil of aniseed. Although the odour of the aniseed was so strong that an hour afterwards the path which I had followed was correctly traced by a friend, this odour did not appear to disconcert the bitch in following my trail, for she ran me down as quickly as usual. It was noticed, however, by the friend who took her to the trail that she did not set off upon it as instantaneously as usual. She began by examining the first three or four footsteps with care, and only then started off at full speed.

(15) Lastly, I tried some experiments on the power which this bitch might display of recognizing my individual odour as emanating from my whole person. In a large potato-field behind the house, a number of labourers had been engaged for eight or ten hours in digging up and carrying away potatoes all the way along half-a-dozen adjacent "drills." Consequently, there was here a strip of bared land in the field about twenty yards wide, and a quarter of a mile long, which had been thoroughly well trampled over by many strange feet. Down this strip of land I walked in a zigzag course from end to end. On reaching the bottom I turned out of the field, and again walked up a part of the way towards the house, but on the other side of a stone wall which bounded the field. This stone wall was breast high, and was situated nearly a hundred yards to windward of my previous course through the potatoes. The bitch, on being led out of the house, was put upon my trail at the top of the field, and at high speed picked out my trail among all the others, following roughly the various zigzags which I had taken. But the moment she gained the "wind's eye" of the place where I was standing behind the wall, she turned abruptly at a right angle, threw up her head, and came as straight as an arrow to the spot where I was watching her. Yet while watching her I had allowed only my eyes to come above the wall, so that she proved herself able to distinguish instantly the odour of the top of my head (without hat) at a distance of two hundred yards, although at the time she was surrounded by a number of over-heated labourers.

(16) On another day, when it was perfectly calm, I tried the experiment of standing in a deep dry ditch, with only the top of my uncovered head above the level of the surrounding fields. When she was led within two hundred yards of the place, she instantly perceived my odour, and ran in a straight line to where I had then ducked my head, so that she should receive no assistance from her sense of sight. This experiment shows that, in the absence of wind, the odour of my head (and no doubt, in a lesser degree, that of my body) had diffused itself through the air in all directions, and in an amount sufficient to enable the setter to recognize it as my odour at a distance of two hundred yards.

From the above experiments I conclude that this bitch distinguishes my trail from that of all others by the peculiar smell of my boots (1 to 6), and not by the peculiar smell of my feet (8 to 11). No doubt the smell which she recognizes as belonging distinctively to my trail is communicated to the boots by the exudations from my feet; but these exudations require to be combined with shoe-leather before they are recognized by her. Probably, however, if I had always been accustomed to shoot without boots or stockings, she would have learnt to associate with me a trail made by my bare feet. The experiments further show that although a few square millimetres of the surface of one boot is amply sufficient to make a trail which the animal can recognize as mine, the scent is not able to penetrate a single layer of brown paper (10). Furthermore, it would appear that in following a trail this bitch is ready at any moment to be guided by inference as well as perception, but that the act of inference is instantaneous (12 and 13 as compared with 2, 8, and 11). Lastly, the experiments show that not only the feet (as these affect the boots), but likewise the whole body of a man exhales a peculiar or individual odour which a dog can recognize as that of his master amid a crowd of other persons (15); that the individual quality of this odour can be recognized at great distances to windward (15), or, in calm weather, at great distances in any direction (16); and that it does not admit of being overcome by the strong smell of aniseed (14), or by that of many other footprints (4).

FOSSIL WOOD FROM THE WESTERN TERRITORIES OF CANADA.[1]

SILICIFIED wood occurs in the country west of Manitoba in the Upper Cretaceous beds, in the Laramie and in the Miocene of the Cypress Hills, and has found its way into the drift. The numerous specimens in our collections, picked up on the plains, are thus of little palæontological value, as their sources are uncertain, and it has become desirable to obtain specimens found *in situ*. A small collection of this kind was made by Dr. G. M. Dawson in the course of the Boundary Survey, and was described in the Report on the 49th Parallel, in 1875. In 1880, Schrœter, in an appendix to Heer's paper on the plants of Mackenzie River, described a few species from the Laramie of that district. More recently, numerous specimens have been collected from beds of known geological age by Dr. G. M. Dawson, Mr. J. B. Tyrrell, and Mr. T. C. Weston, of the Geological Survey, and slices have been prepared by the latter. They include species from the Belly River and Fort Pierre groups, which are Upper Cretaceous; from the Lower Laramie, apparently a transition group between the Cretaceous and Eocene; and from the Upper Laramie, which is probably Lower Eocene, though at one time regarded as Miocene. These woods are mostly coniferous, but there are also angiospermous exogens of several kinds. In describing them in detail, they are not named as species, but merely referred to the modern genera which they most closely resemble. We thus find in the Belly River series two types of *Sequoia* corresponding to the wood of the two modern species, and woods of the types of *Taxus Salisburia* or *Ginkgo*, *Thuja*, and possibly *Abies*, along with exogens referable conjecturally to the genera *Betula*, *Populus*, *Carya*, *Ulmus*, and *Platanus*. In the Laramie we have a similar assemblage of conifers and exogens, with forms referable to *Pinus* and *Abies*, and to *Juglans* and *Acer* among the exogens.

[1] Abstract of a Paper by Sir William Dawson, read before the Royal Society of Canada, May 1887.

Some fruits and other fragments from the Belly River series appear to indicate the presence of a species of *Podocarpus*. Appended to the descriptions of the woods are notices of new species and localities in connexion with the Laramie flora, and remarks on the grand coniferous fruits of the period, as connected with the formation of coal and lignite. The concluding remarks are given in full, as of interest in connexion with the British Eocene flora:—

Concluding Remarks.—While studying the specimens described in this paper, I received the volume of the Palæontographical Society for 1885, containing the conclusion of Mr. Starkie Gardner's description of the Eocene Coniferæ of England. The work which he has been able to do in disentangling the nomenclature of these plants, and fixing their geological age, is of the greatest value, and shows how liable the palæobotanist is to fall into error in determining species from imperfect specimens. Our American species no doubt require some revision in this respect.

I have also, while writing out the above notes for publication, received the paper of the same author on the Eocene beds of Ardtun in Mull, and am fully confirmed thereby in the opinion derived from the papers of the Duke of Argyll and the late Prof. E. Forbes (Journ. Geol. Soc. of London, vol. vii.), that the Mull beds very closely correspond in age with our Laramie. The *Filicites hebridica* of Forbes is our *Onoclea sensibilis*. The species of *Gingko*, *Taxus*, *Sequoia*, and *Glyptostrobus* correspond, and we have now probably found a *Podocarpus* as noted above. The *Platanites hebridicus* is very near to our great *Platanus nobilis*. *Corylus Macquarii* is common to both formations, as well as *Populus arctica* and *P. Richardsoni*, while many of the other exogens are generically the same, and very closely allied. These Ardtun beds are regarded by Mr. Gardner as Lower Eocene, or a little older than the Gelinden series of Saporta, and nearly of the same age with the so-called Miocene of Atanekerdluk in Greenland. I have ever since 1875 maintained the Lower Eocene age of our Laramie, and of the Fort Union group of the North-Western United States, and the identity of their flora with that of Mackenzie River and Greenland, and it is very satisfactory to find that Mr. Gardner has independently arrived at similar conclusions with respect to the Eocene of Great Britain.

An important consequence arising from this is that the period of warm climate which enabled a temperate flora to exist in Greenland was that of the later Cretaceous and early Eocene, rather than, as usually stated, the Miocene. It is also a question admitting of discussion, whether the Eocene flora of latitudes so different as those of Greenland, Mackenzie River, North-West Canada, and the Western States, were strictly contemporaneous, or successive within a long geological period in which climatal changes were gradually proceeding. The latter statement must apply at least to the beginning and close of the period; but the plants themselves have something to say in favour of contemporaneity. The flora of the Laramie is not a tropical but a temperate flora, showing no doubt that a much more equable climate prevailed in the more northern parts of America than at present. But this equability of climate implies the possibility of a great geographical range on the part of plants. Thus it is quite possible, and indeed highly probable, that in the Laramie age a somewhat uniform flora extended from the Arctic seas through the great central plateau of America far to the south, and in like manner along the western coast of Europe. It is also to be observed that, as Gardner points out, there are some differences indicating a diversity of climate between Greenland and England, and even between Scotland and Ireland and the south of England; and we have similar differences, though not strongly marked, between the Laramie of Northern Canada and that of the United States. When all our beds of this age, from the Arctic Sea to the 49th parallel, have been ransacked for plants, and when the palæobotanists of the United States shall have succeeded in unravelling the confusion which now exists between their Laramie and the Middle Tertiary, the geologist of the future will be able to restore with much certainty the distribution of the vast forests which in the early Eocene covered the now bare plains of interior America. Further, since the break which in Western Europe separates the flora of the Cretaceous from that of the Eocene does not exist in America, it will then be possible to trace the succession of plants all the way from the Mesozoic Flora of the Queen Charlotte Islands and the Kootanie series, described in previous papers in these Transactions, up to the close of the Eocene; and to determine, for America at least, the manner and conditions under which the angiospermous flora of the later Cretaceous succeeded to the pines and cycads which characterized the beginning of the Cretaceous period.

THE LIVERPOOL MARINE BIOLOGY STATION ON PUFFIN ISLAND.

THE Liverpool Marine Biology Committee was formed in the spring of 1885 for the purpose of working up thoroughly the fauna and flora of that large rectangular area of the Irish Sea which lies around Liverpool Bay, and is bounded by the Isle of Man and the coasts of Anglesey, North Wales, Cheshire, and Lancashire. During the last three seasons the members of the Committee have conducted a large number of dredging, tow-netting, and other investigating expeditions in various parts of the Liverpool Marine Biology Committee district, and, as a first result of their labours, they published, in the summer of 1886, a "First Report upon the Fauna of Liverpool Bay and the Neighbouring Seas." It became evident at an early stage in these investigations that, as the sand-banks and channels in the immediate neighbourhood of the estuary of the Mersey are comparatively barren, it would be

Biological Station. Old Tower.

FIG. 1.—Puffin Island from the north.

necessary, in order to carry on the work of the Committee satisfactorily, to establish a small marine laboratory somewhere on the coast of North Wales or Anglesey. Such a station, close to the region where there is a rich and varied fauna, and yet within easy reach of Liverpool, would enable the members of the Committee, and other biologists who were working with them, to pay frequent and regular visits to the best ground for the purpose of collecting specimens; and also to carry on observations on the habits of the animals, and to investigate their structures and life-histories. The Liverpool Marine Biology Committee have been aided in their work by small grants this year and last year from the Government Grant Committee of the Royal Society, and have received most important and generous assistance, by the loan of steamers for the dredging expeditions and in other ways, from some of the Liverpool ship-owners—amongst others, from the present Mayor, Sir James Poole, from Mr.

George Holt, and from the Liverpool Salvage Association—and now they owe the attainment of their desire for a marine laboratory to the kindness of Sir Richard Williams Bulkeley, Bart., of Beaumaris, in allowing them to make use, for scientific purposes, of the former Dock Board Telegraph Station on Puffin Island (Fig. 1).

Puffin Island, or Priestholme, is a small uninhabited island close to the north-east corner of Anglesey, and lying with its longer axis north-east and south-west. It is composed mainly of beds of limestone, and has precipitous sides, which have been worn into caves, crevices, and innumerable pools. The best landing-place is on the end nearest to Anglesey, where there is a beach of shingle. The shores all round the island support an abundant fauna, and some of the best dredging-grounds in the Liverpool Bay district lie close to Puffin Island, and a little further to the west along the coast of Anglesey. A glance at the accompanying chart will show the diversity in the depth of water off the north and east ends of the island (Fig. 2).

The house which the Liverpool Marine Biology Committee have now taken possession of as a centre for their further operations was built by the Liverpool Dock Board, and used as a signalling station, but has been uninhabited for some years. It contains four good rooms, besides lofts and out-houses, and a long observatory

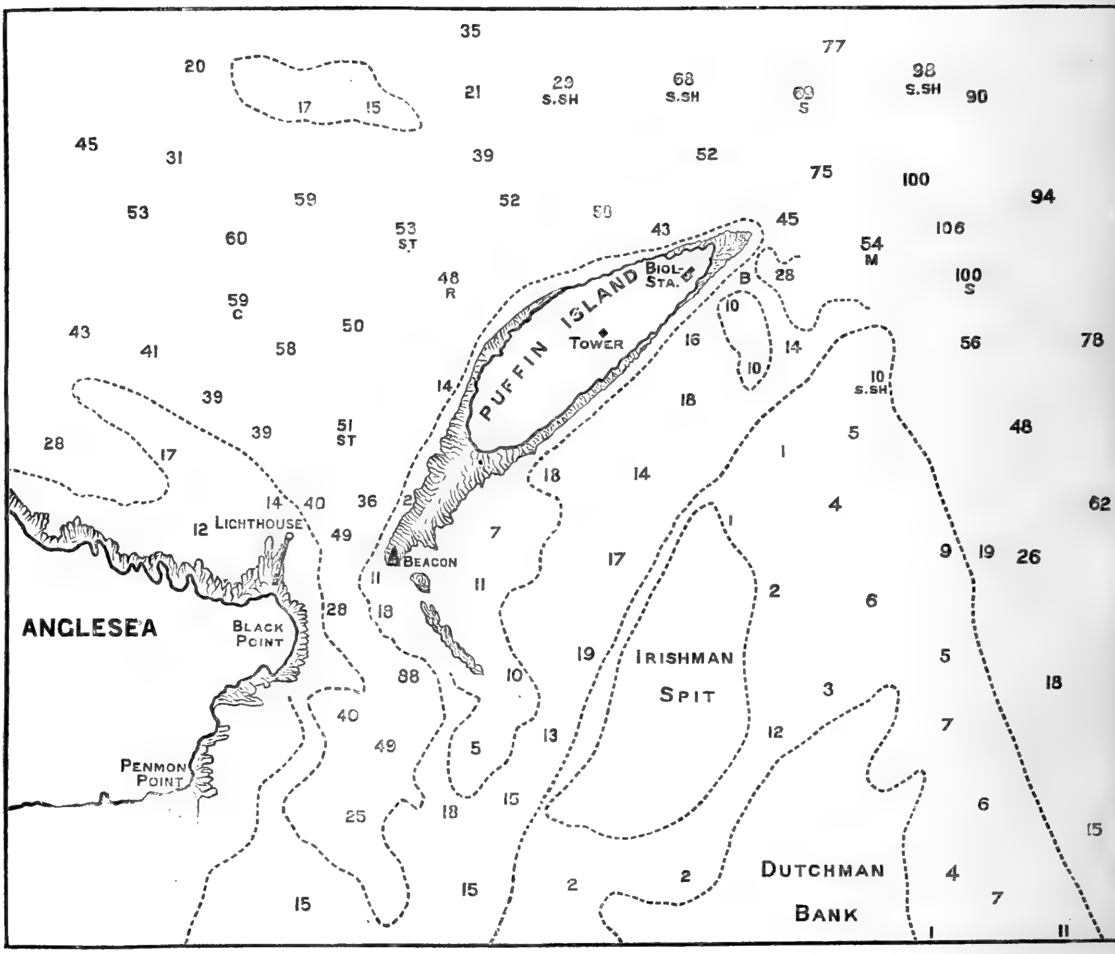


Fig. 2.

running towards the sea (north-east), and lighted by a series of seven windows round the outer end (Fig. 3). This observatory will make a well-lighted, convenient laboratory, while the other four rooms serve as kitchen and sleeping rooms for the naturalists and the keeper of the station.

At the end of May the new doors and windows, shutters, tables, and other fittings, which had been prepared in Liverpool, were ready for transference to the station, and a number of the Liverpool Marine Biology Committee, along with some workmen, were taken down to Puffin Island by the ss. *Hyæna*, which had been lent for the purpose by the Liverpool Salvage Association. The house was rapidly made weather-tight and put in working order, and is now under the charge of a keeper and his assistant. Tanks will soon be erected, and some of the shore-pools are being converted into natural aquaria. A small sailing-boat has been obtained, by which dredging and tow-netting in the neighbourhood of the island can be carried on, and by means of which communication can be kept up with the Liverpool steamers at Beaumaris and the railway at Bangor.

Since the establishment of the station some of the members of the Liverpool Marine Biology Committee have already had half a dozen expeditions to the island, and the following naturalists have commenced work on their respective groups of animals: Mr. I. C. Thompson, on the Copepoda; Mr. J. Lomas, on the Polyzoa; and Prof. Herdman, on the Tunicata. Various other scientific men have come as visitors to see the station, including: Prof. Lodge, F.R.S., Prof. Hele Shaw, Mr. Reginald Phillips, of Bangor, Mr. I. Roberts,

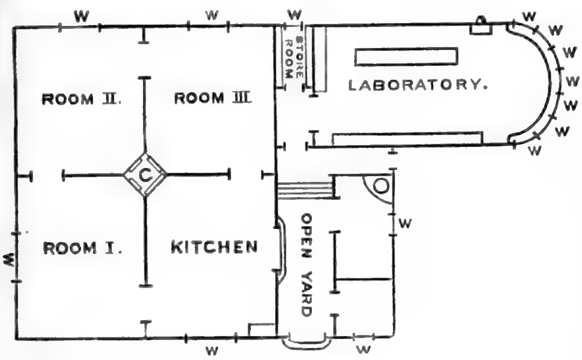


FIG. 3.—Plan of Liverpool Marine Biological Station on Puffin Island. w, windows; c, chimneys.

and Mr. Mellard Reade; and it is hoped that if the weather is favourable on Sept. 3, the biologists taking part in the British Association dredging expedition, arranged by the Liverpool Marine Biology Committee, will have an opportunity of visiting Puffin Island and its Biological Station.

W. A. HERDMAN.

ANTARCTIC EXPLORATION.

IN June 1886, an Australian Antarctic Exploration Committee was appointed at Melbourne. It consisted of five members each from the Royal Society of Victoria and the Royal Geographical Society, Victoria Branch. This Committee has collected a quantity of information respecting the islands lying south of Tasmania as well as respecting lands lying nearer the Pole.

The prospect of obtaining a Government grant for an expedition having scientific purposes only, though the preferable course, was thought to be hopeless. The Committee therefore has recommended the Government of Victoria (which had expressed itself favourably to the project) to offer to steam-whalers, for carrying a scientific staff to certain high latitudes, bonuses, graduated to degrees of southing. We print the conditions, but no tenders can be invited till a grant is assured, and the Government of Victoria is indisposed to act in the matter without other colonies, whose co-operation is doubtful, though Tasmania will most probably offer a small contribution.

Many offers of steam-whalers have been sent from England, Scotland, and Norway, where the owners seem anxious to dispose of their ships and gear. Most important and valuable information and advice have been received from Capt. Gray, of Peterhead.

The following are the recommendations of the Antarctic Committee to the Honourable the Premier of Victoria:—

(1) The Antarctic Committee begs respectfully to recommend to the Honourable the Premier the propriety of stimulating Antarctic research by the offer of bonuses.

(2) That a sum of £10,000 be placed upon the Estimates, to provide for the amount of the bonuses, and for the expenses of the equipment and of the staff.

(3) The amount of the bonuses to be paid to the shipowners for the hereinafter mentioned services is to be decided by tender, and the same, together with the cost of equipment and the staff, not to exceed the sum of £10,000.

(4) That the Government invite tenders from shipowners willing to perform the services required.

(5) That the tenders be sent to the Treasury direct, or through the Agent-General, not later than June 1.

(6) That tenderers must provide two fortified steamships, each of not less than 175 tons register, 60 horsepower nominal, and A1 at Lloyd's, or of an equivalent class.

(7) That tenderers must supply full descriptions of the ships and their equipments.

(8) That the master and chief mate of both ships shall have held similar positions in Arctic steam-ships.

(9) That the tenderer shall provide, free of charge, cabin accommodation in each ship for two gentlemen, who will sail as the scientific staff; also a separate cabin, of a size to be specified, as instrument-room and office.

(10) The scientific staff will have the status of cabin passengers, and be subordinate to the master, but the master must afford them every facility, that does not interfere with the work or safety of the ship, for noting natural phenomena.

(11) The chartered ships will earn a special bonus (to come out of the £10,000 appropriated) upon their entering at the Custom House a cargo of 100 tons of oil, being the produce of fish caught south of 60° S. The special bonus to be paid as follows, viz.:—To ships owned and registered in Australia, £1,000; to ships owned and registered elsewhere, £800.

(12) The services desired are as follows, viz.:—A flying survey of any coast-lines lying within the Antarctic Circle, and not now laid down upon the Admiralty charts. The discovery of new waterways leading towards the South Pole, and of harbours suitable for wintering in. Opportunities to be afforded to the scientific staff to add to our knowledge of the meteorology, oceanography, terrestrial magnetism, natural history, and geology of the region. The discovery of commercial products.

(13) The tenderer must specify the bonus he demands for passing 70° S. with either one or two ships; also the bonus he demands for each degree attained beyond 70° S. by one ship; also the bonus he demands for every occasion upon which he succeeds in establishing on the shore a temporary observing camp.

(14) That the Government should pay for only one such station for each 120 miles of latitude or longitude, unless the master shall have established more at the written request of both members of the staff.

(15) The staff to have the right to refuse to accept the site of any camp selected by the master, and such refusal shall be logged by the master, and read over to the staff in the presence of the mate and the surgeon; and the staff shall hand to the master their objections thereto in writing, and the same must be signed by both of them.

(16) The tenderer will not receive any more bonus for two ships than for one after passing the 70th parallel. The Committee would prefer that one of the ships should remain fishing in the neighbourhood of North Cape, Victoria Land, whilst the other pushed into higher latitudes. In case of accident to the latter, the former would serve as a depot and relief for the shipwrecked crew to fall back upon.

(17) Should the master of either ship despatch an exploring party from his vessel, the contractor will be entitled to a bonus for each sixty miles of latitude or longitude traversed by such party, but the tenderer must specify what sum he will require for each sixty miles so traversed.

(18) That the ships should proceed direct to the bight situated on the meridian of 180°, with a view of one of them getting beyond Ross's furthest, and especially of observing the conditions of the volcanoes at the head of the bight.

(19) The contractor will be liable to no penalty should he fail to reach to any latitude tendered for.

(20) The contractor will have the right to employ his ships in whaling or sealing, and in loading guano or other cargo.

(21) Should the masters be unable to get right or sperm whales to enable them to compete for the bonus offered under the 12th proviso, they will nevertheless be entitled to the bonus should they return with a cargo of any merchantable commodity obtained within the Antarctic Circle, and having a value equivalent to that of 100 tons of whale oil.

(22) Both ships must be in Port Phillip Bay and ready to start on October 15.

(23) That in case of any difficulty arising in England between the Agent-General and the contractor, it shall be referred to the British Antarctic Committee for decision.

THE CAPTIVE KITE-BALLOON.

IT has always been an objection to the extensive use of captive balloons for scientific or military purposes, that a wind of moderate strength suffices not merely to depress them considerably from the vertical, but to cause them to jerk, rotate, and oscillate vertically and horizontally in such a manner as to render them either partially ineffective or totally useless.

During the recent military manœuvres at Dover, it was stated that the captive balloon under the charge of Major Templer was not allowed to ascend beyond the shelter of the surrounding downs, owing to the strong wind then prevailing. It was thus *hors de combat* as far as the enemy was concerned, and this seems to be a common experience of military balloonists.

The jerking, as a balloon after a freshening of the wind suddenly reaches the end of its tether, is, I am told by an experienced member of the Balloon Corps, very trying to the nerves, while the rotation on its axis is a serious obstacle to steady observation.

The depression of a captive balloon in a wind of any sensible strength is also more than most persons would imagine, and as the velocity of the wind generally increases with the height (very rapidly for the first few hundred feet), while the buoyancy of the balloon, owing to several causes, diminishes, this condition becomes more pronounced at the higher levels.

The depression is obviously due to the fact that a captive balloon, as at present employed, can only be secured at its *base*, and thus the normal component of the wind is resolved in a downward direction, pressing the balloon towards the earth. If the fastening could be made two-thirds of the way up its side, this normal component could be resolved in an upward direction, and utilised so as to add to the elevating power of the balloon. The fragile nature of the balloon fabric, however, renders it impossible to do this except by interposing a kite-surface between it and the wind.

All the preceding defects are remedied and several positive advantages are gained by attaching a balloon to a kite in the manner indicated in the accompanying diagram.

(1) The addition of the kite with the fastening at the side instead of the base counteracts the depression produced by the wind, and not only raises its own weight, but even in a light anticyclonic breeze elevates the whole apparatus to a higher level than that which could be attained by the balloon alone.

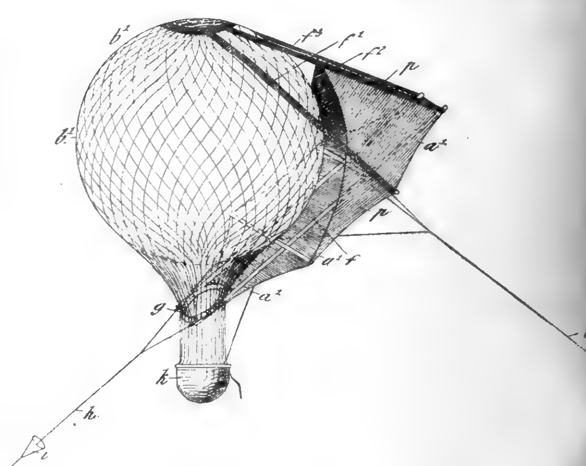
Thus, in an experiment here on Friday, June 10, in the presence of Mr. Eric S. Bruce and others, with a very light wind,[1] the balloon of 113 cubic feet capacity and with 1200 feet of wire out attained *alone* a mean vertical height of 693 feet, while when attached to a kite of 9 feet by 7 feet and the same length of wire it kept steadily at 789 feet. The lifting power in the second case was also greatly increased, as shown by the following comparison of the angles of the kite and wire in the two cases[1]:—

	Angle of Balloon.		Angle of Wire near the ground.
Balloon alone	38°	...	18°
Balloon with kite	$41\frac{1}{2}$°	...	35°

The addition of the kite raised $1\frac{1}{4}$ lbs. more than the balloon could have done alone, with a good deal to spare. It increased the height by 96 feet and diminished the sag by $13\frac{1}{2}$°.

(2) With the tail (made of self-regulating cones) it completely counteracts the jerky, rotatory, and oscillatory movement of the balloon, by keeping the wire taut and exerting a constant pull on the balloon at its lower extremity.

(3) With the addition of the *top hood*, an essential feature of the combination, the kite shields the balloon fabric from the destructive action of the wind.



Archibald's Captive Kite-Balloon. a^1, octagonal kite, with frame of four pieces of bamboo; b^1, spherical balloon; *f*, covering of kite (preferably silk); *p*, extra or top hood; $f^1 f^2$, &c., bands connecting kite and hood with top of balloon; *g*, ring connecting lower end of kite with the converging net cords of balloon; *h*, tail of cones (*i*); l^1, earth-line connected with kite, one branch passing through a pulley to the car (*k*).

(4) The combination can be flown on a much larger percentage of days than the balloon alone.

(5) In a large balloon with car attached the occupant can alter his altitude and azimuth by pulling the lower or side attachments of the kite, and thus extend his area of observation.

(6) With the kite, and except in the rare case of a dead calm, a much smaller balloon is needed to raise a given weight.[2]

(7) The use of wire (a suggestion which I owe to Sir William Thomson) greatly increases the strength, and lessens the weight, of the earth-line.

I arrived at the idea of uniting the two apparatuses while conducting my kite anemometrical observations in 1884, owing to my desire to prevent my kites from coming down suddenly when the wind dropped. I found the balloonists equally desirous of some means for shielding their balloons from damage and keeping them *up* in a *wind*. The kite-balloon satisfies both requirements, and will, I trust, be of use both to scientific as well as military observation.

E. DOUGLAS ARCHIBALD.

Tunbridge Wells, June 25.

[1] I have since ascertained that during the trial the mean velocity at Greenwich [211 feet above the sea with a good exposure for the wind (N.E.)] was 12 miles per hour. The present locality was in a valley 260 feet above sea-level, surrounded by hills rising to 500 feet above the sea.

[1] The lifting power of the balloon with hydrogen was about 5 lbs., the wire weighed about 4 lbs. and the kite $2\frac{1}{4}$ lbs.

[2] The kite portion is portable and easily detachable in the event of a calm.

NOTES.

A BILL dealing with the question of technical education was submitted to the House of Commons on Tuesday, and read a first time. We print elsewhere the speech delivered by Sir W. Hart Dyke in introducing the measure.

THE Report for 1886 of the Science and Art Department has just been issued.

THE summer meeting of the Institution of Mechanical Engineers will be held in Edinburgh, on Tuesday morning, August 2, and Wednesday morning, August 3, in the University. The chair will be taken at half-past nine o'clock by the President, Mr. Edward H. Carbutt, in the Natural History Lecture Theatre. The following papers have been offered for reading and discussion, not necessarily in the order here given :—On the structure and progress of the Forth Bridge, by Mr. E. Malcolm Wood, of London; notes on the machinery employed at the Forth Bridge works, by Mr. William Arrol, of Glasgow; on the paraffin oil industry in Scotland, by Mr. St. John V. Day, Honorary Local Secretary; description of the electric light on the Isle of May, by Mr. David A. Stevenson, of Edinburgh; description of the new Tay Viaduct, by Mr. Fletcher F. S. Kelsey, Resident Engineer; on electro-magnetic machine-tools, by Mr. Frederick John Rowan, of Glasgow; on the dredging of the lower estuary of the Clyde, by Mr. Charles A. Stevenson, of Edinburgh; on the position and prospects of electricity as applied to engineering, by Mr. William Geipel, of Edinburgh. Various excursions are being arranged, and it is desired that members who propose to be present, and to accept the several invitations, should let their intention be known without delay.

THE summer meetings of the Institution of Naval Architects will be held at Newcastle-on-Tyne on July 26 and 28, and at Sunderland on July 27. The following papers will be read at Newcastle: on the application of hydraulic pressure to naval gunnery, by the Right Hon. Lord Armstrong, F.R.S. Vice-President, and Mr. J. Vavasseur, Associate; recent developments in marine engineering, by Mr. Frank C. Marshall, Member of Council; Tyne improvements, by Mr. P. J. Messent, Engineer to River Tyne Commissioners. At Sunderland the following papers will be read: on some recent experiments with basic steel, by Mr. W. H. White, Director of Naval Construction, Vice-President; on the present position occupied by basic steel for ship-building, by Mr. B. Martell, Chief Surveyor to Lloyd's Register of British and Foreign Shipping, Vice-President. There will be excursions to places of scientific interest in the neighbourhoods.

MR. THOMAS HUDSON BEARE has been unanimously appointed by the Governors of the Heriot-Watt College, Edinburgh, Professor of Mechanics and Engineering in that institution. Mr. Beare came over to this country from Australia in 1880, having gained the South Australian Scholarship at the University of Adelaide. He then became a student at University College, London, to which he afterwards returned about three years since to be one of the principal demonstrators under Prof. Kennedy in the Engineering Laboratory.

THE Geographical Society of St. Petersburg has decided to send an Expedition to Turkestan for the scientific investigation of the earthquake at Werny. Prof. Muschketoff, the head of the Expedition, will be accompanied by five other men of science, including the St. Petersburg geologist, M. W. S. Dmitrewski.

THE seventh Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, contains a careful and interesting paper on annatto, a colouring substance obtained from the seeds of *Bixa Orellana*. This colouring substance has long been known and used for various purposes. It is, however, liable to so many fluctuations, and the prices generally are so low, that it has never received serious attention in British colonies, and hence few, if any, plantations have been exclusively devoted in such colonies to the annatto plant. The annatto of commerce is practically a forest product obtained from wild or semi-wild plants, and the supply has only kept pace with the demand. Of late years a slight revival has taken place in the use of annatto, especially in America, and inquiries have in consequence been made for information as regards culture and preparation. This information the writer of the paper in the Bulletin supplies, and his notes will be of great service to all who may wish to become growers of annatto.

ON May 9 the Governor of Jamaica addressed to the Governors of Barbados, the Leeward Islands, the Windward Islands, and British Honduras, a letter relating to the scheme for the establishment of botanical stations in some West India Islands in connexion with the Botanical Department in Jamaica. From this letter, which is printed in the seventh Kew Bulletin of Miscellaneous Information, we are glad to learn that the Government of Jamaica is prepared to adopt the proposed scheme from August 1, or from any subsequent date.

BEFORE the end of the year the great Tweeddale collection and library will, it is hoped, be safely housed in the Natural History Museum. This princely donation to the national collection is the gift of Capt. R. G. Wardlaw Ramsay, to whom it was bequeathed by his uncle, the late Marquess of Tweeddale. With the exception of the Hume Collection it is the finest series of Indian birds in existence, and is especially rich in species from the Philippine Archipelago, where Mr. Alfred Everett collected for some years for Lord Tweeddale. Capt. Ramsay's collections from the Karen Hills, in Burmah, are also most important, this being one of the few localities unworked by Mr. Hume's collectors.

THE American Museum of Natural History, New York, has just acquired the ornithological library of Mr. D. G. Elliot, a well-known American naturalist. This library consists of about 1000 volumes, and is one of the most important in America. Mr. Elliot has at the same time presented his collection of humming-birds to the above Museum. It is, according to the *Auk*, "represented by about 2000 specimens, and includes some fifty or more types. Its importance is further enhanced from its having formed the basis of Mr. Elliot's recent monograph of the family. It doubtless ranks as second in the world in point of completeness, or next to that of the British Museum." The latter collection, however, must now contain at least 10,000 skins, irrespective of the Gouldian series of mounted specimens. Another important addition to the American Museum is that of the large ornithological collection of Mr. G. N. Lawrence, which contains some 300 types. This has been purchased, and is one of the chief of the private collections in America.

THE special groups, illustrating the nesting habits of British birds, which have proved so attractive in the Natural History Museum at South Kensington, have now been introduced into the galleries of the American Museum of Natural History, and twelve cases of American birds have already been mounted. The cost of these effective, but expensive, groups will be defrayed by Mrs. Robert E. Stuart, and the Museum has secured the services of Mrs. Mogridge, who executed the artificial flower-work for the British Museum. Mrs. Mogridge is without a rival in this branch of decorative art.

THE expedition made by Mr. John Whitehead to the great mountain of Kina Balu, in Northern Borneo, has turned out more successful than could have been foretold. In addition to the splendid new Broadbill, described by Mr. Bowdler Sharpe as *Calyptomena whiteheadi* at the last meeting of the Zoological Society, there are nearly twenty other new species, including some very remarkable forms of *Arachnothera*, *Chloropsis*, *Cryptolopha*, and an apparently new genus of *Campophagidæ*. These will all be described in the October *Ibis*, by Mr. Sharpe.

DEALING with the question as to the influence of small birds in assisting the extinction of *Aporia cratægi*, Mr. A. G. Butler, in the current number of the *Entomologist's Monthly Magazine*, says he has collected in Kent for at least thirty years, and during the whole of that time he has never seen any bird but a sparrow attempt to catch a butterfly. Nor has he ever known a small bird to eat a large caterpillar if it could get one that could be more easily swallowed. "Of our indigenous species," he says, "the robin and the great tit certainly select green caterpillars in preference to others, and, when feeding their young, I have watched both these birds with their mouths full of the green pests of the gooseberry and currant. From observation of cage-birds I should say that the finches certainly show a similar preference, the green larvæ of *Mamestra* being chosen before the brown, though all are greedily devoured."

A BOLD attempt has recently been made to penetrate the darkness surrounding the subject of the inter-molecular arrangement of atoms, and to raise our ideas of the constitution of chemical compounds beyond what is expressed by the orthodox chemical formulæ. It has long been felt that the chemical formula of a substance as expressed in one plane on paper, although invaluable as far as it goes, must of necessity be a very misleading one, inasmuch as it in no way indicates the probable position of the various atoms in space. This insufficiency has been especially felt in the case of substances like tartaric acid, where we have several distinct isomers acting quite differently upon polarized light, and frequently forming right- and left-handed hemihedral crystals, although expressed by the same constitutional formula. Since the year 1874, when Van t' Hoff and Le Bel published their celebrated theory of the "asymmetric carbon atom," the idea has been gaining ground that this kind of isomerism must be due to different spacial arrangement, and Van t' Hoff gave impetus to the theory by showing that the existence of four isomeric tartaric acids could be explained by imagining the four radical-groups to be variously placed at the four corners of a regular tetrahedron, of which an asymmetric carbon atom occupied the centre. During the last few days a comprehensive paper has been issued by Prof. J. Wislicenus, on the "Spacial Arrangement of Atoms in Organic Molecules, and its Determination in Geometrically Isomeric Compounds," further expanding Dr. Van t' Hoff's somewhat sceptically received ideas, proceeding to build up the spacial constitution of a large number of unsaturated organic compounds, and giving nearly 200 figures, of which the regular tetrahedron representing CH_4 is the base. Prof. Wislicenus practically demonstrates that the cases of so-called abnormal isomerism may be completely cleared up, and that existing experimental data are generally sufficient to enable spacial constitution to be determined. This remarkable paper will doubtless give rise to much discussion, and appears likely to lead to results which will mark a genuine advance in chemical philosophy.

IN a paper to the Berlin Academy, Herr Liebreich lately called attention to what he calls the "dead space" in chemical reactions: a space, *i.e.*, in which the reaction going on in other parts of a uniformly mixed liquid does not occur, or occurs late, or in less degree. It may be very well observed, *e.g.*, in decomposition of chloral hydrate by sodium carbonate (yielding chloroform) in a test-tube. A layer of 1 to 3 millimetres' depth under the surface remains clear, and separate by a convex surface from the "reaction space," where the solution is turbid from droplets of chloroform. Even after twenty-four hours' rest of the mixture the two spaces can be distinguished; and after mixture by shaking again, the surface of separation is reproduced in a few minutes. In horizontal capillary tubes the dead space appears on both sides, and, if the liquid columns introduced are short, no reaction occurs, as the dead spaces unite. Thus is explained the absence of reaction in the case of liquid absorbed in vessels by glass pearls; there is dead space everywhere. Contact with air seems essential to the formation of dead space. Thus if a vessel closed at one end, and holding the liquids above named, be inverted, so that there is no air space above the mixture, the reaction occurs uniformly throughout. But if the upper end be closed with a fine animal membrane, the dead space appears as in the former case; and if the lower end be also closed with fine membrane the dead space appears there too. Herr Liebreich is studying the phenomena further.

SOME instructive experiments on atmospheric electricity are described by Herr Nahrwold in *Wiedemann's Annalen*, No. 7; one being a suitable lecture experiment, showing the action of electricity from points on finely divided matter in the air. He thinks it established that such a stream of electricity does not electrify the air itself statically (indeed that air and other gases probably cannot be statically electrified), but only dust particles in it. Further, a glowing platinum wire sends out particles which diffuse in air that has been electrically freed from dust, making a fresh charge possible. Here, too, the electricity streaming from such wire does not statically electrify the air, but the charges which are observed as atmospheric electricity belong to fine non-gaseous particles given out by the wire, or already present in the air. An experiment is also adduced to show that at ordinary temperature negative electricity of high potential streams more readily from solid conductors into atmospheric air than positive.

ELECTRICITY in the house has some important bearings on hygiene. One of these M. Sambuc has recently called attention to (*Revue d'Hygiène*), in the liberation of hydrogen, where strong batteries are used in which zinc is dissolved by sulphuric acid. Besides the danger of shattering of the vessels, the hydrogen spreading in the air may form an explosive mixture; and it may have a cooling effect through its great conductivity for heat. It also deadens the voice and alters its timbre. Further, if, as may be, the hydrogen is charged with sulphur, arsenic, phosphorus, carbon, or silicium, there are other and greater dangers. A chemist is known to have died from breathing a little arseniетted hydrogen. These facts are not cited against the use of the electric light, but to induce proper care in those who use it.

THE twenty-sixth volume of the magnetical and meteorological observations made at the Government Observatory, Bombay, containing the results for the year 1885, has just been published, under the superintendence of Mr. C. Chambers, F.R.S. Continuous registrations are obtained by means of self-recording instruments (although not published), and eye-observations are taken five times a day, as a check upon the automatic records. The following is a summary of the principal meteorological results:—The mean barometric pressure for the year was 29·826 inches, the difference of the greatest and least mean daily pressure amounting to 0·581 inch. The mean annual temperature was 79°·2, and the greatest daily mean was 87°·3 on June 6. The absolute maximum was 91°·8 in June (being slightly *lower* than the maximum in the shade at Greenwich on the 4th inst.), and the minimum 62°·1 in February, giving a range of 29°·7. The rainfall measured by a gauge 4½ feet above the ground was 67·91 inches; rain fell on 113 days, and mostly occurred between June and September; the greatest fall was 10·29 inches on August 15.

The Observatory does not appear to possess a sunshine-recorder. Observations for a few selected hours for Bombay and five other stations for the years 1885–86 have already been published separately by the Indian Meteorological Office.

METEOROLOGICAL observations have been regularly made at the Khedivial Observatory at Cairo (Abassieh) during the past five years, and have been published in various forms. The publication has now assumed a more definite shape, under the title, *Résumé Mensuel*, and is issued by the Ministry of Public Instruction. The observations are taken every three hours during the day and night. Yearly summaries are not given, but we find from the monthly values that the mean shade temperature for the year 1886 was 69°·6. The absolute maximum in the shade was 113°·4 in June, and the minimum, 36°·7, in December; giving a yearly range of 76°·7. The thermometers are placed much too high, being about 33 feet above the ground, instead of about 4 feet. The amount of rainfall is not regularly published.

THE Jahrbuch of the *Magdeburgische Zeitung* for the year 1885 (Magdeburg, 1887, 88 pp. 4to) contains, in addition to the usual observations and reproductions of the continuous registrations of barometer and sunshine-recorder, a table showing the extremes of temperature on the surface of the earth observed by means of five maximum and five minimum thermometers, one pair lying flat and the other four pairs being inclined about 45° under the four principal points of the compass, between May 1885 and April 1886; but there is no discussion of the results. There is also an interesting appendix relating to the choice of hours that will give the nearest approach to the mean daily temperature. The author has used the continuous records for Berne, Vienna, Magdeburg, Pawlowsk (near St. Petersburg), and Upsala for a year, and has found the following to be the mean values of the corrections to be applied to the various yearly means:—For 8h., 2h., 8h., 0°·040; for 7h., 2h., 9h., −0°·092; for 6h., 2h., 10h., 0°·104; and for max. and min. 0°·084. The best combination according to this investigation is therefore 8h., 2h., 8h., whereas in this country 9h. a.m. and 9h. p.m. are found to give a good mean. The combination of max. and min. also gives a fairly approximate value for mean latitudes. The author has also investigated the epoch of the maximum and minimum temperature for the same places, and shows how the highest daily temperature occurs later as the summer advances, being at about 3h. p.m. in June and July and between 12h. and 1h. in January and December; and further, that the lowest temperature does not always take place at about sunrise, as is generally supposed, but only during summer, while in winter the minimum is near midnight. The present Director of the Observatory is A. Grützmacher; the former Director, Dr. Assmann, having been appointed to the Meteorological Office at Berlin.

MR. A. L. ROTCH has published the results of the observations made at the Blue Hill Meteorological Observatory, Norfolk County, Massachusetts, U.S., in the year 1886 (NATURE, vol. xxxv. p. 472). This Observatory, which was established by Mr. Rotch in 1885, is now one of the best-equipped stations in the United States, and the current expenses amount to about 2500 dollars a year. An auxiliary station has also been established at the foot of the hill, 440 feet below the Observatory, and some curious variations of temperature and precipitation have been noted between the two stations, but enough data have not yet been accumulated for publication. Among the special instruments in use may be specified a Campbell-Stokes bright-sunshine recorder, which is believed to be the only one in the United States; a Jordan sunshine-recorder, which registers both bright and faint sunshine photographically; and a mirror for the measurement of the azimuth and altitude of clouds, but these results are not yet ready for publication. The mean temperature for the year was 45°·6. The absolute maximum in the shade was 91°·0 in July, and the minimum −15°·0 in January, giving a yearly range of 106°. The greatest daily range was 38°·2 on December 25, and the least 1°·7 in February. The total rainfall and melted snow was 46·99 inches, measured on 132 days; the greatest monthly fall being 8·29 inches in February, and the least 1·52 inch in June. The work is accompanied by tracings from the self-recording instruments, selected to illustrate certain phenomena during the year, with explanatory text, a practice which is both inexpensive and very instructive. The hourly tabulations of atmospheric pressure and wind velocity have been published *in extenso*.

ON February 5 last there was a shower of ashes, lasting from 7 a.m. to 11 a.m., at Finschhafen in Kaiser Wilhelm's Land. It covered the surrounding district with a layer of pale grey volcanic ashes. As the condition of the winds at the time was abnormal, it is impossible to say in what locality the volcanic eruption took place. Dr. Schrader reports that on February 2 a bright red halo, as if produced by smoke at a great elevation, was noticed around the sun; a few evenings before, similar halos had been noticed around the moon. Samples of the ashes have been sent to Dr. Neumayer, of Hamburg, for analysis.

SOME time ago, Mr. F. W. Putnam, the American archæologist, wrote a letter to the newspapers, pointing out that the Serpent Mound in Adams County, Ohio, had lately been much damaged by "wash-outs," and begging that steps might be taken for its preservation. Thereupon three Boston ladies took the matter in hand. The money they asked for was soon obtained, and now the ground upon which the mound is situated has been bought, and handed over to the guardianship of the Trustees of the Peabody Museum of American Archæology and Ethnology. Mr. Putnam, through whom the purchase was effected, proposes to spend the approaching autumn in the neighbourhood of the mound, restoring it where it has been injured, transforming wheat-fields into grass lawns, making paths and fences, and planting trees. "So long as the place is respected and guarded by all who visit it," he says in a letter to a Cincinnati newspaper, "the park will be free to all, but should any vandalism be committed, an arrangement would at once be made to put a keeper at the place, and possibly entrance fees would have to be charged in order to pay the expenses."

PROF. A. H. KEANE's translation of "The Necropolis of Ancon, in Peru," a German contribution to our knowledge of the culture and industries of the empire of the Incas, presenting the results of excavations made on the spot by W. Reiss and A. Stübel, has been issued in fourteen parts by Messrs. A. Asher and Co. during the years 1880–87. The work is now ready in three volumes, which contain, besides a comprehensive text, 141 coloured plates in folio. A separate volume, complete in itself, but at the same time forming a supplement to the present work, is in course of preparation. It will contain treatises by Herren W. Reiss, A. Stübel, L. Wittmack, R. Virchow, and A. Nehring.

WE have received Part I. of the Annual Report of the Board of Regents of the Smithsonian Institution, showing the operations, expenditures, and condition of the Institution, to July 1885. In addition to the Secretary's Report, there is a general appendix containing some valuable scientific papers. In one set of these papers an account is given, by eminent writers, of the progress made during the year 1884 in astronomy, geography, physics, chemistry, and other sciences. Other papers deal with various problems in anthropology.

THE Clarendon Press is publishing a fourth edition of "Exercises in Practical Chemistry," by Mr. A. G. Vernon Harcourt, F.R.S., and Mr. H. G. Madan. The first volume, containing elementary exercises, has been issued. In the preface to this new edition, Mr. Madan, who has undertaken the task of revision, explains that he has made some verbal alterations, introduced additional experiments and exercises, and somewhat altered the course of analysis of a single substance. In many cases the preparation of useful compounds of the radicle is more fully dealt with than in former editions.

THE "Flora of West Yorkshire," a volume of about 800 pages, by Mr. Frederick Arnold Lees, will be ready in August. It will be published by the Yorkshire Naturalists' Union, by subscription, and will form an extra volume of the Botanical Series of the Transactions of the Union. The work is divided into four sections—(1) Climatology; (2) Lithology; (3) the Botanical Bibliography of the Riding; (4) the Flora proper. With regard to the fourth section, it is claimed that "such a complete flora for any district in the world has never before been published, more than 3000 species being dealt with."

AN interesting volume relating to the "Grand Concours International des Sciences et de l'Industrie," which is to be held at Brussels in the year 1888, has just been issued. It consists of reports drawn up by the Committees which have been appointed to make preparations for the Exhibition. Each of these reports includes a letter addressed to producers, a general and detailed classification of objects, a list of sub-committees, and a series of desiderata in the department to which the report relates. If the "Grand Concours International" corresponds to the scheme which the Committees have worked out, it will be one of the most complete and suggestive Exhibitions that have yet been held.

ON August 7 the University of Göttingen will celebrate the 150th anniversary of its foundation.

THE annual *conversazione* given by the students of the Finsbury Technical College was held on Friday the 15th inst., and was remarkably successful. The College was tastefully decorated with flowers and flags, and a large fountain, illuminated by powerful coloured arc and incandescent lamps, played during the evening. All the rooms were thrown open to visitors, and exhibitions of chemical, electrical, and mechanical apparatus and manufactures were arranged in the laboratories. Over fifty of the leading scientific firms lent exhibits, and one electrical firm sent over £500 worth of apparatus. In the workshops specimens of the work of the students during the session were shown. Two concerts, both attended by crowded audiences, were given; and Prof. Ayrton lectured on "Church Bells," and Prof. Meldola on "Spectrum Analysis." Over four hundred visitors were present, including many distinguished men of science and commerce; and the students are to be congratulated on having provided a very pleasant entertainment for their friends.

THE additions to the Zoological Society's Gardens during the past week include a Pig-tailed Monkey (*Macacus nemestrinus*) from Java, presented by Mrs. Lewis; a Tiger (*Felis tigris* ♂) from India, presented by Mr. Sandford Kilby; a Turtle-Dove (*Turtur communis*), British, presented by Mr. R. Humphries; a Bonnet Monkey (*Macacus sinicus* ♀) from India, two Booted Eagles (*Nisaetus pennatus*) from Spain, a Golden-crowned Conure (*Conurus aureus*) from Brazil, two Alligators (*Alligator mississippiensis*) from the Mississippi, two Common Toads (*Bufo vulgaris*) from North Africa, deposited; a Ruffed Lemur (*Lemur varius*) from Madagascar, an Elate Hornbill (*Ceratogymna elata*) from West Africa, two Common Boas (*Boa constrictor*) from South America, purchased; a Squirrel-like Phalanger (*Belideus sciureus*) born in the Gardens; two Diuca Finches (*Diuca grisea*), an Auriculated Dove (*Zenaida auriculata*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE NICE OBSERVATORY.—M. Faye has published in the *Comptes rendus*, tome cv. No. 1, a note on the work of the Nice Observatory, from which the following particulars are extracted:—As soon as a small meridian circle by Gautier had been erected at the new Observatory, M. Perrotin, the Director determined the difference of longitude telegraphically from Paris and from Milan. These operations gave for the difference: Paris–Milan, 27m. 25·325s., whilst a direct determination previously made by MM. Perrier and Celoria gave 27m. 25·313s. The value 43° 43′ 16″·9 has been provisionally adopted for the latitude. With the equatorial of 0·38 m. aperture M. Perrotin has undertaken an extensive series of double-star measures, which have already proved of great excellence and value. It is proposed to continue these measures on a more extended scale with the large telescope of 0·76 m. aperture. A large number of observations of comets and of minor planets have been made by M. Perrotin and by M. Charlois, his assistant. The latter has also quite recently discovered a new asteroid (No. 267). M. Faye goes on to speak of the spectroscopic researches carried out at Nice by the late M. Thollon, particularly those connected with the investigation of the telluric lines in the solar spectrum. As our readers will remember, M. Thollon showed that in the regions B and α of the solar spectrum some of the telluric lines are due, not to an element varying with the temperature, such as aqueous vapour, but to a constituent of the atmosphere, such as oxygen, the influence of which varies with the altitude of the Sun only. M. Egoroff afterwards confirmed this by showing that the lines referred to are due to the oxygen present in our atmosphere.

The instrumental equipment of the Nice Observatory is now all but complete, and M. Faye speaks with enthusiasm of the career of usefulness before it—favoured as it is with a splendid climate, and, thanks to the munificence of M. Bischoffsheim, with instruments which suffice to place it in the front rank of modern Observatories.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *JULY* 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 July 24

Sun rises, 4h. 14m.; souths, 12h. 6m. 14·3s.; sets, 19h. 59m.; decl. on meridian, 19° 54′ N.: Sidereal Time at Sunset, 16h. 8m.

Moon (at First Quarter on July 27) rises, 8h. 38m.; souths, 15h. 26m.; sets, 22h. 0m.; decl. on meridian, 5° 53′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	5 20	12 33	19 46	13 24 N.
Venus	8 33	15 1	21 29	4 47 N.
Mars	2 8	10 28	18 48	23 53 N.
Jupiter	12 20	17 35	22 50	9 29 S.
Saturn	3 48	11 48	19 48	20 57 N.

Occultation of Star by the Moon (visible at Greenwich).

July.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
25	B.A.C. 4277	6	20 20	20 45	34 350

July.	h.	
24	4	Venus in conjunction with and 3° 8′ south of the Moon.
27	1	Jupiter in conjunction with and 3° 59′ south of the Moon.
29	5	Mercury in inferior conjunction with the Sun.

Meteor-Showers.

The Aquarids, R.A. 340°, Decl. 13° S., near δ Aquarii, form the principal meteor-shower at this season of the year; the meteors from this radiant are slow, in marked contrast to those from Perseus, radiant at R.A. 32°, Decl. 55° N., at the same time, which are swift.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei ...	... 0 52·3	... 81 16 N.	.. July 27,	21 51	*m*
Algol	... 3 0·8	... 40 31 N.	,, 30,	2 5	*m*
δ Libræ ...	... 14 54·9	... 8 4 S.	,, 29,	22 50	*m*
S Ophiuchi ...	... 16 27·8	... 16 55 S.	... ,, 30,		*M*
U Ophiuchi ...	... 17 10·8	... 1 20 N.	... ,, 26,	3 16	*m*
			26,	23 24	*m*
U Sagittarii ...	... 18 25·2	... 19 12 S.	... ,, 25,	0 0	*M*
β Lyræ... ...	... 18 45·9	... 33 14 N.	... ,, 25,	8 30	*M*
T Sagittarii ...	... 19 9·7	... 17 10 S.	... ,, 28,		*M*
S Vulpeculæ	... 19 43·8	... 27 0 N.	... ,, 25,		*M*
η Aquilæ ...	... 19 46·7	... 0 43 N.	... ,, 25,	22 0	*M*
R Vulpeculæ	... 20 59·4	... 23 22 N.	... ,, 28,		*M*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

THE new supplementary part of *Petermann's Mitteilungen* (No. 87) is devoted to Dr. R. von Lendenfeld's explorations in the Australian Alps in 1885–86. The region explored by Dr. Lendenfeld covers the greater part of the mountain districts of Victoria and New South Wales, and already in NATURE and elsewhere he has given some details concerning the geological and glacial results of his work. In the present memoir he gives a sketch of the Australian Alps in general, their geology, physiography, meteorology, flora, and fauna; he indicates the general physiognomy of the mountain system, its leading ranges, its valleys, and its river systems. He then devotes separate sections to the Kosciusko group and the Bugong group, and to a discussion of the Australian Ice period. There can be no doubt, Dr. Lendenfeld maintains, that at one time the Australian highlands were deeply glaciated, and that during the Tertiary the climate of the country must have been far richer in moisture than it is at the present day.

IN the new number (vii.) of *Petermann's Mitteilungen* Dr. Gerhard Rohlfs describes in a letter to Dr. Schweinfurth the results of his recent exploration of the limestone plateau which borders each side of the great Wadi Arabah, in Central Egypt. General Tillo brings together elaborate data bearing on the variation of the mean sea-level above or below a normal zero in the various seas of Europe; and Nikolaus Latken contributes a short paper on mining in East Siberia for 1874–85. There is an excellent map of the Khuriseb Valley, extending south-east from Walfisch Bay, West Africa, by Dr. Stapff, which, with the accompanying paper, gives a very full idea of the geology of the region.

A NUMEROUS and carefully-equipped Expedition is being sent out this summer by the Finnish Society of Botany and Zoology for the exploration of the interior of the Kola Peninsula. Another Expedition, organized by the St. Petersburg Society of Naturalists, set out last month to Petropaulovsk, to explore the White Sea and the Mediterranean coast.

UNDER Prof. O. Doering, the Government of the Argentine province Cordoba is establishing a network of meteorological stations which will begin work in January 1888. It is intended to form and equip 40 stations of the first order, 15 of the second, 10 of the third, and 10 of the fourth order. The instruments are being obtained from Berlin.

DR. L. BRACKEBUSCH, Professor of Geology and Mineralogy in the University of Cordoba, has recently returned from a five-months' excursion in the Cordilleras, bringing with him rich collections of minerals, and a mass of geological, geographical, and hypsometrical data.

THE Venezuelan Government has, it is stated, organized an Expedition for the geological and anthropological exploration of the territory on the Upper Orinoco and the Amazons.

AT a recent meeting of the Geographical Society of the Pacific, Prof. Davidson stated that his study of the ocean currents had brought him to the conclusion that a branch of the Japanese warm current, the Kuro Siwo, does pass into the Arctic Ocean through Behring Strait; and he promised to lay before the Society, at a future meeting, some information on the subject.

ACCORDING to the last mail from Zanzibar Lieut. Wissmann has arrived at the Kavala mission station on Lake Tanganyika. The explorer left Luluaburg on the Sankuru in November last, to traverse the unknown country in which are the sources of the Lulongo, the Chuapa, and the Lomami. He then meant to reach Lake Tanganyika by Nyangwé.

TO the last part of the *Verhandlungen* of the Vienna Geographical Society (Nos. 5 and 6 of Band xxx.) Herr W. Putick contributes a valuable paper on the subterranean district of Inner Carniola, the curious region known as the Karst.

THE TECHNICAL EDUCATION BILL.

THE following is the speech delivered by Sir W. Hart Dyke on Monday in introducing the Technical Education Bill into the House of Commons:—

"In the observations that I am about to make I shall be as concise as possible, because I know that members are waiting to deal with other important matter. I feel that I am guilty of something like cruelty in introducing at this period of the session, after all we have gone through and with the labours still before us, any further legislation, but I plead in extenuation the fact that this is no new topic. It is one which has for some time past stirred up among the artisan classes considerable interest. Voluntary efforts have for some time past been made in this country in regard to technical instruction, and if I am asked why it is that we are going to endeavour to supplement by legislation what has been done the answer is that it is because we believe in the reality of this movement. For some years, not only among our artisan classes, but among our large employers of labour in industrial centres, it has been recognized that, though the commercial depression cannot be traceable to the lack of technical and commercial education in this country, yet that some part of it is due to the fact that Continental nations have had great advantages over us in regard to technical training for their youths, and that this has given them considerable commercial advantages over us. I am encouraged to hope that these proposals will meet with some acceptance from the House. If they enable the best material which is now turned out by our schools to continue longer in their school life and to start into some new educational groove for the benefit of themselves and of the industrial localities in which they live, and for the benefit also of the community at large, I think I may venture to urge that the time of the House will not be wasted in discussing these proposals. It is perfectly true that it may be urged that as I have not long held my present office I am rather rash in introducing this subject, and still more so considering that a Royal Commission has been sitting for some time and dealing with this great educational question. But I think that the House will agree with me that this is somewhat outside the scope of the Commission which is now sitting. There was a Royal Commission on Technical Education which reported in 1884. That Commission let in a flood of light on the question of technical instruction, and I should like for one instant to refer to their special recommendation as regards this country. As the House is aware, that Commission extended its labours to Continental countries, and conducted an exhaustive inquiry in connexion with this subject. The Commission pointed out that there was a considerable difference in respect of our treatment of the educational question and its treatment in countries abroad. They also pointed out that with the exception of France there was no European country of the first rank that has an educational Budget so large in amount as our own. They say that all our existing educational institutions will not alone accomplish the object aimed at, and that the localities must rely more than they have done hitherto upon their own special exertions. I may quote further from the Report of the Commission in reference to the advisability of introducing technical instruction into our schools. The Commissioners state that in Manchester, Sheffield, Birmingham, and other great centres, a considerable step has already been made in this direction, and they ask this pertinent question: "If we introduce needlework into girls' schools, why should not grants be made for manual instruction in boys' schools?" The Commissioners also recommended that rudimentary drawing should be continued throughout the standards. These are some of the recommendations with which the present Bill proposes to deal. I think I am not taking too sanguine a view of this scheme when I say that it will carry out all those proposals. The object of the Bill is to enable local authorities to provide for the establishment of technical schools or to assist in providing them, and also to give local authorities power to supplement existing teaching in elementary schools by technical instruction, whether by day or evening classes. There will also be a proposal in the Bill with regard to the ratepayers, to whom a power of vetoing any proposal under the Bill will be given. We propose that the Bill should be administered by the Science and Art Department—that is to say, that it should be administered subject to the directorate of that Department. We also propose that the Bill should have the limitation that no scholar should come under its operation until he has reached the sixth standard. The authorities for administering the powers conferred by the Bill will be School Boards where they exist, and where they do not exist town councils. I should just like to refer to Clause 4 of the Bill. To make the Bill acceptable to the ratepayers you must show that it is a cheap Bill and that consideration has been shown for them. Clause 4 is what I may call the operative clause of the Bill, and it enables local authorities to provide technical schools. Of course that would involve expense in building; but there is a sub-section of Clause 4 which enables the local authority to combine with any other local authorities. This will enable a system of combination to be adopted which will prove a great saving to the ratepayers; and, further, the next sub-section provides that the local authority may contribute towards the maintenance or provision of any technical school which has been established by any other local authority. It is further intended to include a provision that local authorities shall be empowered to rate for the purpose of supplementing any existing institution. These, I think, the House will admit are provisions which will enable this Bill to be worked cheaply. A further sub-section gives the local authority power to make any arrangements it may deem necessary or expedient for supplementing the technical instruction at present given in the schools. This provision I consider one of the most valuable in the Bill. It will enable technical instruction to be at once given without putting the ratepayers to any expense in building. I should like to refer for a moment to the limitation to the sixth standard. Though this will necessarily exclude many children, I think all interested in education will admit that the Bill should apply to the pick of our scholars, and that a good educational foundation should be required. With regard to the question of agricultural instruction, I am free to admit that the Bill, as drawn, can extend that instruction only to a very small extent, but I believe that the measure is capable of very considerable development, and that under certain of its sub-sections agricultural instruction may be afforded to a satisfactory extent. It is proposed to insert a provision in the Bill that where any local authority passes a resolution to establish a technical school a certain proportion of the ratepayers may demand a poll, but I am here at once met with a difficulty with regard to the metropolis—namely, that it is very wrong to propose to bring into existence the enormous voting power within the metropolis for such a purpose by this Bill. It may be asked how I propose to protect the ratepayers of the metropolis in regard to this matter. I have a proposal on the subject which has been drawn up by the Vice-Chairman of the London School Board, the hon. member for Worcestershire, and which has received the approval of the Chairman of that Board. I believe, from all I can gather, that that proposal will be popular with the present London School Board, and that it has been accepted by the hon. members for the metropolis who have been consulted in this matter, on the understanding that this policy alone will be carried out by the London School Board until the next election. I believe, also, that this proposal will be approved by the ratepayers of the metropolis. I have been asked by the hon. member for Worcestershire whether the scheme would not involve an extra charge for building, and I have been able to assure him that it will not do so. Therefore, Sir, I am prepared to admit that the members for the metropolis must be considered in this matter. Of course, if they think that further security to the ratepayers will be necessary it will be possible to insert an addition to this clause in the Bill to the effect that no action with reference to this Bill shall take place until after the next School Board election. I believe it would be a mistake to do anything of the kind; I believe that the interest of the ratepayers and of the great mass of working men in the metropolis may be safely trusted in the manner proposed by the Bill. Then, Sir, I may be asked this question, which, I think, is a very pertinent one. It is true that a vast amount has already been expended in the cause of technical instruction, and I may be asked: "If you once establish the principle of rating, will you not check the principle of voluntary effort?" I believe we shall do nothing of the kind, for several reasons. I believe that this will be an essentially popular measure among the working classes. I believe it will be impossible to check voluntary effort in a cause such as this where you supplement it by rates, for I believe that those who are spending money voluntarily are doing it in a cause which they know to be a very vast one, and that for all sums of money whenever spent in this cause more than compound interest will be repaid as the result. There are numerous instances in regard to this matter. It was only the other day I noticed that in Lambeth the Public Libraries Act was adopted. That is a case in point. What was the result there of adopting the principle of rating? I noticed that at the concluding meeting held, when arrangements were to be made for this new library under the system of rating, the hon. member for Barrow-in-Furness, who was in the chair, announced that a friend of his had not only given the ground but was going to build the whole library at his own cost. Numerous instances of the same type have come under my notice. Therefore I do not think we ought to dread that the establishment of voting power will check in any degree voluntary effort. I have only one other point to deal with—the administration of this Bill. We propose that this Bill shall be administered by the Science and Art Department at South Kensington. I have been anxious that the Bill should be so administered, for there we have a Department whose educational capacity has been thoroughly well tested. I have heard some hon. members attack the results of South Kensington as rather expensive. I am anxious that the House should be in a position to judge of the actual yearly expenditure at South Kensington, not only as regards administrative expenditure, but as regards its results. By the leave of the House, I shall therefore lay upon the table a document that will show in a concise form the actual expenditure at South Kensington for five years, both as regards administration and as regards results. Hon. members will then see how vast an increase there has been as regards payment by results and how small has been the increase of administrative expenditure. I should like to read to the House what that Department is now doing with regard to science and art. During 1886-87 there were 1936 schools or separate institutions in which instruction was given in one or more branches of science. There were 6976 classes in different branches of science, and the number of individual students under instruction was 100,419. At the May examination 127,900 papers were sent up for examination to South Kensington. I should like also to give some more instances to show the vast strides made in chemical instruction. In chemistry, 21,085 papers were worked at examinations. To show the advance made of late years in the facilities afforded for instruction in science of a thoroughly practical and experimental character, it may be mentioned that thirty years ago there were only one or two places where students could obtain laboratory instruction in chemistry, and that at very high fees. The Royal College of Chemistry, established in 1845, was one of these. Another was soon after started in Craig's Court. Now there are 234 chemical laboratories in connexion with the Science and Art Department in which students can obtain laboratory instruction at very low fees. There were 4257 separate benches at the last examination which afforded accommodation for 16,155 candidates. In the last session there were in operation 234 schools of art and 626 art classes, with 71,132 students in them; 50,000 were examined in May last, and the number of papers worked was 75,678. I should not for one moment have attempted to put the administration of this Bill under the Department of Science and Art at South Kensington if I had any doubt of its ability to work it with efficiency and economy. I thank the House for the attention with which it has heard me. I need not go again through the details. Happily this cannot be regarded as a party question. It is one which interests members on each side of the House, and although I do not submit the Bill as covering all the ground of technical instruction, I do believe that it is a measure which will do an enormous amount of good to our industrial population. In conclusion I would urge that, if it were only for the two provisions alone with regard to continuation classes and to evening classes, this Bill is worthy of the serious consideration of the House. I hope that hon. members will not at this time of the session overload the Bill with amendments.

SCIENTIFIC SERIALS.

Bulletin de la Société des Naturalistes de Moscou, 1886, No. 3.—On two great comets (41 and 42) of 1886, by Th. Bredichin (in French).—On the *Agromyza lateralis* and its metamorphoses, by Prof. Lindeman (in German).—On the iron-bearing mud of Lipetzk, by E. Kislakovsky. It appears much like that of Franzensbad in Germany, and especially that of Ciechocinek in Poland.—On the Ammonites of the group *Olcostephanus versicolor* (Trautschold), by Mary Pavlow (in French, with two plates). Studying a rich collection of *Ammonites versicolor*, some of which reach 8 inches in diameter, while others have the size of a pin's head, the author considers them as belonging to the genus *Olcostephanus*, and establishes the following species, of which the last three are new: *O. versicolor*, *elatus*, *subinversus*, *inversus*, and *coronatiformis*.—On the importance of oxygen for plants, by W. Palladin (summed up in German). An elaborate research into the amount of matter destroyed in consequence of fermentation in an atmosphere devoid of oxygen, as also into the relations between the breathing of plants and their growth.—On the dynamic centra of a rotation-ellipsoid, with relation to earth, by K. Weihrauch, being a mathematical inquiry (in German) from which it results that the centres of attraction are situated in the earth nearer to the centre of figure than would be the case in an homogeneous ellipsoid of the same average density.—On the Algæ of Moscow, by A. Artari (in French), being a continuation of a former publication, and containing a list of eighty-five more species, chiefly Bacillariaceæ.—On the fauna of the lakes of the Slavyansk mineral waters, by P. Stepanoff. The fauna is mixed and contains representatives both of fresh-water and marine species, these latter being chiefly found amidst the Infusoriæ.—The annual report of the Society contains obituary notices of the late President of the Society, Dr. Renard.

No. 4.—Vascular plants of Caucasus, by M. Smirnoff. In this second paper (in French) the nebulosity of different parts of Caucasus is discussed, and data given.—Wild plants of the Government of Tambof, by D. Litvinoff, continued.—The species of *Thrips* living on corn in Middle Russia, by Prof. Lindeman (in German). The new species *Thrips secalina* and *Phlocothrips armata* are described together with former ones.—Zoological researches in the Kirghiz Steppe, by P. Nazarow, being a most valuable review of the fauna of the steppe, especially of its avi-fauna (with a map).—Speeches pronounced at the death of Dr. Renard.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 16.—"Experiments on the Discharge of Electricity through Gases." (Second Paper.) By Arthur Schuster, F.R.S.

In thinking over the phenomena presented to us in vacuum tubes, I always felt a difficulty owing to our ignorance of the conditions which hold at the surface of bodies, either suspended in or near the discharge, or even at the boundary of the vessel through which the discharge is passing. It is evident enough that if there is a flow of electricity on the surface of a non-conductor that flow must be tangential, but it is not so clear whether we are justified to conclude from this that there can be no normal forces at such surfaces, for it is not necessary that the flow should always take place along the lines of force.

Supposing we suspend two pieces of gold leaf, as in an electroscope, at any place in a partially exhausted vessel, and render them divergent by electrification, they should collapse as soon as the discharge begins to pass, if tangential forces only can permanently exist at their surface. This I have tested by experiment, and found to be the case.

A cylindrical glass vessel 38 centimetres high and 15 centimetres wide, was divided into two approximately equal compartments by a vertical metallic screen. There was an open space of about 5 millimetres between the screen and the sides of the vessel, a space of about 4 centimetres above, and 2·5 centimetres below the screen. One compartment contained two pieces of gold leaf, which could be charged from the outside. The other compartment contained two electrodes about 5 centimetres apart, and 2 centimetres from the screen; these distances could be varied during the experiment. The screen was always conducted to earth, and the electric fields on the two sides of the screen were therefore nearly independent of each other. When the gold leaves were electrified and divergent, and discharges from the induction-coil passed between the electrodes on the other side, no effect could be observed at atmospheric pressure: the gold leaves remained divergent.

At a pressure of about 4·3 centimetres of mercury, the effect I was looking for first appeared; when the discharge passed, the divergent leaves slowly collapsed, and as the pressure was further diminished the collapse took place more and more quickly.

We have here, then, even with the discontinuous discharge, a neutralization of all normal forces at the surface of the gold leaf.

It seemed to me to be interesting to observe more particularly the effects of the ordinary discharges we have at our command, at atmospheric pressure. I took two light balls, and suspended them so that they could be made to diverge by electrification. The electrodes (either spheres or points) of a Voss machine were placed at a distance of 3 inches from each other, and the electrified balls were placed at a distance of 9 inches from the discharge. The results are contained in the following table, in which the first two columns indicate whether the electrodes of the Voss machine were points or spheres. The third column gives the electrification of the balls, and the fourth column the results.

Negative electrode.	Positive electrode.	Balls.	Result.
Sphere	Sphere	Positive	Balls collapse slowly
,,	,,	Negative	,, remain divergent
Point	Point	Positive	,, collapse quickly
,,	,,	Negative	,, remain divergent
Sphere	,,	Positive	,, ,, ,,
,,	,,	Negative	,, collapse slowly
Point	Sphere	Positive	,, ,, quickly
,,	,,	Negative	,, remain divergent

It will be seen that when the two electrodes are similar, whether spheres or points, the balls collapse when they are electrified positively only; but that when one electrode is a sphere and another a point, the balls collapse if their electrification is of the opposite nature to that supplied by the point.

The conclusion thus arrived at, which will be proved beyond possibility of doubt in the second part of this paper, is this: we can only have tangential forces at the surfaces of vessels inclosing a gas through which a discharge is passing, provided no current crosses the surface.

After I had convinced myself that an electrified body placed in a partial vacuum through which an electric current is going, has its electricity quickly neutralized, it was doubtful still whether this neutralization was due to an actual discharge or merely to a covering of electrified particles of an opposite sign. The question is a vital one in all cases where potentials have to be measured. For we can only measure potentials of a gas by measuring the potential of a metal in contact with it; and if an electrified body is covered by electrified particles of a different sign, there is a finite difference of potential between the metal and the gas, and we should have to inquire carefully, in each particular case, how far such a difference would affect our conclusions.

The question is settled by the principal result of this paper:

A steady current of electricity can be obtained in air from electrodes at the ordinary temperature which are at a difference of potential of one-quarter of a volt only (and probably less); provided that an independent current is maintained in the same closed vessel.

In other words, a continuous discharge throws the whole vessel into such a state that it will conduct for electromotive forces which I believe to be indefinitely small, but which the sensitiveness of the galvanometer I used has prevented me from tracing with certainty below a quarter of a volt. There cannot be therefore a finite difference of potential between a gas and a metal in contact greater than that amount.

The same vessel was used as in the previous experiment.

On one side of a screen conducted to earth, were the two main electrodes, from which the current of the large battery passed. On the other side were two auxiliary electrodes connected to the two poles of a small battery. Whenever the main current passed, the small battery was found to send a steady current which could be measured. The smallest electromotive force which was observed to send a current under these conditions was one-sixth of a Leclanché.

An electromotive force of one-sixth of a Leclanché is about one-quarter of a volt, and a current has thus been obtained in a gas from an electromotive force which could not maintain a current through water.

An electromotive force of 0·1 volt gave doubtful results, but this was probably due to the experimental difficulty of detecting the current.

In some previous experiments, which, however, were not quite free from objection on other grounds, the lowest electromotive force for which the currents could be measured was 0·2 volt.

The experimental arrangement which is the best for the qualitative investigation of the effect is not the best for quantitative measurements, and I have therefore not endeavoured to follow out to any great extent the quantitative laws of these currents produced by low electromotive forces. I may give, however, some facts which I have observed. The intensity of the current depends on a great many circumstances.

(1) It increases rapidly with the intensity of the main discharge, and also with a reduction of pressure, as far as I have tried it (that is about half a millimetre).

(2) The intensity of the current from the auxiliary battery increases less rapidly than the electromotive force.

(3) In some experiments, in which one of the electrodes of the auxiliary battery was a copper wire and the other a copper cylinder, the current was nearly always considerably stronger when the larger surface was the kathode.

(4) Anything that facilitates the diffusion of gas from the main current to the auxiliary electrodes will increase the strength of the current observed. In some experiments, in which the screen separating the two fields was made of wire gauze instead of tinfoil, the currents were stronger than those given above.

These experiments show conclusively that there is nothing peculiar in the gaseous state of a body to prevent any electromotive force, however small, to produce a current. If a finite electromotive force is required under ordinary circumstances, the fact cannot be accounted for, as Edlund and others have done, by a special surface resistance which has to be overcome by a finite difference of potential at the surface.

I think the facts are very well accounted for by the theory which I have proposed in my last paper. If the two atoms of a gas making up the molecule are charged with opposite electricities, but are held together in addition by molecular forces, a finite force is required to overcome the latter. But as soon as that force is overcome and the atoms themselves are set free to diffuse and constitute a current, these atoms will be able to follow any electromotive force which we may apply. If, then, we have auxiliary electrodes, these electrodes will establish their electric field, which we can never screen off completely from any other part of the vessel except by closed surfaces. The atoms, with their positive and negative charges, will diffuse across to the auxiliary electrodes and give off their electricity to them. No finite difference of potential is required in the auxiliary electrodes, because, even if there is work done in making an atom interchange its positive for negative electricity, that work is undone again at the other pole, where atoms of a similar kind interchange negative for positive electricity.

I should like, in conclusion, to point out an important application of these results. I have last year obtained by calculation results which seem to show that the principal cause of the diurnal variation of terrestrial magnetism is to be looked for in the upper regions of the atmosphere. Prof. Balfour Stewart at various times suggested that the air-currents in these regions may, owing to the lines of force of terrestrial magnetism, have electric currents circulating in them.

The difficulty against this supposition always seemed to me to lie in the fact that the electromotive forces required to start a current were larger than those which could possibly exist in the atmosphere. But as there are very likely continuous electric disturbances going on, such as we observe in auroræ and thunderstorms, the regions within which these discharges take place would act as conductors for any additional electromotive force however small, so that any regular motion, such as tidal motions, could very well produce periodic effects affecting our magnetic needles.

If these original discharges increase in importance, then, according to the results obtained in this paper, the currents due to the smaller periodic causes would increase also, and they may increase in a very rapid ratio. We know that the electric discharges in the upper regions of the atmosphere are considerably stronger at times of many sunspots, and this may account for the fact that at those times the amplitude of the daily oscillation of the magnetic needle is considerably increased.

I have had considerable assistance in these experiments from my assistant, Mr. Stanton, to whom my best thanks are due.

Geological Society, June 23.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—On nepheline rocks in Brazil, with special reference to the association of phonolite and foyaite, by Mr. Orville A. Derby. The author refers to the phonolites and associated basalts of Fernando Noronha, a deep-sea island off the north-eastern shoulder of the continent of South America. Nepheline rocks of a somewhat different character are abundantly developed on the mainland, and under conditions favourable for throwing light on the relations existing between the granitic type, foyaite, and the other members of the group. There are some mountains near Rio de Janeiro composed of these rocks, as is also the peak of Itatiaia, 3000 metres high, the loftiest mountain of eastern South America. A cursory examination of some of these localities having shown an apparent relation between foyaite, phonolite, trachyte, and certain types of basalt, Mr. Derby determined to visit the Caldas region, where a railway under construction gave unusual facilities for examining this series. A fine development of foyaite, phonolite, and tuff was found, associated with several types that have not yet been met with in the other localities. The existence of a leucite basalt was recognized. The bulk of the paper was devoted to a detailed description of these railway-sections, and the following deductions are drawn:—(1) The substantial identity, as regards mode of occurrence and geological age, of the Caldas phonolites and foyaites. (2) The connexion of the latter through the phonolites with a typical volcanic series containing both deep-seated and aërial types of deposits. (3) The equal, if not greater, antiquity of the leucite rocks as compared with the nepheline rocks, whether felsitic, as phonolite, or granitic, as foyaite. (4) The probable Palæozoic age of the whole eruptive series. The President said it was seldom that a paper containing such important facts was presented to the Society. It was reserved to Mr. Derby to have proved that plutonic rocks containing nepheline (foyaite) passed into volcanic masses which were true phonolites. This Mr. Derby had clearly established by observations in the field. He had also shown that leucite existed in rocks of Palæozoic age, thus rendering untenable the last stronghold of those who insisted on making geological age a primary factor in petrographical classification. He alluded also to the value of the independent determinations of Prof. Rosenbusch. Mr. Bauerman said he had been over portions of the ground with the author, and was glad to add his testimony to the value of the paper. He spoke of the importance, in a geological sense, of these generalizations. It was remarkable how highly crystalline masses of rock pass over into a sort of phonolite. These were associated with Palæozoic masses, which were pre-Permian, or at least pre-Triassic. He alluded to the difficulty of investigating Fernando Noronha, and also to the difficulties attendant upon the investigation of rocks in Brazil, which were subject to such an enormous amount of local alteration. Prof. Bonney also expressed his sense of the value of the paper. He alluded to the comparative rarity of nepheline and leucite rocks, and to the confusion in the nomenclature. He was reminded of the nepheline rocks near Montreal, where dolerite was broken through by nepheline syenite, associated with tephrites and phonolites. Although there might be a doubt here, these rocks were most probably of Silurian age; but the evidence in Brazil was still clearer as to the Palæozoic age, and he believed that, in the case of some other masses, the evidence had satisfied the Canadian geologists. He alluded also to the nepheline rocks in the Katzen-Buckel, where there was a similar passage from coarse-grained to fine-grained. Dr. Hatch said that in this case leucite was clearly shown to be of Palæozoic age, and regarded the paper as a step towards the better classification of this group of rocks. Prof. Seeley asked for evidence as to the identification of the leucite. The President thought there was no possibility of a mistake in this respect. As regards the rocks of the

Katzen-Buckel, none were truly holocrystalline, and hence they could not be compared with foyaite or elæolite-syenite.—Notes on the metamorphic rocks of South Devon, by Miss Catherine A. Raisin, B.Sc. Communicated by Prof. T. G. Bonney, F.R.S.—On the ancient beach and boulders near Braunton and Croyde in North Devon, by Prof. T. McKenny Hughes.—Notes on the formation of coal-seams, as suggested by evidence collected chiefly in the Leicestershire and South Derbyshire coal-field, by Mr. W. S. Gresley.—Note on some Dinosaurian remains in the collection of A. Leeds, Esq.; Part I. *Ornithopsis leedsii*, Part II. *Omosaurus*, sp., by Mr. J. W. Hulke, F.R.S.—Notes on some Polyzoa from the Lias, by Mr. Edwin A. Walford.—On the superficial geology of the southern portion of the Wealden area, by Mr. J. Vincent Elsden. Communicated by the President.—Report on palæobotanical investigations of the Tertiary flora of Australia, by Dr. Constantin Baron von Ettingshausen.—On some new features in *Pelanechinus corallinus*, by Mr. T. T. Groom. Communicated by Prof. T. McKenny Hughes.—On boulders found in seams of coal, by Mr. John Spencer.

EDINBURGH.

Royal Society, June 6.—Mr. J. Murray, Vice-President, in the chair.—Mr. J. B. Readman read a paper on a furnace capable of melting nickel and cobalt.—Mr. R. Kidston communicated the last part of his paper on the fossil flora of the Radstock series of the Somerset and Bristol coal-fields.—Prof. Grainger Stewart read a paper on investigations into the discharge of albumen from the kidneys of healthy people.—Dr. H. R. Mill communicated the result of his investigations on the salinity and temperature of the Firths of Inverness, Cromarty, and Dornoch, and of the North Sea.—Prof. Ewart discussed the existence of Bacteria in the lymph, &c., of fish and other vertebrates.

June 20.—Sheriff Forbes Irvine, Vice-President, in the chair.—Prof. Geikie communicated a paper by Prof. Frederico Sacco on the origin of the great Alpine lakes.—Dr. E. Sang read a paper on the minute vibrations of a uniform chain hung by one end, and on the functions arising in the course of the inquiry.—Dr. A. W. Hare read a note on the biological tests employed in estimating the purity of water.—Prof. Tait submitted a paper by Mr. A. H. Anglin on alternants which are constant multiples of the difference-product of the variables.

PARIS.

Academy of Sciences, July 11.—M. Janssen in the chair.—Presentation of the minutes of the International Astronomical Congress for the execution of the photographic chart of the heavens, by M. Mouchez. It was stated that although the Congress held in Paris last April concluded its labours before the end of the same month, the publication of its proceedings has been delayed till now, owing to the necessity of sending the proofs for revision to the members scattered over various parts of the world. Two main resolutions were arrived at, the first regarding the adoption of the photographic process, and of a uniform class of instruments, so as to secure the greatest possible degree of homogeneity in the results. The instrument unanimously adopted was that of Gautier, already in use for two years in the Paris Observatory. The second resolution regarded the period and extent of work to be carried out at the various international stations. It was decided that there should be two series of stellar photographs, the first comprising stars to the 11th magnitude approximately, the second to include all down to the 14th magnitude, or about 15,000,000 altogether. A permanent Bureau was also appointed, for the purpose of executing the decisions of the Congress and maintaining constant relations between the members and the Observatories taking part in the work of stellar photography. A special bulletin may also perhaps be issued from time to time, to report generally on the progress of this great astronomic undertaking.—Heat of formation of hydrotelluric acid, by MM. Berthelot and Ch. Fabre. Four determinations effected by the agency of the perchloride of iron in solution, give a mean of 29·12 calories.—On the presence of microscopic crystals of albite in various limestone rocks of the Western Alps, by M. Ch. Lory. The genesis of these crystals appears to have been generally favoured in the Western Alps by the conditions under which the Triassic formations have been developed. They occur somewhat exceptionally in association with the Middle Lias at Villette, and about the head of the long fjord of the Miocene sea, which flowed from the Maritime Alps to a point a little north of Saint-Jean-de-Maurienne. Hence the formation of these microscopic crystals appears to be connected with the special character of the deposits, and to be independent of the more or less intense local mechanical actions which affected the various stratified rocks at the time of the Alpine dislocations.—Presentation of M. Godefroy Malloizel's volume containing a complete list of M. Chevreul's writings issued between the years 1806–86, by M. de Quatrefages. The cost of this publication has been met by the balance of the sum subscribed by the youth of France to strike a medal in honour of M. Chevreul on his hundredth anniversary. Besides the titles and dates of everything issued by M. Chevreul during the last eighty years, careful tables of contents are appended to all memoirs and scientific papers of any considerable length. An introduction is added by M. Desnoyers, Librarian of the Museum, and a fine portrait of the illustrious *doyen* of the *savants* of the whole world, by M. Champollion.—On antipyrine as a substitute for morphine in subcutaneous injections, by M. Germain Sée. The continued experience of the author since his first communication on antipyrine as an anæsthetic (April 18, 1887), shows its decided superiority over morphine in all cases of rheumatic, hepatic, and cardiac affections. It is administered in the same way, but is more easily prepared, more efficacious, and entirely free from the dangerous consequences too often attending the use of morphine.—On a simple dynamic method of determining the degree of isotropy of an elastic solid body, by M. E. Mercadier. According to Saint Venant, in all true solid isotropes $\frac{\lambda}{\mu} = 1$, where λ and μ are two characteristic quantities of a solid body, by means of which may be expressed all the coefficients relative to its elasticity (Lamé). Hence, if this relation can be measured for different bodies, their degree of isotropy may be determined by the difference between the value of such relation and unity. M. Mercadier here supplies a simple method for making this determination based on the theory of the vibrations of circular plaques, the laws of which have recently been verified by him. He shows, for instance, that for glass $\lambda = \mu$; that is to say, that it is an isotropous body. This is an extremely simple confirmation, by a *dynamic* process, of the result of the beautiful experiments made by M. Cornu on glass by a *static* method.—On the alums formed by selenic acid, by M. Charles Fabre. Continuing the studies of Wohlwil, Wöhler, and Petterson, the author here describes a series of selenic alums with alumina or sesquioxide of chromium base, which he has succeeded in preparing. They comprise the alums of alumina corresponding to the general formula $Al_2O_3 . 3SeO_3 + MO . SeO_3 + 24HO$, and the alums of chromium corresponding to the general formula $Cr_2O_3 . 3SeO_3 + MO . SeO_3 + 24HO$.—Researches on the reactions of the vanadates considered from the stand-point of chemical analysis, by M. Ad. Carnot. In this paper the author completes the study of the reactions produced between the vanadates and the chief metallic salts under the ordinary conditions of analysis. Amongst the salts here treated are those of cobalt, nickel, zinc, cadmium, copper, mercury, lead, and bismuth.

BERLIN.

Physiological Society, June 17.—Prof. Du Bois-Reymond, President, in the chair.—Prof. Ewald spoke on the behaviour of salol (salicylate of phenol) in the stomach, a question which he has investigated in order to obtain information as to the movements of the stomach in relation to the time in which the contents of this organ are sent on into the intestine. Prof. Nencki had stated that salol is not acted upon by gastric juice, but is split up into salicylic acid and phenol by the action of pancreatic juice. Prof. Ewald's experiments confirmed the statement that salol undergoes no change in the stomach; thus, after administering salol, this substance could be detected in portions of the contents of the stomach examined at intervals of from one-half to three hours after it had been taken. Pancreatic juice was found to be similarly inert on salol, but on the other hand it was decomposed by most alkaline fluids. When injected into the intestine through a fistula, salol could readily be detected after half an hour, as salicyluric acid, in the urine. Since, therefore, salol undergoes no change in the stomach, but is readily decomposed in the intestine, and appears in half an hour as salicyluric acid in the urine, it was found to be extremely well suited to the purposes of the proposed experiments. When salol is given to healthy men whose gastric apparatus is in a normal condition, whether on an empty stomach, or with food, or at different stages of gastric digestion, salicyluric acid was found in their urine on an average three-quarters of an hour after it had been taken. From the data given above, the salol must have remained one-quarter of an hour in the stomach. In the case of patients suffering from gastric dilatation, the salol remained much longer in the stomach. The time which elapses between the administration of salol and the appearance of salicyluric acid in the urine may hence be used as an important means of diagnosing cases of slight gastric dilatation. After prolonged electrical stimulation of the abdominal muscles, the passage of salol into the intestine was quickened.—Prof. Zuntz criticised a theory of the excretion of carbonic acid in the pulmonary alveoli which has been put forward by von Fleischl, according to which the shock given to the blood by the contraction of the heart is to be regarded as the chief cause of the diffusion of the carbonic acid through the alveolar walls. The speaker refuted this theory as being both unproved and unnecessary. —Dr. Goldschneider communicated the results of his experiments on the reaction-time of the perception of temperature. It has been known for a long time that cold is more quickly perceived than heat. As a starting-point, the speaker had first carried out some direct measurements. He sought out portions of the surface of the body which were equally sensitive to heat and cold; these parts were then stimulated as far as possible with equal intensity, and the results were as follows, taken as a mean of about two thousand separate measurements:—The reaction-time for cold as a stimulus is for the face 13·5, for the arm 18, for the abdomen 22, and for the knee 25 hundredths of a second. When an equally strong heat stimulus was applied, the numbers obtained were 19, 27, 62, and 79 hundredths of a second. The ratio of the reaction-times was found to be about the same when the stimuli were applied to such a nerve as the trigeminal which goes straight to the brain and to a spinal nerve. The experiments on thermal stimulation were made by bringing a metallic button in contact with the skin and recording electrically the moment of contact; the resulting sensation was indicated by a Beiss key. The degree of heat and cold employed as a stimulus was selected so as to differ by equal amounts from the temperature of the skin. From the results of the experiments, Dr. Goldschneider deduced some theoretical conclusions as to the nature of our sensations of heat and cold.

Physical Society, June 27.—Prof. von Helmholtz, President, in the chair.—Prof. von Bezold demonstrated the currents, which he has very fully investigated, which occur in a fluid as the result of varying temperatures or the rotation of the vessel in which the fluid is contained. These currents were made visible, as they occur in a large mass of water, by means of a few drops of hectograph ink, which at first spreads itself out in radiating lines over the surface, then sinks in the form of threads and columns, and, following the direction of the currents in the fluid, presents an extremely interesting appearance of rotatory formations.—Dr. Richarz has closely studied what takes place in an electrolyzing cell during the decomposition of water in the immediate neighbourhood of the electrodes during the passage of the currents of electrolytic convection. As is well known, an electromotive force of 1·5 Daniell is necessary in order that the current may pass electrolytically and the water be decomposed; if the electromotive force is less than the above, the water is not decomposed, but at the same time it can be shown that the electricity does traverse the fluid. According to Von Helmholtz's views on electrolysis, when the electromotive force is small, currents of electrolytic convection pass through the fluid, which are kept up by the occlusion of the positively charged hydrogen atoms at the kathode and by the neutral oxygen in solution. Starting from the work of Moritz Traube, who has proved the formation of hydrogen peroxide at the kathode in the electrolyzing cell, Dr. Richarz has been able to prove, both qualitatively and quantitatively, the formation of hydrogen peroxide at the kathode during the passage of convection currents. This formation of the peroxide takes place, according to the views of the speaker, by the union of two atoms of the occluded hydrogen with the neutral molecule of the dissolved oxygen, which has given up its positive charge to the kathode. As the result of this separation of the occluded hydrogen, fresh portions of hydrogen can be occluded by the metal of the electrode, and in this way a renewal of the electric current can take place.—Prof. Neesen described a vapour-calorimeter, consisting of a glass vessel into the centre of which projects a glass tube, closed at the lower end, for the reception of the substance under investigation. This tube is surrounded with a mass of lamp-wick, which is saturated with ether, and dips into a small quantity of liquid ether in the bottom of the outer vessel. Another glass vessel, exactly similar to the above, is joined to it by means of a capillary U-tube, in which a small drop of ether serves as an index. When a warm substance is dropped into the calorimeter, an amount of ether is converted into vapour proportional to the heat given up, and the volume of this vapour, as measured by the displacement of the ether index, gives the heat yielded by the substance. Prof. Neesen is still engaged in testing and improving the calorimeter, and only made the above preliminary communication as this was the last meeting of the Society before the summer vacation.—Dr. Grunmach exhibited a double quartz plate, which was not made of a right- and left-handed quartz plate, but was cut from a twin crystal, in which the fusion of the two crystals is so perfect that every slice cut from this twin crystal may be used as a double plate in the polarizing apparatus.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Walks in the Ardennes: J. W. Richards (Low).—Welsh Question and Druidism, Third Edition: Griffith (R. Banks).—Annual Report of the Department of Revenue, Settlement, and Agriculture, for 1885–86 (Madras).—Journal of the Royal Statistical Society, June (Stanford).—Journal of Physiology, vol. viii. No. 2 (Cambridge).—The Indian Forester, vols. ii., iv., vi., vii., viii. (Calcutta).—Annalen der Physik und Chemie, 1887, No. 8a (Leipzig).—Museum d'Histoire Naturelle des Pays-Bas, tome ix. Catalogue Ostéologique des Mammifères: F. A. Jentink (Brill, Leide).

CONTENTS. PAGE

THURSDAY, JULY 28, 1887.

THE GEOLOGY OF NORTHUMBERLAND AND DURHAM.

Outlines of the Geology of Northumberland and Durham. By Prof. G. A. Lebour. (Newcastle-upon-Tyne: Lambert and Co., 1886.)

THE normal guide-book cannot be said to be as a rule very entertaining reading, and a work like the one before us is essentially a geological guide-book. But the guide-book may be so treated as to present points of interest even to the reader who never puts it to the use for which it was primarily intended. Such a guide-book has been produced by the joint labours of a great poet and a great geologist; and a great historian, when he leads us round towns and cities thick with objects full of historical associations, puts into our hands a guide-book of this type. No one, least of all the author, would for a moment think of placing the unpretending little volume on the geology of Northumberland and Durham in the same class as the books to which allusion has just been made, but it is very curious to note how many questions of interest are started during the perusal of what at first sight looks like nothing more than a rather dry description of local geology. Some of these points may now be noticed.

The fact that peat-lakes have often more than one outlet, though it is coupled by the rather questionable statement that "ordinary lakes with two outlets do not exist," throws light on a much disputed question in physical geography. For two outlets to co-exist for any length of time in a lake, it seems necessary that the outflowing streams should have the same eroding power, and this will be the case if these streams have the same fall, and if their beds are composed of the same material. A lake in Arran which has two outlets is wholly surrounded by granite, and in its case the two conditions mentioned are probably satisfied. The eroding power of the sluggish outflows from a bog must be very small, and the material on which it is exerted is everywhere peat. Here it is easy to realize the possibility of there being several outlets. But there are few cases in which the balance of power could be exactly maintained, and hence lakes with two outlets must be rare.

It is extremely interesting to find a *Lingula* recorded from beds high up in the Coal-measures. New cases of marine bands in the upper portion of the Coal-measures are constantly being brought to light, and each fresh discovery strengthens the belief that the occasional presence of marine forms is not confined to the Lower Coal-measures or Ganister Beds, or is even commoner there than in the Middle Coal-measures. If this be so, the attempts which have been made to draw on palæontological grounds a line between the Lower and Middle Coal-measures, at once fall to the ground. Marine bands seem to be less plentiful in the Coal-measures of Durham and Northumberland than in those of Lancashire and Yorkshire. This accords well with the hypothesis that the outlets which connected the Coal-measure lake or estuary with the open ocean lay to the west. It was through these openings that marine forms now and again migrated into the area, and the further a spot was from the door of entry, the fewer would be the immigrants which reached it.

Under the head of "Millstone Grit" we are told that the rocks, which in Lancashire and Yorkshire are conspicuous under this name, are in Northumberland in no wise distinguished from the Coal-measures proper, that they have no distinctive fossils—in short, nothing peculiar to them but their position. But the contrast between Lancashire and Yorkshire on the one hand, and Northumberland on the other, is really by no means so great as these words would seem to imply. It is true that in the first-named and adjacent counties a portion of the Carboniferous rocks has certain lithological peculiarities so strongly marked, that it is convenient to separate it from the beds above and call it Millstone Grit, but the distinction rests solely on the comparatively unimportant points of coarseness of grain and massiveness, and when we look to points of real importance, such as conditions of deposition, fossils, and the like, Millstone Grit, Ganister Beds, Coal-measures, and other similar groups are seen to be arbitrary, though very convenient, subdivisions of a formation that is essentially one from top to bottom.

Prof. Lebour has happily seized on the only line of demarcation among the Carboniferous rocks which can have any real significance; that, namely, which separates rocks in which the fossils are all practically marine from rocks in which marine fossils are the exception, and in which they are the *exuviæ* of marine creatures which paid occasional visits to the area, but whose stay there was short.

And this brings us to the lower and marine portion of the Carboniferous system, which is divided in the present work into two members, named respectively the Bernician and the Tuedian. The contrast between the Mountain Limestone of Derbyshire and Yorkshire, almost pure limestone from top to bottom, and the beds in the south of Scotland, which we must look upon as its time-equivalents—shales and limestones in which it is often difficult to find limestones at all, and more difficult still to recognize them when they are come across—this contrast has become one of the hackneyed instances of geology. The name Bernician is applied by Prof. Lebour to beds on the same geological horizon in the north of England. They are in a general way intermediate in character between their equivalents on the north and on the south; but in a work intended mainly for the use of young students the author has wisely warned his readers that they will not find in Nature that regularity and uniformity of change that some geological diagrams might lead them to expect. The limestones are not all wedges with their sharp edges pointing north, and, moreover, the total thickness both of the whole group and of its various subdivisions varies very much from place to place. We would suggest, in the interest of those students who have not yet got beyond books, that it be pointed out in the next edition that this is only what was to be expected; that in a subsiding area it is almost certain that sinking will go on faster at some spots than at others; that pits and holes will be thus formed in the sea-bed; and that in a deposit laid down under such conditions great variations in thickness and character must necessarily arise. It would not be amiss to call attention also to the bearing such considerations have on the attempts which are occasionally made after minute correlations and identifications of individual beds in such a group of strata. The example set in this matter in the present work is excellent, for a clear distinction is drawn between those limestones or other beds about whose continuity there can be no question, and those whose occurrence is local; but example may be usefully enforced by precept.

The same wholesome refusal to draw hard and fast lines where none have been drawn by Nature is seen when we come to the chapter on the Tuedian beds. Special attention is drawn to the fact that, though these can be separated, as far as lithological character goes, from the overlying Bernicians, the line of demarcation is by no means everywhere of the same geological age. The Tuedian beds resemble so closely the "Cement Stone Group" of the central valley of Scotland, that they are doubtless the southern continuation of that subdivision. The Scotch beds, as is well known, were laid down in an assemblage of ponds, creeks, and lagoons separated by banks of sand and muddy shoals. The Tuedians of the north of England do not seem to show quite such rapid changes horizontally as are common in their Scotch equivalents, but they must have been formed under very similar conditions. Beds thoroughly Tuedian in character occur on the west of the Cross Fell Range near Shap: they are very thin, and we are there probably close to the southern boundary of the area over which these peculiar beds were laid down.

Very interesting are the accounts of the somewhat peculiar group of rocks discovered in the deep borings for rock salt alongside the Tees. First came more than 1000 feet of Red Marls and Sandstones, which may be very safely assigned to the Trias. Judging by what is seen at the outcrop, we should have expected the main mass of the Magnesian Limestone to follow; but such was not the case. The hole then entered a group of rocks consisting of gypsum, anhydrite, rock salt, and beds of limestone. Prof. Lebour is of opinion that the Magnesian Limestone was not reached by any of the holes. Hereby several questions may be started. Are the 1000 feet of red marl and sandstones to be assigned to the Red Marl or the Red Sandstone? A nice difficulty for the system-mongers; but before we try to solve it, we may ask whether these two subdivisions are as sharply marked off from each other in Yorkshire and Durham as in other parts of England. There is no reason why they should be; and if they are not, we may well content ourselves with calling the whole Trias. Then how are we to account for the presence of the rock salt and gypsum, which, as far as is known, is never seen along the outcrop, or indeed anywhere else in England? It seems likely that towards the end of the Permian period unequal subsidence produced hereabouts a depression in the bed of the water; that, as now happens elsewhere under similar conditions, the Permian lake became largely laid dry, so that water remained only in this and perhaps other similar basins; and that, from the highly concentrated solutions which remained in these lakelets, local deposits of a strongly chemical character were precipitated. The author remarks on the close resemblance which these deposits bear to the subdivision of the Permian called "Rauchwacke" in Germany, and would apply this name to them. That they and the German "Rauchwacke" were formed under very similar conditions there can be little doubt, but there is no proof that the two were formed at the same time, and this is almost necessarily implied if we give them the same name. In a group of rocks like the Permian, formed in so many distinct basins, and under changing conditions the order and nature of which were probably never the same in any two basins, the minor subdivisions must necessarily be totally different in different areas, and any attempt to correlate these minor subdivisions can be little better than guess-work. If the subdivisions are to have distinctive names, it seems better that the beds of each basin should each have a set of names to itself. Similar objections apply to the habit of designating the subdivisions of the English New Red Sandstone by German names; it is the practice to look upon the New Red Marl as the time-equivalent of the Keuper, and the New Red Sandstone as that of the Bunter, but there is absolutely no proof of this. It is worse when a statement is made that the Muschelkalk is absent in England, and a fictitious unconformity is postulated between the New Red Marl and the New Red Sandstone to account for its absence. Who can say whether the lower part of the New Red Marl, or the upper part of the New Red Sandstone, or both, were not forming here while the Muschelkalk was being deposited in Germany?

The peculiarities of structure exhibited by the Magnesian Limestone are shortly but clearly described. They have been long known, but little has been done towards explaining how they were produced. The problem is one of extreme complexity, but a persevering attack on it, even if it did not lead to a complete solution, would almost certainly throw great light on what we in our ignorance call concretionary action. Sundry structures, formerly referred to this mysterious cause, have been shown to be due to simpler and less recondite processes, but there is a large residuum of cases still awaiting explanation.

We have by no means exhausted all the questions and suggestions which this little book will prompt; but we hope we have said enough to show that it will prove to the attentive reader far more interesting than might at first be supposed. And we may learn from it that in Great Britain, the very motherland of geology proper, ransacked as it has been for now well-nigh a century by the ablest of geologists, there is still many a corner, full of unsolved problems, awaiting the attention of those geologists who cannot wander far from home but are yet anxious to win their spurs.

A. H. GREEN.

PHYSIOLOGICAL PSYCHOLOGY.

Elements of Physiological Psychology. A Treatise on the Activities and Nature of the Mind from the Physical and Experimental Point of View. By George T. Ladd, Professor of Philosophy in Yale University. (London: Longmans, Green, and Co., 1887.)

THE aim of this volume is twofold: first, to give a clear, accurate, and up-to-date account of the psycho-physiology of man; and then to enter a protest against a merely materialistic interpretation of the phenomena. Such a protest, coming from one who is well abreast of modern physiology, is likely to carry weight which could not but be lacking to the opposition of a thorough-going disciple of the "old psychology" school. No one can say that Mr. Ladd's conclusions are reached in and through his ignorance of the real nature and value of the facts on which materialists base their arguments.

The work consists of a short introduction and three parts, of which the first deals with "The Nervous Mechanism," the second with "The Correlations of the Nervous Mechanism and the Mind," and the third with "The Nature of the Mind."

The first part opens with a chapter on the elements of the nervous system, and then proceeds to show how these elements are combined into a systematic whole. The morphology of the nervous mechanism having thus been described, its general physiology is dealt with in the next two chapters, and the automatic and reflex-motor functions of the central mechanism are successively brought under review until, in ascending order, we reach the cerebral hemispheres, the special functions of which are reserved for the second part. The reasons the author gives for adopting this plan are: (1) that nothing is known as to the molecular structure of these hemispheres, or as to their automatic and reflex-motor centres and activities which adds anything of importance to the description of the nervous system as a mechanism or to the mechanical theory of its action; and (2) that the correlations which exist between the structural condition or physiological function of the nervous system and the phenomena of mind are chiefly (if not wholly) capable of study as illustrated in the cerebral hemispheres. An important chapter on the end-organs of the nervous system then follows, and is succeeded by one on development. A concluding chapter in this part is devoted to the mechanical theory of the nervous system, in which, while the author admits that the changes which take place in the brain are essentially the same as those with which the science of molecular physics has elsewhere to deal, he reaches the conclusion that "it cannot be pretended that even a beginning has been made toward a satisfactory theory of the functional activity of the central organs considered as a special case in molecular physics."

In this part, together with much that is familiar, there is not a little that has hitherto seen the light only in scattered publications.

The second part opens with two long chapters on the localization of cerebral functions. The experiments and conclusions of Fritsch and Hitzig, Exner, Ferrier, Munk, Goltz, and others are carefully described and considered. The conclusions to which the author is led by his review of these labours are as follow:—

"Three principles may be laid down as summing up the results reached by inference upon the basis of experiment with respect to the localization of function in the cerebral cortex. The first principle is to be accepted in the form of a general postulate derived from a study of the other parts of the nervous system, and confirmed on attempting to apply it to the cerebral hemispheres. It may be stated as follows: the different elementary parts of the nervous system are all capable of performing its different specific functions when, and only when, they have been brought into the proper connexions and have been exercised in the performance of those functions. This principle includes two important laws which, we know, hold good throughout the nervous mechanism, and which lie at the physical basis of important psychical facts and laws; they are the *law of Specific Energy* and the *law of Habit*. The remaining two of the three principles alluded to above may be said to follow from the first: they are the principle of *localized function* and the principle of *substitution*. The former asserts that, in the normal condition of the nervous system, all parts have not the same definite functions. Everything in both its anatomy and physiology indicates that the principle of localized function does apply, in some sort, to the cerebral hemispheres. So-called 'centres,' however, are in no case to be regarded as portions of the nervous substance that can be marked off by fixed lines for the confinement of definite functions within rigid limits. These areas are somewhat different for different brains of the same species; they widen when a heightened energy is demanded of them; their centres are neither mathematical points nor very minute collections of cells. Nor are these areas perfectly isolated localities; on the contrary, they obviously overlap each other in certain cases. Furthermore, the functions of the cerebrum are not absolutely confined to those centres with which, under ordinary circumstances, they are chiefly or wholly connected; in which, that is to say, they are localized. If such centres, for any reason, become incapacitated or relatively unfitted to perform their normal functions, the same functions may be performed by other areas of the cerebral cortex, provided these areas also stand in the proper connexion. This is the principle of substitution."

In the remaining chapters of the second part we are led to consider sensations in their qualitative and quantitative relations, the nature of "things," or the presentations of sense (which introduces us to "space-form"), and the time-relations of mental phenomena (which introduces us to "time-form"). Then we are conducted, through feeling, to the higher faculties—memory, will, conception—the physiological basis of which is sought in vain. The concluding chapter of this part gives a summary of the general correlations of body and mind. The author seems to intend that two points shall stand out prominently: first, the essentially synthetic activity of the mind in constructing those presentations of sense which we call things or objects; and secondly, notwithstanding a vague correlation, the inconceivability of any physiological basis thereof. "For that spiritual activity which actually *puts together* in consciousness the sensations, psycho-physics cannot even suggest the beginning of a physical explanation." And again: "When we speak of a physical basis of memory, recognition must be made of the complete inability of science to suggest any physical process which can be conceived of as correlated with that peculiar and mysterious *actus* of the mind, connecting its present and its past, which constitutes the essence of memory."

In the third part the conclusion to which especial prominence is given is this: that the subject of all the states of consciousness is a real unit-being, called mind; which is of non-material nature, and acts and develops according to laws of its own, but is specially correlated with certain molecules and masses forming the substance of the brain. The nature of this correlation is considered at length. To speak of mental states and processes as the "product" of the nervous mass of the brain in any sense of the word corresponding to that which we rightly apply to the various secretions of the body, involves us at once, it is held, in the grossest absurdities; while the theory that claims that *all* mental phenomena, whatever their varied characteristic shading, have exact equivalents, as it were, in specific forms of the nerve-commotion of the living brain is marked by its "surprising audacity." "Standing on a slender basis of real fact, it makes a leap into the dark which carries it centuries in advance of where the light of modern research is now clearly shining." The author, however, by no means rejects, he strongly contends for, a causal nexus as existing between brain and mind. He regards the term organ (or instrument) of the mind, as applied to the body, as particularly calculated to emphasize the relation of the ideas and volitions which arise in consciousness to the control of the muscular apparatus. He will have nothing to do with monism, but contends that psycho-physical science, simply observing the facts and building on them, establishes the dualism of brain and mind. "We affirm, then," he says, "that we are entitled to say: The changes of the brain are a *cause* of the states of consciousness; and the mind behaves as it does behave, *because* of the behaviour of the molecules of the brain." "We affirm also that we are equally entitled to say: The states of consciousness are a cause of the molecular condition and changes of the nervous mass of the brain, and through it of the other tissues and organs of the body."

So far, in dealing with the third part, we have perhaps made it appear that, in the author's view, the correlation is complete. And the passages we have quoted seem to justify this view. But many other passages reject such an interpretation with scorn. "In investigating the correlations which undoubtedly exist between the nervous mechanism and the phenomena of consciousness, it is found that some of these phenomena imply activities of the mind which do not admit, in any sense of the word, of being thus correlated." "Judgment itself is a form of mental phenomena for the essential part of which no physical equivalent can be discovered or even conceived of." "To account for this boundless expansion of the activities of consciousness (in the early years of childhood), with its surprising new factors and mysterious grounds of synthesis and assumption, by proposing an hypothesis of 'dynamical associations' among the particles of nervous substance in the brain, is a deification of impotency." "Not one of the higher acts of feeling, knowing, or willing, so far as its *sui generis* character is concerned, admits of being correlated with, or represented under, any of the conceivable modes of the motion and relation of molecules of nervous substance."

It would seem, then, that the author plays rather fast and loose with this correlation, as indeed is apt to be the fashion with dualists. We doubt whether he is justified in saying that psycho-physical science establishes the dualism of brain and mind. Here, it seems to us, the writer's usual caution forsakes him. Idealism, materialism, occasionalism, dualism, monism, are none of them theories that are in any likelihood of being "established" for many a long day. They are of the nature of *beliefs*; and strong as is his advocacy of the dualistic creed the author falls into error if he dreams of its speedy establishment. We could wish that he had squarely faced the difficulties which the acceptance of the dualistic hypothesis entails, a few of which are but barely mentioned on page 597. These and many others may not be difficulties to him; but surely he who would establish a doctrine should meet half-way such difficulties as are likely to trouble unbelievers. We could wish, too, that he had given us a more detailed criticism of the monistic creed which he rejects. To ask *why* the double-faced unity (the human being) manifests itself both in physical and mental states—"one being, in two wholly incomparable modes of manifestation"—and to say that monism has to undertake the task of showing *how* the one reality can appear under these two phenomenal forms of being—matter and mind—is surely not a very powerful or acute criticism. There are many *hows* and *whys* which can only be answered by quietly pointing to the facts. We do not say that monism can in this way be "established"; but we regard the criticism as weak.

Nor are we impressed with the force of the argument upon which so much stress is laid, that for certain higher mental activities no physiological correlate can be conceived. It seems to us that, if anywhere, the inconceivability comes in at the very beginning. If once the conceivability of a correlation between a nerve-commotion of any kind and a state of consciousness be admitted, there need be no further talk of inconceivability in the matter. *There* lies the rub: elsewhere we only find questions of degree and of relative complexity.

We cannot take leave of this valuable and important work without expressing our sense of its ability, its thoroughness, and its candour. There is no other book in the English language that covers its ground.

C. Ll. M.

OUR BOOK SHELF.

The Essentials of Histology. By E. A. Schäfer, F.R.S. Second Edition. (London: Longmans, Green, and Co., 1887.)

THIS edition is, in several respects, an improvement on the first. The volume is less bulky, and there are some useful additions to the text so as to bring this up to date, especially as regards the methods of histological study. There are seventeen valuable illustrations added. The omission in the first edition of references to the authors of the illustrations has, we are glad to see, been corrected in this edition.

On the whole, we think the book a clear exposition of the present state of human histology, and, as such, it will prove useful to students and teachers. E. KLEIN.

Aluminium: its History, Occurrence, Properties, Metallurgy, and Applications, including its Alloys. By Joseph W. Richards. 12mo, pp. 346 (Philadelphia: Band. London: Sampson Low and Co. 1887.)

THIS volume is mainly a compilation based upon the late H. St. Claire Deville's treatise published in 1858, and the newer work by Dr. Mierzinski in Hartleben's "Chemisch-Technische Bibliothek," which appeared in 1883. As no special work on aluminium had previously appeared in English, we agree with the author that no apology is necessary in presenting it. The subject has been systematically treated both from the scientific and technical points of view, and as regards the latter the information has been brought up to date by including notices of Webster's improvements in the Deville process, Messrs. Cowles Brothers' electrolytic method of producing aluminium alloys, and the Castner process of reducing sodium from caustic soda at a low temperature, which, in conjunction with Webster's processes, seems likely to render the production of cheap aluminium commercially possible.

The author has contributed to the appendix a series of experiments made by himself on the formation and reduction of aluminium sulphide, which are of interest, although the results, in the reduction experiments at any rate, appear to have been mainly negative. Iron, tin, copper, and antimony were employed as reducing agents, but only with the first two metals was any reduction effected. The concluding paragraph, therefore, reads rather oddly:—"These processes have been covered by patents, but have never been made successful. It appears that if rightly managed they will give good results and produce aluminium alloys cheaply."

Questions on Physics. By Sydney Young, D.Sc., F.C.S., Lecturer on Chemistry, and Tutorial Lecturer on Physics in the University College, Bristol. (London: Rivingtons, 1887.)

ASSUMING that books consisting of a series of questions with their answers collected together at the end supply a legitimate want and do a real service in the cause of scientific education, Dr. Young's "Questions on Physics" is a valuable addition to those already existing. It is as free as it is possible to make such a book from the charge of encouraging "cram," as the questions are many of them not adapted to rule-of-thumb methods of solution. Many of them also are descriptive of some instrument or principle, in which case, of course, answers are not given. The author takes in succession mechanics, acoustics, heat, magnetism, electricity, and optics. After the answers he gives a series of tables which will be found useful. There are no questions on moment of inertia, or on the ballistic galvanometer. One sentence—the last part of question 155—may vex the student: "Calculate the focal length of a concave lens which gives a magnification of three diameters at a distance of three inches."

The book is intended for the intermediate examination in science and preliminary scientific examination of the London University.

Eminent Naturalists. By Thomas Greenwood, F.R.G.S. (London: Simpkin, Marshall, and Co., 1886.)

THIS is a little book (200 small 8vo pages) intended, as the preface says, to furnish "short yet comprehensive sketches of some leading naturalists." The sketches are certainly "short," but can only be said to be "comprehensive" in the sense that this term may be applied to an epitaph. It is difficult to understand what object such very sketchy biographical sketches can be supposed to serve. Moreover, in this case the subjects appear to have been selected at random; the result being that the portrait gallery, such as it is, presents a somewhat incongruous assemblage—to wit, Linnæus, Lubbock, Thomas Edward, Louis Agassiz, Cuvier, Buffon, Lyell, and Murchison. Whether this curious arrangement is intended to express the writer's idea of the order of merit of these men, or whether, like his choice of naturalists, it is purely haphazard, we are not informed. But surely, if a biographer goes back as far as Linnæus for his material, and carries down his survey to the present generation, even the most popular of popular readers might have expected him to supply a less deficient index of "eminent naturalists."

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. No notice is taken of anonymous communications.*]

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Carnatic Rainfall.

IF I have rightly interpreted General Strachey's courteous criticism of my paper on the Carnatic rainfall, the gist of his objections may be summed up by saying that the method by which I endeavoured to estimate numerically the genuineness of the apparent cyclical variation of that rainfall, as a recurrent phenomenon, is logically invalid. This, I must frankly admit, is really the case; my error has been somewhat of the nature of a *petitio principii*, and is indefensible. I have reasoned upon a series of values directly obtained from the observations, as if they had been obtained deductively from some independent source, and had been found, on trial, to agree, within certain allowable limits, with the results of the observations. This procedure, as General Strachey has shown, is manifestly illogical; and the inferred "high probability that the apparent undecennial fluctuation of the Carnatic rainfall is no chance phenomenon," in so far as this conclusion depends on the above erroneous reasoning, necessarily falls to the ground. But only in so far. The validity of the data afforded by the registers remains, of course, unaffected; and these data, as they stand, seem to me to furnish evidence of so pronounced a character that it is at least improbable that the apparent fluctuation is fortuitous. The considerations on which I base this opinion are:—

(1) That each series of eleven years, taken separately, shows a dominant fluctuation of that period, and these fluctuations show much accordance, both in their ranges and in the epochs of their critical phases. Simple inspection of the tabulated annual means is sufficient to convince one that there is no regular fluctuation of anything like the same magnitude, differing much from the eleven-year period.

(2) The range of the fluctuation as educed by the harmonic formula (restricted to two periodical terms), is four times as great as the mean deviation of the recorded amounts from the corresponding computed values. And this fact fulfils a condition which, in a less rigorous form, General Strachey suggested, I believe,[1] as a test in his discussion of the Madras rainfall registers, communicated to the Royal Society in May 1877, and the failure of which he rightly assigned as a reason for doubting the reality of the supposed cyclical fluctuation of the Madras rainfall.

That the second of these considerations is valid has been established in my former communication. The computed range of the fluctuation was shown to be 14 inches, and the mean annual deviation of the observed from the computed values ± 3·5 inches. To render the first more obvious, I have computed the harmonic constants, separately, from each of the two undecennial series, and therefrom the annual values in each case. The constants are:—

1st Cycle.		2nd Cycle.	
$u' = 7{\cdot}23$ ...	$u'' = 0{\cdot}66$	$u' = 4{\cdot}22$ ...	$u'' = 5{\cdot}44$
$U' = 190° 16'$ ...	$U'' = 322° 10'$	$U' = 233° 59'$ ...	$U'' = 240° 14'$

and the computed annual values—

1st Cycle.	Inches.	2nd Cycle.	Inches.
1864	− 1·70	1875	− 8·14
1865	− 4·62	1876	− 8·63
1866	− 6·35	1877	− 2·61
1867	− 6·62	1878	+ 3·46
1868	− 4·99	1879	+ 3·69
1869	− 1·39	1880	+ 0·07
1870	+ 3·18	1881	− 1·46
1871	+ 6·79	1882	+ 2·11
1872	+ 7·23	1883	+ 6·71
1873	+ 5·81	1884	+ 5·97
1874	+ 2·12	1885	− 1·04

[1] I quote from memory, not having the Proc. Roy. Soc. at hand.

the average annual rainfall being, as before stated, about 35 inches.

These figures have, in themselves, as General Strachey truly observes, no physical signification, but they show that there is a very pronounced harmonic element, with a period of eleven years, underlying the observed quantities, and that in some of its most salient features it seems to be recurrent. Physical considerations only come in when, and in so far as, its features can be correlated with those of the solar variations; a point already noticed in my former paper, and on which I need say nothing further. But of course it is the supposed connexion of the two classes of phenomena that constitutes the chief interest of the subject under discussion. HENRY F. BLANFORD.

Folkestone, July 25.

The Progress of the Scottish Universities.

YOUR issue of July 14 (p. 252) set forth in vivid graph the rapid increase in size of the Scottish Universities. But as we must not forget that in progress, advance of type or improvement in quality is more important than increase of quantity, it behoves us to test the qualitative change of the Scottish Universities, and to make sure that they are not of the nature of malignant tumours—rapidly-growing masses with tissues of an embryonic type.

The test is not hard to find in the case of organisms with a unction so definite as the Universities. Increased efficiency

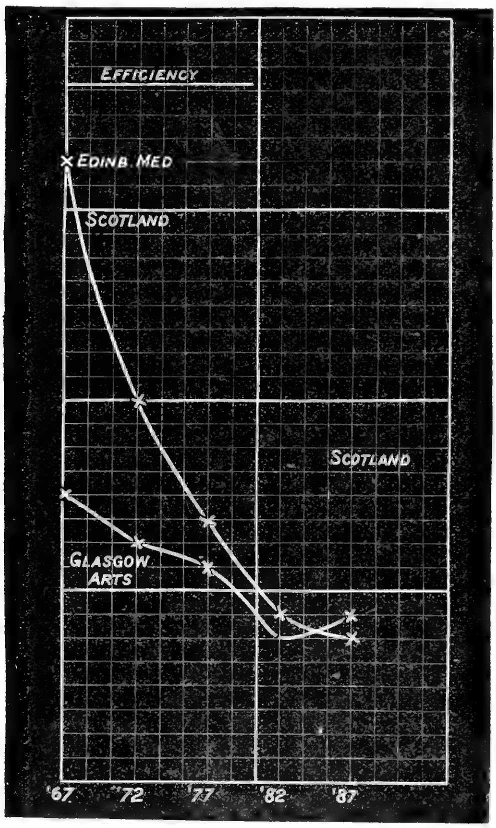


FIG. 1.—Efficiency.

and decreased cost must be the tests, and the results are startling, as shown by the accompanying graphs of the official returns.

The first shows the efficiency in the Arts Faculty in Glasgow, the Medical Faculty in Edinburgh, and for two points the whole of Scotland as tested by the fraction $\frac{\text{Professors}}{\text{Students}}$.

The second shows the quantity, in seconds, of Professor of Anatomy which the students can have for £1 in Edinburgh.

The result is an entire reversal of the usual optimistic picture of progress by growth in quantity, and as I am both hopeful and

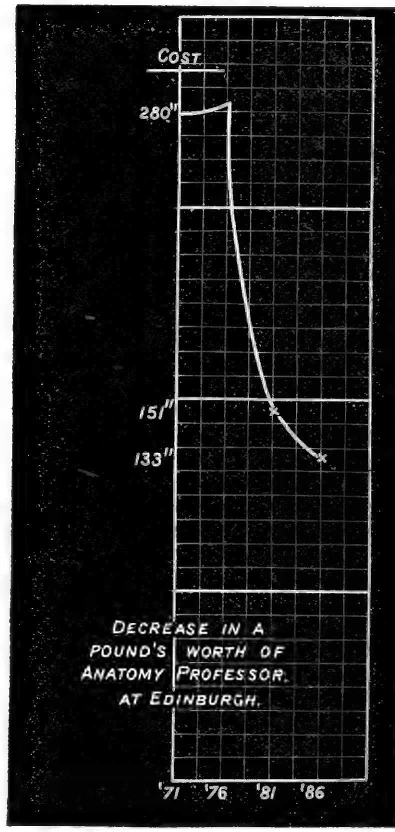


FIG. 2.—Cost.

anxious for the advance in quality of the Universities in which I have spent many years, I hope you will allow me to call attention to its urgency. M.A. ET MEDICUS.

Floating Eggs.

THE floating eggs which a correspondent in NATURE of July 14 (p. 245) describes and refers to *Orthagoriscus*, are apparently those of the angler or frog-fish (*Lophius piscatorius*), which are known to naturalists. They are laid, as Agassiz states (Proc. Amer. Acad. Arts and Sci., vol. xvii. part iii. p. 280), "embedded in an immense ribbon-shaped band, from 2 to 3 feet broad, and from 25 to 30 feet long." The ova of *Orthagoriscus* do not appear to have been yet obtained, and Mr. Green's description accords essentially with the features presented by the eggs of *Lophius*, though no colour is mentioned, whereas the eggs of the frog-fish are of a light violet-gray tint, and when the dark pigment develops in the young embryos the band assumes a blackish hue resembling crape. Examples, I may add, have been obtained on the west coast of Scotland; but, though *Lophius* is extremely abundant at St. Andrews, and on the east coast generally, the ova have not been procured here, as yet. EDWARD E. PRINCE.

St. Andrews Marine Laboratory, Scotland, July 16.

Expression of the Emotions.

IN reading the very interesting letter of "J. M. H." (NATURE, July 14, p. 244), I was much struck with the similarity of purpose and singularity of expression in the robin and in a cat of mine, of which can equally be said, it "invented a note by which it called me to feed it. It was quite peculiar—hushed, short, and muttered, as it were." This note is also used on other occasions,

when searching for me, or when exceeding joyous or high-spirited. It is a kind of "crowing," and quite distinct from purring. Darwin, in his "Expression of the Emotions," does not mention it. Is it exceptional? J. L.
Driffield.

EDUCATION IN AMERICA.[1]

IN the Report of the American Commissioner of Education it is shown that the stimulating influence of the educational exhibits and conferences that formed a feature of the New Orleans Exposition is manifest in almost every department of education. A special Circular of Information respecting the Exposition is in preparation by General Eaton, who is at the head of this wide system. His successive Reports are mines of educational wealth; they have aroused and stimulated educational workers everywhere.

In the collection of essays included in Parts II. and III. of "Educational Exhibits and Conventions," many educational subjects are dealt with by specialists. It is claimed on behalf of Massachusetts that it published the first periodical in the English language devoted to the advancement of education, viz. the *American Journal of Education*, started January 1, 1826. The very broad views it set out with are still urged in the United States: that education should be regarded as the means of fitting man for the discharge of all his duties, and that it accordingly includes much that is generally left to home influence. The editor of the *New England Journal of Education* now carrying on the work there observes that "a history of educational journalism in New England culminates in *Barnard's Journal of Education*, full of instruction as to systems, institutions public and private, technical and special schools, history, biography, philosophy, &c., &c." The annual Reports of Horace Mann are "the very gospel of the new education, and are found in the libraries of every country," and the acts of this apostle form an interesting chapter here. It is to be observed that in this model State, where less than two-fifths of 1 per cent. of its native children belong to the illiterate class, no technical education is supplied in the State schools, the old aim of widening the scholar's mind being preferred to that of imparting information. A "noble showing," observes General Eaton, though this last Report records a very small falling off.

Massachusetts is, however, far ahead of some other States. With all the matter for congratulation which follows, and although education is so popular that in Texas the enforcement of it among the few who need compulsory measures may be placed in the hands of the police, surveyors have to own to an increase of even the *proportion* of ignorance in the United States, which is nearly as alarming as ever it was in England. Over 2,000,000 voters, one in every five, are unable to read the ballots which they cast. As an effort to meet this illiteracy, it was suggested in the Congress of Educators that some 65,000,000 dollars should be allowed to various States from national funds, and the proposition of Dr. T. W. Bicknell, of Boston, was that the money should be allotted in proportion to the number of illiterates in every State between the ages of ten and twenty years, diminishing from four dollars a head for the first three years, to one dollar the tenth, eleventh, and twelfth years, when all such illiteracy ought to be overcome.

The highest class of education does not seem to be gaining ground. Dr. Payne, of the Ohio Wesleyan University, urged the importance of increased College education, and of the personal example and influence of high-class teachers, calling attention to the fact that not 15 per cent. of either doctors or lawyers in the United States are graduates of any University. He explains this unpopularity of College education by the great length of time which is given there to unpractical classics, which might easily be made familiar in a shorter way, more economical of time. He asserts that two years and a half might be made sufficient for the work done in a College in four years. Clever and painstaking pupils are yoked together with the idle and stupid; and the same energy and thrift of time by which the former would attain this result makes them reject a University education altogether. A similar reform is required with the object of economy in expenses.

This same "commercial spirit of the age," Prof. Garnett laments, has caused the number of pupils in the University of Virginia to fall off during the last twenty years. This institution is divided into nineteen distinct schools, and each pupil chooses from which he will make up his course of studies. Each school gives a certificate of proficiency or a diploma of graduation, and the University gives the various titles of Bachelor or Doctor of Letters or of Science, of Philosophy or of Arts: also of Bachelor of Law or of Scientific Agriculture; of Doctor of Medicine; of Civil Engineer and of Mining Engineer. With the same desire for higher results, also, Colonel W. P. Johnston, President of the University endowed by "that princely benefactor of education in Louisiana, Paul Tulane," under the roof of which the papers were read, urged the need of a University doing what it could, if it could not do what it would. Much work, he pleaded, was required in a Louisiana University that a German University would reject.

However, General Eaton remarks that 1884-85 was characterized by great activity in all departments of College and University work, and by full and earnest discussion of important questions pertaining to the conduct and development of these institutions, and especially as to the separate functions of Colleges and Universities. Apparently many enthusiasts have convinced themselves that the teacher now stands, not only in the place of the parent, but also of the State and of all guiding influences. Other writers here, besides General Eaton, Canadian as well as those of the United States, describe education as if the school-master would soon have the entire bringing up of the young, starting from the kindergarten school, superintending their games as well as their studies, and maintaining a hold over them till the technical school, seen already to be very near by General Eaton, has turned them out self-supporting citizens. Doubtless the wonderful division of labour and of knowledge into special departments makes it possible for teachers to bring up children with more science than formerly; but surely the human race cannot afford to release parents from the duties which fall so naturally to them, and to waste the zeal and enthusiasm with which mothers especially enter upon these duties. General Eaton has much to say about the responsibilities of teachers. Dr. Mayo cautioned them that the United States were determined to have the best of everything. As the old coaches had been superseded by the Pullman cars, so inferior teachers must make way for superior. But to read these enthusiastic educationalists' views of the duties of teachers, "the burden laid upon them seems greater than they can bear."

None urge the almost boundless importance and dignity of the office of teacher of the young with such fervour and consistency as Brothers Maurelian, Justin, and Noah, of the Christian School. All that they say is quite true except the idea that the ordinary assistant is able to judge of and then to guide the character of every child under his care. It is more than "fond" parents can do for their own children even; and happy must the child be who finds a teacher more devoted than its own parents!

[1] "Educational Exhibits and Conventions at the World's Industrial and Cotton Centennial Exposition, New Orleans, 1884-85." Part II. Proceedings of the International Congress of Educators. Part III. Proceedings of the Department of Superintendence of the National Education Association, and Addresses delivered on Education Days. (Washington: Government Printing Office, 1886.)
"Report of the Commissioner of Education for the Year 1884-85." (Washington: Government Printing Office, 1886.)

Nowhere is the importance of high-class teachers better understood than among the Japanese. A short address given here by their Commissioner describes their eager search after European knowledge for several generations before the present reformed Government came into power, and now the rule is that all employed in instruction—normal teachers at the end of seven years, ordinary teachers at the end of five—must be re-examined to ascertain whether they are keeping up with the progress of the age. But great efforts are made to render the profession in every way attractive. Teachers are exempt from military conscription. Titles, quasi-offices, and ranks are given to them, so that the profession may not be treated as a low or unimportant one. For a similar encouragement of learning, University men are also freed from military service; and even the students of the middle-class schools are exempt from conscription for six years. One speaker, who had been resident in Japan, but had travelled through Europe, claimed for Tokio also the best Froebel kindergarten that he had ever seen. It would not be surprising if the great experiment referred to above were really tried in Japan—such a system of school work as that described by Prof. Hailmann, competent to supersede home teaching altogether. He rather naïvely remarks that his mother was a natural kindergartner. The kindergarten work is a system of technical instruction in which the scientific teacher undertakes to inculcate systematically what parents have hitherto taught as amateurs. Little science and little system are shown in most homes; in fact the kindergartners complain of home influences thwarting their teaching, and urge that young women should attend their schools to learn how to bring up their own families; and one cannot read the principles laid down for a kindergarten school without feeling how appropriate they are for home rule.. In the case therefore of those who can afford such a training, this seems the most efficient and desirable way of carrying the work out; where, on the other hand, a mother has been debarred from such a training, the school may really supersede her home work with advantage. Kindergarten schools accordingly, from every State, were represented at the New Orleans Exposition. The system can hardly, however, become universal, for each child is to be taught in some different way, according to its character, and it is urged by Mrs. Ogden, "if we must crowd, let us crowd the big children, and not the little ones."

As illustrating the principles of kindergarten schools, Prof. Spring, of the Chautauqua School of Sculpture and Modelling, showed, in an experimental address, how much of science could be illustrated by moulding a lump of clay; affirming that a young child caught at the character of various shapes as quickly as an adult. The Commissioner in his Report remarks a large increase in these schools in 1884-85—28 in Pennsylvania alone, 33 in the south and west. Few are supported at public expense, yet the system has had a marked effect in improving the methods of teaching employed.

Prof. W. Hudson, of Texas, lays it down that the interest which a lad can be induced to take in his lessons is a measure of the extent to which his perception, reason, and judgment will be drawn out. More life and reality can be put into a lesson in natural history or botany, and they are therefore more valuable school subjects, and far more useful, than classics. Such pursuits are interesting in leisure hours also, and will keep him out of the mischief to which unemployed energy is so prone. Many experiments in different schools are reported by General Eaton, but so far the only exercises of this kind that it has been found practicable to bring within the reach of the entire school populations are drawing, clay-modelling, and sewing.

A paper was read by Mr. E. M. Hance, Clerk to the Liverpool School Board, on the experimental science instruction first introduced into English elementary schools by that Board. Colonel W. P. Johnston tried to show that technical education is the most beneficial that can be given to the black population. In, we fear, a rather too hopeful simile he compares these latter to the chosen people educated in all the wisdom of the Egyptians before their return to independence. He trusts that one of their great destinies is to re-people with a civilised race their old continent of Africa. Prof. W. J. Thom also urges a technical education for the Negro—not a high-school education, but a farm-labourer's and domestic servant's training. "Unless they know how to work and how to do work, their destruction seems a natural consequence." He, however, looks forward to the black population reaching ten times its actual number, and its present far more rapid increase than that of the white race renders this probable enough. Presidents Fairchild and Long, on the other hand, think that uniform education will heal the breach between the races: the former predicting that twenty-five years of mixed schools would set coloured men on a full equality with the most eminent whites, and hardly leave a vestige of the present "constitutional ineradicable antipathy," which latter epithet we are inclined to judge from the past history of races gives the truer view. He thinks it is a relic of slavery, and asserts that the objection to mixed schools is, not that the antipathy will injure the schools, but that the schools will annihilate the antipathy and bring about an undesired social equality. Strongly pointing against the above hopeful opinions is General Eaton's reference to a tendency among some trade-unions to exclude coloured citizens from industrial training and employment. He accordingly urges that all parties should promote this industrial training by every means, both on the above account and also as the best preparation of Negroes for new and remunerative occupations which must spring up round them. The religious education of the Negro is becoming a special difficulty, and Prof. Thom fears the spread of Mormonism among a race which has neither tradition, habits and customs, nor reverence for law and religion. One matter to which he calls attention may perhaps be a sign that there are influences telling against the blending of the races, viz. that already there is a divergence of Negro dialect from the standard of the vernacular so great as partially to "destroy the uplifting idealism contained in the English tongue."

A most interesting paper, to an English reader especially, bearing on this matter is an account of the present condition of the Negroes in Jamaica after fifty years of freedom. They have nearly doubled their number in the time, and are in more comfortable circumstances. Their dwellings compare favourably with those of Ireland or Scotland. Improvements are recorded of the island generally, exactly answering to the improvements in an English town during the same time, and all done voluntarily and with far less labour than in the old slave times. If they do not love work, still as much voluntary labour was forthcoming as was required to make a railway, without any difficulty on the part of the contractors. Cambridge Local Examinations are held in the island, and some high honours have been taken. Such a sketch must be set against the dark pictures usually drawn. General Eaton, too, in his Report, we are glad to see, thoroughly endorses the accounts of energetic improvement in education still taking place.

A striking feature of the wide views of their duties and responsibilities which are now making their way among educationalists is well brought out in this compilation. There are careful and interesting papers upon all the physical aspects of education; and much is laid down about bodily exercises and training which, though excellent in itself, seems hardly yet to belong to the department of the schoolmaster. The Commissioner urges in his Report that a gymnasium should be attached to every city system of schools, and quotes Germany's example, followed by Austria after Sadowa. Credit is given also to the Germans for leading the way in ventilating school-rooms scientifically. In 1858 Pettenkofer's method of testing the impurity of the air in a room came into use, and a description is here given of a different simple apparatus for the same purpose. The conclusion is drawn that organic matter in "bad air" is more frequently the dangerous part of it than superabundant carbonic acid. England, while at the higher schools formally ignoring this branch of education, nevertheless really recognises it in the games which make themselves of so much importance at the Universities and the great schools that feed them. Physical training was despised and repressed by the monks of old, who founded these schools, and taught that the body was at enmity with the soul, and that the more the former was weakened the more the latter was strengthened and purified; and if with Mr. Galton we regret that all the softening elements of human nature were eliminated by monastic celibacy, we may also console ourselves that, but for it, many injured constitutions must have been handed down as the result of such tenets. Schoolmasters now know that the difficulties of teaching are immensely increased by any physical disorder, and that an absolute incapacity to learn follows some bodily ailments. Imperceptibly increasing from the sleepiness which follows upon a good dinner comes the dulness caused by the bad air of ill-ventilated rooms. There is a long and full paper on this latter subject prepared for independent publication by the Bureau of Education, of great value but too general in its teaching to be epitomised here.

Another cause of what to thoughtless teachers seems irritating stupidity is partial deafness. Interesting observations upon the varieties of this infirmity among school children have been made by Dr. Sexton, of New York. Careful estimates show that only 5 per cent. of the entire population of the United States have normal hearing. Ten per cent. of pupils have such defective hearing as to make special placing and such like care for them in schools necessary. If a teacher has not made himself fully acquainted with the amount of this deafness, a very slightly deaf pupil will either be liable to be sent to the deaf-and-dumb school, or he will leave the ordinary school in disgust at the teacher's harsh and unfair reprimands. Prof. Graham Bell's audiometer is found to answer well in the work of classifying defective hearers. On behalf of the deaf-mute school Mr. Dobyns boasted that theirs was the only universal language: when he met an educated deaf-mute not only from America but from France, Germany, England, or Japan, he could hold communication with him.

From an examination of about forty thousand cases, a Committee on the subject draws the important conclusion that, while very few pupils indeed are short-sighted when they first enter school, "the number afflicted, and the degree or intensity of the disease, gradually but surely increase through the entire school life, from class to class, from year to year, until, when the Colleges and Universities are reached, in many cases more than half the students are near-sighted." This Committee strongly recommends increased use of the black-board and less use of books. This practice has been found to reduce the amount of myopia to one-half. A Report of a second Committee on the causes of it recommends that the head should not be bent forward too much over a desk. Near-sightedness has increased in Alsace since German letters have been used there. There is a special danger of its being brought on at about fifteen years old, the age of puberty.

While these deficiencies are to be found in so large a proportion of children, however, Mr. Jepsen, teacher of music in New Haven, limits the number of children who have really what is called "no ear" for music to less than 4 per cent., and he urges that it may profitably be taught in a thoroughly scientific way to be familiarly read. The Commissioner has felt the importance of this matter so much that through the co-operation of a Music Teachers' Association the heavily burdened Bureau has been already able to draw up and issue a Circular of Information on the study of music in public schools. It is remarked that singing seems to be despised as a school pursuit in the United States, and to be less popular and more neglected than in England. It is taught that mental culture comes chiefly through the eye; moral culture through the ear and voice. Sounds can be taught to children much more easily than numbers. To read music, again, is as great a superiority over singing it by ear as being able to read is better than having learnt a few pieces by heart.

Bearing upon the same question of classifying children according to their powers is a short paper read by Mr. E. Chadwick, of educational celebrity in England, who urges the economy of dividing the bright children from the dull, so as to educate them in less time—a most desirable arrangement for all parties, where it is practicable.

Two papers, one of them also by Mr. Chadwick, take up the subject of rewards and punishments. Mr. Chadwick protests against the use of the stick, while Prof. Barbour urges first the needlessness, and then the danger, of giving prizes, which may breed a sordid character, supply unworthy and therefore unstable motives. They are, he thinks, of no value at all to any but a very few in each class. But in each case it is necessary to supply a motive which the very young can fully appreciate; some terror must be held over the young transgressor's head, and so long as terror is the motive power, the stick is as fair as any other, with the advantage that each culprit is an example to all who see his discomfort, and the influence upon them is nearly equal to that of being caned themselves. The refined torture of solitary confinement, which is considered less degrading, has not this advantage. In like manner, everything in this world is done for a prize, even if that prize be a "high calling," and school-boys require some outward and visible sign of successful labour, books, marks, or class-places. The grosser methods of marking it might well be dropped as the children get older. But rewards we all strive for, and it is untrue that no higher and wider valuation of knowledge replaces the ambition to take home a prize which first led to a laborious pursuit of it.

W. ODELL.

ABSTRACT OF THE RESULTS OF THE INVESTIGATION OF THE CHARLESTON EARTHQUAKE.[1]

II.

LET us suppose an elastic wave to originate at a point C (Fig. 1) situated at the depth q, below the surface. Let the intensity of the shock (amount of energy per unit of wave-front) at the distance unity from C, be denoted by a. Since the intensity is inversely proportional to the square of the distance, the intensity at the epicentrum would be $\frac{a}{q^2}$. Take any other point on the surface of the earth at the distance x from the epicentre, and connect it with C by the line $Cx = r$. The intensity at any such point will obviously be equal to $\frac{a}{r^2}$. If we denote the intensity by y, we shall then have the equation—

$$y = \frac{a}{r^2} = \frac{a}{q^2 + x^2}.$$

This equation expresses a curve which will serve as a

[1] Paper read before the National Academy of Sciences at Washington. on April 19, 1887, by C. E. Dutton, U.S.A., and Everett Hayden, U.S.N., U.S. Geological Survey. Continued from p. 273.

graphic representation of the way in which the surface intensity varies along a line radiating from the epicentre.

The first noteworthy feature of this curve is the contrast between the rapidity with which the intensity diminishes near the epicentre and the slowness with which it diminishes at remote distances. Thus, at a distance from the epicentre equal to the depth of the focus, the intensity has fallen one-half; at twice this distance it has fallen to one-fifth; and at three times the distance to one-tenth of

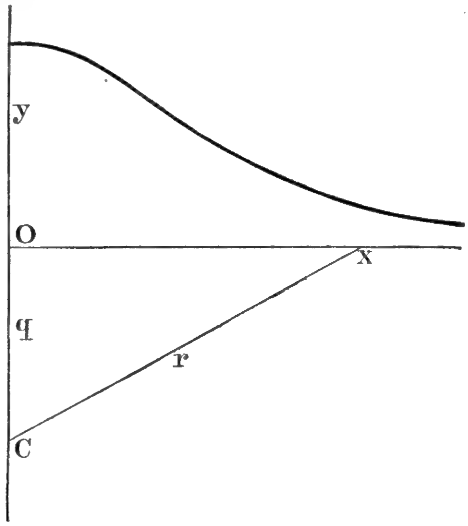


FIG. 1.

the intensity at the epicentre. This suggests at once the possibility of making an approximate estimate of the depth of the focus, based upon the rate at which the intensity of the shock at the surface diminishes in the neighbourhood of the epicentre. If we were able to construct upon any arbitrary scale whatever a series of isoseismal curves around the central parts of the earthquake with an approach to accuracy, this depth would follow at once from the relations of these isoseismals to each other. In the case of a very powerful earthquake in

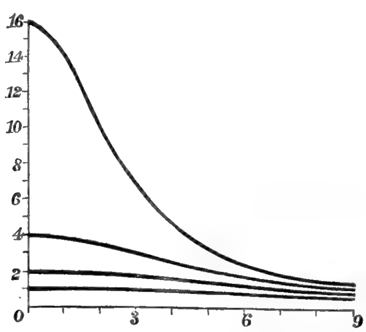


FIG. 2.—Energy constant, depth varying in ratios 1, 2, 3, and 4.

a region which is so flat and uniform in its features as the vicinity of Charleston, this can be done with a rough approach to accuracy.

To appreciate more fully the validity of this mode of reasoning, let us take a series of these intensity curves and vary the values of the constants. And first let us suppose the total energy of the shock measured by the constant, a, remains the same, while the depth of the focus varies. The first series of curves (Fig. 2) will enable us to make a comparison of the effect of two or more shocks of the same total energy but originating at different depths. The intensity at the epicentre being inversely proportional to the square of the depth, the shallower shock would be much more energetic than the deeper one; while at a great distance from the epicentre the two would be approximately equal in their effects. The rate of diminution of intensity would be correspondingly varied, and we might commit large errors in estimating these ratios on the ground, while the error of the depth deduced for the focus would be less than our errors of estimate. In short, the method is not sensitive to small or moderate errors of observation.

The second series of curves (Fig. 3) is conditioned upon the assumption that the depth remains constant while the

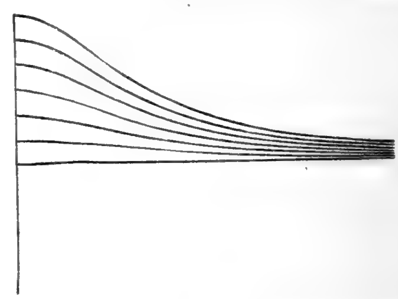


FIG. 3.—Depth constant, energy varying from 1 to 6.

energy of the shock varies. In these curves, the ordinates corresponding to any abscissa are proportional to each other in a simple ratio. In the first series they are proportional to each other in a duplicate ratio.

The third series (Fig. 4) represents the effect of varying both the energy and the depth in such a way that the intensity at the epicentre is constant.

It will appear, therefore, that every shock must have some characteristic intensity curve, depending upon the total energy and the depth below the surface. The intensity at any point along the surface will therefore depend upon these two quantities: energy and depth. It still remains to find some means of discriminating whether the intensity at any point is due to a more energetic

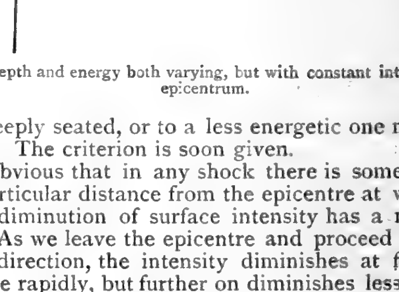


FIG. 4.—Depth and energy both varying, but with constant intensity at the epicentrum.

shock deeply seated, or to a less energetic one nearer the surface. The criterion is soon given.

It is obvious that in any shock there is some point at some particular distance from the epicentre at which the rate of diminution of surface intensity has a maximum value. As we leave the epicentre and proceed outwards in any direction, the intensity diminishes at first more and more rapidly, but further on diminishes less and less rapidly. We wish to find the point at which the rate of decline changes from an increasing to a decreasing rate. In the curve this point is represented at the point of inflexion where the curve ceases to be concave towards the earth, and begins to be convex towards it. To find the co-ordinates of this point we differentiate the equation of the curve twice, and equate the value of the second differential coefficient to zero, and deduce the corresponding value of the abscissa x—

$$\frac{d^2y}{dx^2} = \frac{8ax^2 - 2a(q^2 + x^2)}{(q^2 + x^2)^3} = 0;$$

which equation is satisfied when

$$8ax^2 = 2a(q^2 + x^2),$$

whence

$$\pm x = \frac{q}{\sqrt{3}}.$$

In this value of x, it is seen that the constant, a, has disappeared, and the abscissa of the point of inflexion is therefore independent of the energy of the shock, and dependent upon the depth alone. The meaning of this is that the distance from the epicentre to the point where the rate of decline of the intensity is greatest is simply proportional to the depth of the focus, and is the same whether the energy be greater or less. This property of the intensity curves makes us independent of any absolute standard of measurement of the intensity, and all that we require is to find with reasonable approximation the points where the intensity falls off most rapidly. The depth of the focus follows at once.

The determination of the epicentral tract is chiefly the work of Mr. Earle Sloan, of Charleston, a young civil engineer who immediately after the disaster made an extensive series of observations. In the brief time at his disposal he accumulated a surprisingly large amount of detailed information, and in searching for it exercised a discrimination and sagacity which would have been highly creditable to the most experienced and learned observer. It is to be regretted that his business engagements prevented him from continuing the work. As it is, he has located with considerable precision the epicentral tract, and has furnished data which show well the variation of intensity along several lines radiating from it.

The summary obtained from the examination of Mr. Sloan's data is as follows:—The tract which includes the most forcible action of the earthquake is an elliptical area about twenty-six miles in length, and with a maximum width of about eighteen miles. The major axis of this area is not a straight line, but a curve which is concave towards Charleston, and is situated from fourteen to sixteen miles west and north-west of that city. Along this line there are three points each of which has all the characters of an epicentrum, determined by as many distinct shocks, each having a focus of its own.

Much of the most powerful shock centres in the northernmost focus, though the other two were of sufficient energy to have occasioned great havoc if either of them had occurred alone. The southernmost was also considerably more energetic than the middle one. The distance between the northern and southern epicentres was about twelve miles. Within this tract, except near the edges of it, the motion was most conspicuously of subsultory character, *i.e.* motion in which the vertical component predominated over the horizontal. The marginal portions of this area, where the character of the movement changes, and where the intensity falls off most rapidly, seem to be very well indicated. The positions where the intensity most rapidly declines may be confidently located with an error not exceeding one or two miles on both sides of the epicentres. The South Carolina Railroad crosses the tract in a straight line very near the most forcible seismic vertical. The first point where the intensity falls off with greatest rapidity is near the ninth mile-post, measuring from the railway depot in Charleston, and so well marked upon the ground are the indications of this change, that it seems very improbable that this point is more than a mile distant either way from the precise point we seek to locate. Passing north-westward through Summerville to the opposite side of the tract, we find the corresponding point of most rapid decline in the vicinity of the twenty-third mile-post. This gives us a base-line with which to measure the depth of the focus of the principal shock. The computed depth is twelve miles, with a probable error of one or two miles. The computed depths of the other foci are about the same, but the probable errors are somewhat larger.

In speaking of a focal point of a shock, it must be understood as referring to the centre of all the forces, considered with reference both to amount and direction, which constitute a great seismic impulse. The presumption is that this impulse originates in a large subterranean tract, of which this ideal focus is merely the central point, or nearly so. The form of the subterranean tract may be anything; and, within limits, may have its three dimensions, length, breadth, and thickness, of any magnitude, and bearing any ratios to each other. The form and dimensions of it we cannot of course determine, though it may be possible to obtain some notion of its most general features if the data are sufficient.

This method of computing the depth of a seismic focus is here proposed for the first time. The method employed by Mallet, which consists in finding the angle of emergence of a wave front from the earth by studying the configuration of cracks in buildings is believed to be valueless by nearly all seismologists. There is no definite angle of emergence of the nature he contemplates disclosed at the surface. Certainly in Charleston there was nothing of the kind to be found. The method employed by Seebach is sound in theory, but it requires such extreme accuracy of time determinations that very small errors of time give very large errors in the result. Our own method consists of finding two points on opposite sides of the seismic vertical, at which the changes in seismic action along a given line are most strongly marked. These points ought to be indicated in powerful earthquakes with a fair approach to precision, and the probable errors of determination should not usually exceed one or two tenths of the distance between the two points. The feebler the shock, however, the less is the degree of precision to be expected. Whatever may be the errors in the estimate of this distance, the resulting error in the computed depth is smaller than the error of observation in the ratio of the square root of three to two. How much the estimate may be vitiated by want of homogeneity in the superficial strata we have no means of determining, but we do not believe that it would be so affected to any great extent in such a region as South Carolina. Being independent of any absolute measures either of the surface intensity or of the total energy of the shock, the greatest difficulty of all is at once eliminated. Our opinion of this method is that it is incapable alike of very great precision and of very great errors.

Probably the first thought occurring to anyone examining this method will be that the determination of the two required points would be liable to very large errors. But if he will examine the varying values of the ordinates of the curve corresponding to varying values of the abscissæ, and of the depth, we think he will be satisfied that the limits within which each of the two points of inflexion must fall cannot be wide apart, and that an error in the determination of the base-line greater than two-tenths of its estimated length would, in such a country as Carolina, be very improbable. It will appear that the relations of these variables are such as to restrict the locus within which the desired points are to be found to a very narrow annulus around the epicentrum. We think the method will greatly improve on acquaintance.

We have endeavoured to apply our method of computing the depth of the focus to other earthquakes, but have found difficulty in obtaining anything more than very general results, such as the following:—The depth of the

Charleston earthquake was relatively great, and we find reason for believing that, among those great earthquakes of the last 150 years of whose effects we possess any considerable knowledge, none have originated from a much greater depth, and few from a depth so great. Our reasoning is this:—Very few earthquakes have been felt at a distance from the origin so great as 1000 miles. But the greatest distance at which the tremors are felt is the best measure of the total energy of the shock. On the other hand, the intensity of the Charleston earthquake in the epicentral tract was relatively low in comparison with other great earthquakes. If, then, any shock is more intense at the epicentre, without extending to a greater distance than that of the Charleston earthquake, it is certain that its focus was nearer the surface. This is true of the vast majority of recent earthquakes which have been sufficiently investigated. It is suggested that all estimates of the depth of foci much exceeding that of the Charleston earthquake are in need of re-examination.

The city of Charleston is situated from eight to ten miles outside of the area of maximum intensity, and did not experience its most destructive power. Following the law which we have laid down, the intensity of the shock at Charleston was only three-tenths what it must have been at the epicentrum and about one-third the intensity at Summerville. The diagram showing the long intensity curve stretching from Charleston to a point

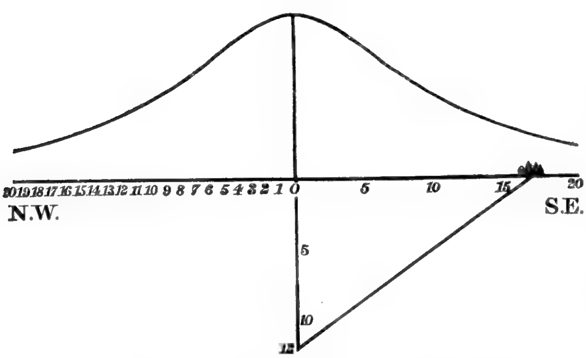


FIG. 5.—Intensity curve of maximum shock twenty miles each side of epicentrum.

forty miles north-west of it will illustrate the position of the city with reference to the varying force of the shock.

Had the seismic centre been ten miles nearer to Charleston, the calamity would have been incomparably greater than it was, and the loss of life would probably have been appalling. Another circumstance greatly broke the force of the shocks. All the coastal region of the Carolinas consists of a series of clays and quicksands, which have been penetrated by artesian borings to a depth of 2000 feet, and which are believed to have a much greater thickness. These beds of loose material no doubt absorbed and extinguished a considerable portion of the energy of the shocks. We have already remarked that a wave passing from firmer and more elastic material into material less firm and elastic produces at first an increased amplitude of wave-motion, which is liable to be more destructive or injurious to buildings. But if the mass of less consistent strata be very great, the reverse result is produced by reason of the rapid extinction of the energy in passing through a considerable length or thickness of very imperfectly elastic material. We cannot but think that Charleston owes in some measure its escape from a still greater calamity to the quicksands beneath the city.

Another aspect of the same fact, if such it be, is found 100 miles west and north-west of Charleston. Here the loosely-aggregated sediments of Tertiary and Cretaceous age which cover the Carolina coastal plain have thinned out, and the crystalline rocks appear at the surface, thinly covered with soil and alluvium. All along the junction of these loose strata and superficial material with the metamorphics the intensity of the shocks was conspicuously greater than to the eastward and southward. The loose covering of these firm rocks is just thick enough to give full effect to the increased amplitude of vibration which occurs when the wave passes from very solid and elastic rocks to those which are less so.

We have also endeavoured to reach some trustworthy estimate of the amplitude of movement at the surface, but the results are meagre and far from satisfactory. The "amplitude of the earth particle" in any earthquake is a question of great practical importance, and it is much to be regretted that no better facilities for determining it can be obtained. There were, however, many occurrences at Charleston bearing upon this question, which are extremely difficult to explain upon any valuation of the amplitude less than 10 inches to a foot. Such amplitudes, however, were most probably limited to spots here and there, while in other spots it was probably much less. That within a small area the amplitude of movement in the surface soil varies between very wide limits seems to be a practically certain conclusion from the observations. In Charleston it appears to have been greatest in the "made ground," where ravines and sloughs were filled up in the early years of the city's history. The structures on higher ground, though severely shaken, did not suffer so much injury.

With regard to the time data from which the speed of propagation must be computed, we are not yet in a position to give final results, but can only state how the problem stands at present. The time reports have been placed in the hands of Profs. Rockwood and Newcomb, with the request that they would scrutinize and discuss them. But neither has been able to finish as yet the task he has so courteously undertaken. Probably the greatest difficulty in the way of determining the speed of propagation arises from the ill-defined character of the disturbance at considerable distances from the origin and from the very considerable duration of it. Wherever a time observation seems to be well authenticated, there still remains in most cases the difficulty of deciding to what particular phase of the earthquake the record refers. And this difficulty is a very serious one. At Summerville the first shock came almost like an explosion. Before people had time to think, they were pitched about like ten-pins. At Charleston there was a perceptible interval estimated at from five to eight seconds from the first note of warning to the maximum of the great shock. At Savannah (ninety miles distant), the interval from the beginning to the first maximum was considerably longer—probably ten to twelve seconds; at Augusta (115 miles), the interval was still greater. And, generally speaking, the greater the distance the more the phenomena were "long drawn out." The duration of the earthquake at Charleston will probably never be known with accuracy. But the general testimony ranges between fifty and ninety seconds. At Washington (450 miles), Prof. Newcomb with his watch in his hand observed a duration of perceptible tremors, with two maxima, lasting about five and a half minutes. Prof. Carpmael's magnetographs recorded the disturbance, and he interprets their photographic traces as showing a duration of about four minutes. Mr. G. W. Holstein, of Belvidere, New Jersey, gives five minutes very nearly as the observed duration. From other localities come well-attested observations showing durations of several minutes, though few of these pretend to give the whole time with any accuracy. This progressive lengthening of the shocks is a well-marked feature of the testimony. The explanation suggests itself at once. The elastic modulus of compression being greater than that of distortion, the speed of the normal waves is the greater while the waves of distortion lag behind.

It is obvious that the phase which it is desired to observe should be the arrival of the first impulses. But the great duration of the tremors has left much doubt on this point. Stopped clocks were plentiful all over the country, but at what phase of the earthquake did they stop? So great, indeed, are the uncertainties on this point that the observations of intelligent men, with watches in their hands measuring a part of the shock and estimating the beginning, are in most cases to be preferred to stopped clocks, even though we know with certainty that the clocks had been accurate to the second. It matters little how we twist and turn the time data: the smallest estimate we can put upon the speed of propagation must prove to be a great surprise to seismologists.

The time at Charleston of the occurrence of the main shock has been fixed at 9h. 15m. 10s. p.m., 75th meridian or Eastern standard time.[1] (All times in this paper, unless otherwise specified, are reduced to that meridian.) The uncertainty does not exceed ten seconds. The beginning of the first tremors at Charleston was from six to eight seconds earlier. The time at Summerville was probably less than four seconds earlier than Charleston. For all localities within 200 miles the time observations are of little value. So swiftly did the waves travel that a small error in the time record gives a very large uncertainty in the resulting speed.

The nearest point which yields a valuable record is Wytheville, Va. (286 miles).[2] Mr. Howard Shriver was sitting at a transit instrument, waiting for the passage of a star, and at once noted the time at 9h. 52m. 37s. (reduced to 75th meridian), giving a speed of about 3·3 miles (5300 metres) per second. There is some slight uncertainty about the precise phase of the shock corresponding to the observation.

The Signal Service Observer at Chattanooga (332 miles) gives only the nearest minute for the principal shock at 9h. 53m., corresponding to a speed of 3·02 miles per second, or 4860 metres.

The best observation in our possession is that of Prof. Simon Newcomb himself, at Washington (450 miles), who gives the time of the beginning of the shock at 9h. 53m. 20s., with an uncertainty not greatly exceeding ten seconds. The resulting speed is 3·46 miles per second, or 5570 metres.

From Baltimore (486 miles), Mr. Richard Randolph, C.E., reports a very intelligent and carefully verified observation of 9h. 53m. 20s. as the beginning of the shock—exactly Prof. Newcomb's time for Washington, giving a speed of 3·74 miles, or 6000 metres, per second.

At Atlantic City, N.J. (552 miles), a large pendulum clock in the Fothergill House stopped at 9h. 54m., very nearly. If this may be taken to be the beginning of the shock, the speed would be 3·26 miles per second, or 5250 metres.

George Wolf Holstein, Belvidere, N.J. (622 miles), gives 9h. 54m. for the beginning of the shock and 9h. 59m. for the end, and compared his watch next morning with the time of the Pennsylvania Railroad. The gradual and uncertain character of the beginning and end would not admit of precise determination to seconds. The speed, taking 9h. 54m. for the beginning, would be 3·66 miles, or 5900 metres.

From New York City (645 miles) and its suburban towns and cities come many reports, all of which give either 9h. 54m. or 9h. 55m. as the nearest minutes. If we take, as a mean, 9h. 54m. 25s. at New York and Brooklyn for the beginning of the shock, the speed would be 3·31 miles, or 5330 metres.

At distances greater than 600 miles the difficulty of associating the time records with particular phases of the shocks becomes very great. In most cases the motion was the swaying movement, with only faint tremors of the rapid kind, and those who felt them were slow in recognizing their character. Readers must form their own opinions as to the degree of approximation to the time of the earliest movements from the following records We give them only as we received them, without attempting any discussion.

J. O. Jacot, watchmaker and jeweller, at Stockbridge, Mass. (772 miles), was sitting by his regulator clock; distinctly recognized the nature of the movement, and noted the time as 9h. 56m. The phase of the shock is uncertain.

At Albany, N.Y. (772 miles), Mr. J. M. Clarke, of the New York State Museum of Natural History, heard the mortar falling down the chimney, and the creaking and straining of the building. As soon as he appreciated the character of the disturbance he noted the time by his watch as 9h. 56m. 30s. He did not ascertain the error of his watch. In the same city, Dr. Willis G. Tucker says he instantly looked at his watch, and after comparing it next morning with the time of the Dudley Observatory, and making correction of the error, gave 9h 55m., very nearly, with an error probably not exceeding twenty seconds.

From Fonda, N.Y. (780 miles), Francis L. Yates reports 9h. 55m. (no particulars).

At Ithaca, N.Y. (695 miles), the regulator clock on the wall of the railway depot stopped at 9h. 55m. "exactly."

At Gowanda, N.Y. (666 miles), where the shocks were faintly felt, W. R. Smallwood, watchmaker and jeweller, noted the end of the perceptible shocks at 9h. 55m. 30s. by his regulator clock.

At Toronto (753 miles), the earthquake was recorded automatically upon the magnetographic traces in the observatory of Prof. Chas. Carpmael, Superintendent of the Meteorological Service of Canada. In his letter of September 14 he says:—"I may state that at 9h. 55m. p.m. all our magnetic needles were set in motion by earth tremors. The vibrations of the magnets were continued for about four minutes. I would say that from later and more careful measurements from our magnetic curves I make the time of the earth tremor at Toronto to be 9h. 54m. 50s. p.m., standard; this time, I should say, would not be astray more than a few seconds." As this record was automatic, and gave not only the time but the phases, it has been thoroughly investigated by Profs. Newcomb and Carpmael, assisted by Mr. C. A. Schott, of the U.S. Coast Survey. The final result of this re-examination is to change Prof. Carpmael's computation

[1] For European readers it seems necessary to refer briefly to the American "standard time" system, which will assist them in estimating the character of these time records. Throughout the Atlantic States all clocks designed for accurate time-keeping are set daily to the time of the 75th meridian west of Greenwich. In the Mississippi Valley they are similarly set to the time of the 90th meridian; in the Rocky Mountains to that of the 105th, and on the Pacific coast to the 120th meridian. They are called respectively, Eastern, Central, Mountain, and Pacific time, and the differences are exact hours. At some convenient hour every day the wires of every railroad and telegraph company in the country are put into circuit with the clock of some astronomical observatory (or with some standard clock controlled by an astronomical clock), and time signals are sent to every railway station and telegraph office. The station agents, or telegraph operators, of these companies are held responsible that these signals are received, and that their clocks are regulated by them daily. A failure to do so is a breach of discipline. The greatest purveyor of accurate standard time is the Western Union Telegraph Company, which furnishes it at a small charge to some railways, to telephone exchanges, to town and city offices, to hotels, to private corporations; in short, to anybody who wants it. For the Eastern and Southern States it takes its time by a special wire from the National Observatory at Washington. The system is essentially perfect, whereby clocks can be set once each day to exact standard time in every railway station and telegraph office in the country. And at every such station and office it is the duty of somebody to see that it is carried out. How accurately this is done is another matter. It depends upon the discipline of the companies and the habits of individuals, in which there are no doubt varying degrees of precision. The clocks supplied are always good ones, and ought not to have daily errors of over four or five seconds. But the best clock ever made will not keep good time unless properly managed. The demand for extremely accurate time throughout the greater part of the United States is enormous, and this acts as a constraint upon the companies and their *employés* to carry out the system with precision. This same demand has led to the organization of private companies in large towns and cities who receive time from the Western Union Telegraph Company and purvey it to private houses, hotels, merchants, workshops, &c.

[2] The distances have been measured somewhat hastily with a scale upon the War Department map of the United States, taking the greater epicentrum seventeen miles north-west of Charleston as the origin.

to 9h. 56m. 18s. for the beginning of the tremors, with a probable error of fully one minute. This large probable error is due to the very small scale upon which the magnetograph records time intervals (one-tenth of a millimetre corresponding to twenty seconds), and to want of sharpness in the photographed traces. This time gives 2·66 miles per second, or 4250 metres, with a probable error of one or two tenths of the amount.

The clock in the Western Union Telegraph Office at Pittsburgh (523 miles) was stopped at 9h. 54m.

From Cincinnati and suburban towns (500 miles) come many reports. In this city local mean time is largely used, owing to the fact that it is nearly midway between the 75th and 90th meridians, where the only inconvenience of standard time is at a maximum. The correction to the 75th meridian is + 37m. 40s. The Western Union Telegraph Office gives 9h. 54m. The *Times-Star* newspaper gives, from the clock in its own office, 9h. 16m. "exactly" (9h. 53m. 40s. standard); at the *Commercial Gazette* office, 9h. 17m. 45s. local, 9h. 55m. 25s. standard (probably noted after the shocks were over). At the fire tower, after the principal shock, 9h. 16m. 17s. was noted; clock error, twenty-three seconds slow, giving 9h. 54m. 20s. standard. Two other observers, noting by watches, give 9h. 16m.; and one notes an advanced stage of the shocks at 9h. 17m., but give no means of estimating their errors. At Covington, Ky., across the Ohio River, I. J. Evans, watchmaker and jeweller, reports his regulator clock stopped at 9h. 17m. 20s., Cincinnati local mean time. Phase of shock unknown.

From Crawfordsville, Ind. (622 miles), E. C. Simpson, C.E., reports through Prof. J. M. Coulter, of Wabash College: "Suddenly felt my chair move, jumped up and said, 'We are having an earthquake'; at once pulling out my watch I found it was 8h. 54m. p.m. standard time (Central)." Prof. Coulter adds that the watch was exactly with railroad time as shown at the railroad station, and also by the town clock.

From Dyersburg, Tenn. (569 miles), Louis Hughes writes:—"My time-piece was an English patent lever watch of Chas. Taylor and Son, London, which from business necessity I keep closely with railroad time at the station, which receives the time at 10 o'clock every morning. The railroad uses Central time. My first thought was that the shaking was caused by the children in the next room; but in the next moment, recognizing the peculiar sensation, I dropped the newspaper and observed the time, which was probably four to six seconds after 8h. 54m., and from that approximated it in even minutes." Speed 3·25 miles, or 5230 metres.

At Memphis, Tenn. (590 miles), the Signal Service Observer reports a considerable number of stopped clocks, one at 9h. 54m. and the others at 9h. 55m. For some unaccountable reason the seconds were not noted. The phase is unknown.

The foregoing comprise those time reports which seem to justify the presumption that the errors do not exceed one minute. There are others, which are obviously rude approximations, giving exact hours, quarter-hours, or tens of minutes. There are also some which look at first like good observations, but which surely involve some large unexplained error.

As the discussion of the time data is now progressing, no further comment will be offered here beyond the remark that there can be no doubt that the speed of propagation exceeded 3 miles, or 5000 metres, per second. The only questions are how much this speed was exceeded and whether the speed along any given line was constant. As regards the latter question, the data are not yet precise enough to justify an opinion. This matter will be inquired into.

The high rate of propagation will probably prove unexpected to European seismologists. We propose, however, to follow it up with the suggestion that it is about the normal speed with which such waves ought to be expected to travel, and that all determinations of the rate of propagation in any former great earthquakes which are much less than 5000 metres per second (for normal waves at least) are probably erroneous in proportion as they fall short of the Charleston earthquake. Finding as the time reports accumulated that a speed in excess of 5000 metres was indicated, and this presumption having become a conviction, we were led to inquire whether there was not some speed deducible from the theory of wave-motion in an elastic solid to which all great earthquakes ought to approximate.

In a homogeneous and perfectly elastic solid, the rate of propagation is, according to theory, dependent upon two properties of the medium: elasticity and density. There are two coefficients of elasticity in solid bodies, one of which measures their resistance to changes of volume; the other, to changes of form. Absolute experimental determinations of the values of these coefficients have never been made. If, however, we knew the ratios of these coefficients in one substance to the homologous coefficients in any other substance, and if we also knew the rate of propagation in either of them, the rate in the other would be at once deducible. The rate in steel bars has been the subject of much experimentation, and is given by Wertheim, whose researches have been as careful as any, at 16,800 feet per second. But as the waves in a steel bar are essentially waves of distortion, he multiplies this result by $\sqrt{\frac{3}{2}}$ or $\frac{5}{4}$ for the normal wave, giving a speed of 21,000 feet per second. The elastic modulus of steel for engineering purposes is usually taken to be 29,000,000. The corresponding modulus for such rocks as granite and basalt in a very compact state is about 8,000,000. If we may assume that these moduli are proportional to the two elasticities of the two substances respectively, we can compute the rate of propagation in rock. This assumption may or may not be true; but we assume it to be so. Let V_s be the rate of propagation in steel, and V_r the rate of propagation in rock, and let e_s and e_r be their true compressional elasticities, and let D_s and D_r be their respective densities. Our assumption is that $29 : 8 :: e_s : e_r$ from which we may form the equation—

$$\frac{V_s}{V_r} = \sqrt{\frac{e_s}{D_s} \times \frac{D_r}{e_r}}.$$

Taking the density of steel at 7·84, and of deeply-buried rocks in their most compact state at 2·85—

$$\frac{V_s}{V_r} = \sqrt{\frac{29}{7 \cdot 84} \times \frac{2 \cdot 85}{8}} = 1 \cdot 15 \text{ nearly.}$$

Taking the rate of compressional waves in steel to be 6400 metres per second, gives 5570 metres for similar waves in very compact and dense rock. The corresponding rate for waves of distortion would be 4450 metres. These results are so near to those deduced for the Charleston earthquake that they seem to be worthy of consideration.

The experimental measurements of the rate of impulses obtained by Milne and Fouqué seem to us inapplicable. The elasticity of the surface soil, we think, is no more to be compared with that of the profound rocks which transmit the great waves of an earthquake than the elasticity of a heap of iron filings is to be compared with that of an indefinitely extended mass of solid steel. The difference is *toto cœlo*. But the rate of propagation is a question of elasticity and density chiefly. The effect of temperature we have not considered. Perhaps the most striking experiment ever made with an artificial earthquake was at the Flood Rock explosion at Hell Gate, near New York, where General Abbott found a speed of propagation approaching very closely to that of the Charleston earthquake.

The question which is undoubtedly of deepest interest in this connexion is whether the Charleston earthquake throws any new light upon the origin of such events. While we are not prepared to say that absolutely nothing will be added to our information on this question, we are forced to admit that we expect very little new light. Hitherto our efforts have been devoted to bringing together the facts and to arranging and comparing them, and we have as yet given but little consideration to this final question. It will, however, shortly engage our attention, and in anticipation of this we prefer to remain silent for the present, fearing that, if we commit ourselves here to any preference for a particular view, we may find ourselves encumbered with a bias arising from the intensely human propensity to defend, through thick and thin, utterances which have once been formally given.

ON A POINT OF BIOLOGICAL INTEREST IN THE FLOWERS OF "PLEUROTHALLIS ORNATUS," RCHB. F.

IN December of last year (1886), in the Orchid-house at Kew, a specimen of *Pleurothallis ornatus* [1] flowered. Not only is this the first time that it has done so at Kew, but I am informed by Mr. Watson, of the Royal Gardens, who drew my attention to it, that hitherto *P. ornatus* has not been known to flower in captivity.

The flowers of this plant present a most interesting adaptation, whereby to attract insects, of which I propose in this note to give a short account.

The genus *Pleurothallis* is characterized (generally speaking) by the inconspicuousness of its flowers, which, as a rule, are of a reddish-brown colour. The flowers are either solitary and axillary, or in few-flowered racemes. The outer perianth-whorl (sepals), though never exceeding a few millimetres in length, is several times longer than the inner (petals). The sepals are sub-equal, and the lateral ones slightly connate at the base. The two lateral petals are small and wing-like on either side of the column. The short, superiorly-grooved labellum is always shorter than the petals, and articulates with the column by a narrow flexible neck. Such an arrangement, in consequence of which the labellum is more or less vibratile, and after a touch will oscillate several times, is found in several allied genera, *e.g. Restrepia*, and especially *Bolbophyllum*. The genus *Pleurothallis* is tropical American, and epiphytic.

Pleurothallis ornatus is especially distinguished from other members of the genus in the fact of its sepals possessing an extremely conspicuous fringe of white cirrhi. In no other species of the genus, of which I have been able to find figures or specimens, is anything of the kind seen.[2] The hair-like structures which form this fringe in *P. ornatus* average about 2 millimetres in length; and when it is remembered that the extreme diameter of an expanded flower does not exceed 10 millimetres some idea of the conspicuous part played by the fringe is obtained. Figs. 1 and 2 are respectively front and lateral views of a flower, magnified about five diameters. Each hair it will be seen narrows very much at its proximal end, and is in this way rendered versatile. From the fact of the hairs being air-containing they are excessively light, and moved by every breath of air. The motion of course is an entirely *passive* one—they are simply swayed to and fro on the hinge formed by this tapering.

In Fig. 3 is represented a microscopic view of one of the cirrhi detached. It consists simply of a prolongation of one of the epidermal cells at the edge of the sepal—and its lumen is continuous with that of the epidermal cell from which it originates (cf. Fig. 4). In form, the hair resembles that of a flattened club. Its width, throughout most of its extent, averages 0·2 millimetres. But it is flattened in the plane at right angles to this, so that its thickness is only about 0·025 millimetres. Externally the hair has a granular aspect, arising from numerous slight rugosities of its delicate cuticle (cf. Fig. 3). At its proximal end it narrows as it runs into the epidermal cell from which it arose.

In the expanded flower the hairs are air-containing, the protoplasm being entirely collapsed and dried up.

The versatile hairs are inserted along the margin of the sepals at intervals of less than 1 millimetre. Towards the attached part of the sepals they become much shorter.

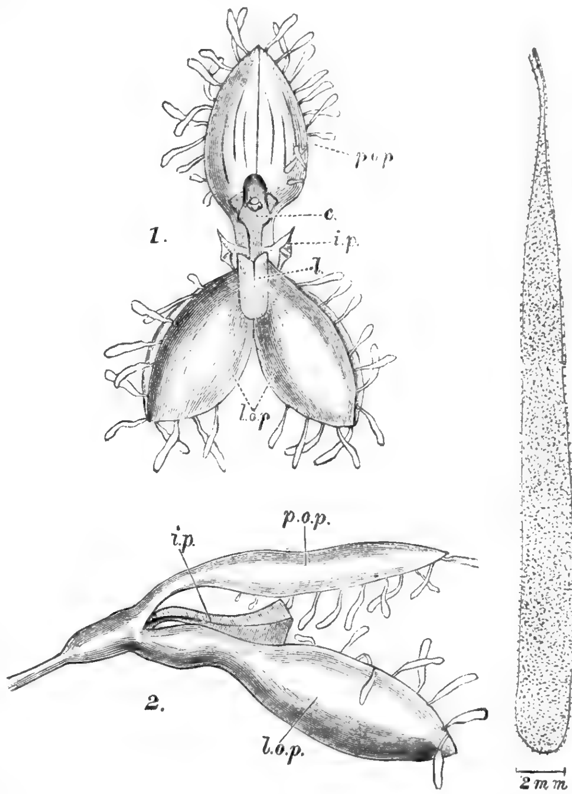


FIG. 1.—View of the flower from in front, × 5 diameters. *c*, column: *i.p*, petals; *l*, labellum: *l.o.p*, lateral sepals; *p.o.p*, posterior sepal.
FIG. 2.—Lateral view of same flower, × 5 diameters. References as in Fig. 1.
FIG. 3.—A single isolated vibratile hair, much magnified.

The precise mode of insertion is seen in Fig. 4, which represents a transverse section of the edge of a sepal. The hair is formed from one of a group of small cells (*h.c*) at the extreme edge of the sepal. In the figure are seen its relations to the parenchyma, and to the upper and lower epidermis (*u.e* and *l.e*) of the sepal.

I have been unable to examine buds of the plant, consequently no account can be given of the development of these hairs.

As regards its biological meaning, there can, I conceive, be little doubt but that the fringe serves to attract insects which fertilize the otherwise inconspicuous flowers. The white lustrous appearance of the cirrhi is a consequence of their air-content; and it is as important a factor as their versatility, in successfully rendering the small brown flowers conspicuous to insects. As I have

[1] Described by Prof. H. G. Reichenbach in Wittmack's *Gartenzeitung*, 1882, p. 105. To him I am indebted for this reference.

[2] Except perhaps in *Pleurothallis ciliata*, which is described and figured by Knowles and Westcott in "The Floral Album," vol. i. p. 40. Here, however, it is the *petals* which have a ciliated border. No description is given of the hairs, though the authors mention having examined them microscopically. The figure is a bad one, and barely shows the existence of a fringe.

said above, the motion is provoked by the least possible breath of air.

I do not remember a mechanism entirely like this elsewhere among either Orchids or other Phanerogams. Many Orchids are provided with long fringes, but these are due to excessive dissection of the sepals (as in Cirrhopetalum), or to hairs—often multicellular—which, however, are non-versatile. Mr. Rolfe, of the Kew Herbarium, reminds me of the case of *Bolbophyllum lemniscatum*. I need not here further mention the extraordinary appendages of the sepals: they are figured in the *Botanical Magazine*, Pl. 5967.

The labellum in *P. ornatus* is quite small: in Fig. 1 it is shown at *l*, but in the lateral view (Fig. 2) it is hidden by the two petals (*i.p*). Like the labellum in so many allied Orchids, it moves readily on its narrowed neck if touched. The oscillations are performed especially in a vertical plane.

The usually accepted view as to the meaning of this vibratility is that hinted at by Morren ("Recherches sur le mouvement et l'anatomie du labellum du *Megaclinium falcatum*," 1841, p. 95) and Darwin ("Fertilization of

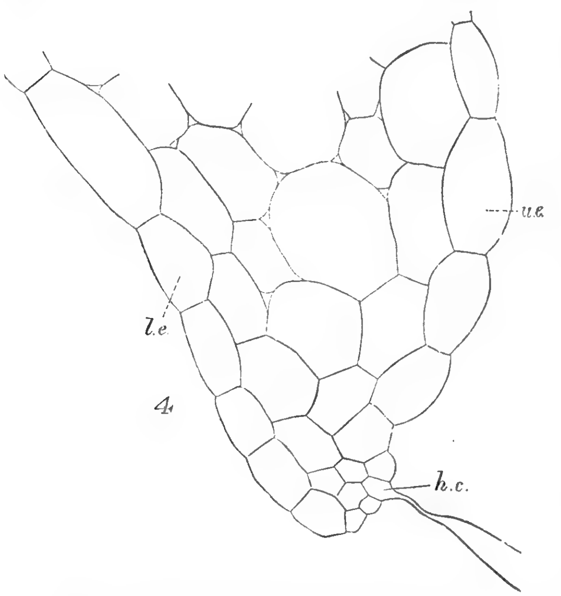


FIG. 4.—Transverse section through the edge of a sepal, showing the insertion of one of the vibratile hairs. *h.c*, cell bearing the hair (part of which is represented); *l.e*, epidermis of lower side of sepal: *u.e*, epidermis of upper side.

Orchids," p. 171); *i.e.* that by the continued motion of the labellum caused by the wind, insects are led from motives of curiosity to visit the flower. This explanation will hardly hold for such a genus as *Pleurothallis*, where the labellum is extremely small, and its motion would be hardly obvious from outside the flower. Here the labellum acts rather as a spring-board. The insect entering the flower will lean upon and displace the labellum, which, from the extreme elasticity of its neck, will oscillate up and down in precisely the same manner as a spring-board would. By this is insured the insect's head being thrust against the stigma or pollen-masses, and the act of pollinization promoted. Sometimes I have found that if the labellum be displaced by gently pressing downwards it will be retained for a few seconds in the displaced position on removing the force. Soon, however, the elastic reaction overcomes the resistance of the sepals (by which it is temporarily jammed), and the labellum flies up again, considerably overstripping its normal position of rest. After one or more small oscillations, it comes to rest. Such a simple experiment as this shows well enough how such an arrangement can aid cross-fertilization.[1] I believe this is the chief part played by the vibratile labellum in *Bolbophyllum*, in which genus the elasticity is especially manifest. This in no way excludes the attractive function suggested by Darwin. This latter could only hold for cases where the labellum is easily visible outside the flower, and for such cases as *B. barbigerum*, *B. tremulum*, &c., where it is richly plumed. On the other hand, there is no reason why the "spring-board" function should not operate in every case of vibratile labellum; hence I regard this as its primary significance, whilst the attractive one is secondary only. This is a question which I hope soon to follow up.

F. W. OLIVER.
Jodrell Laboratory, Kew.

CUBIC CRYSTALS OF GRAPHITIC CARBON.

IN the analysis of a meteoric iron found in 1884 in the sub-district of Youndegin, Western Australia, and of which two of the four fragments have been generously presented to the British Museum by the Rev. Charles G. Nicolay, Curator of the Geological Museum, Fremantle, I have obtained some crystals, a description of which may be of interest to the students of carbon.

The crystals were obtained as an insoluble residue on treatment of 8·3200 grammes of the iron with aqua regia: they are bright, opaque, grayish-black, have a metallic lustre, and present forms belonging to the cubic system. As their characters were not recognized as belonging to any known mineral, it seemed unlikely that the nature of the crystals could be completely determined, seeing that the total weight obtained was only 3 milligrammes; further, two fragments of the iron, weighing 2 and 7 grammes respectively, had not yielded a single crystal, and there was thus a possibility of their being so localized in the iron as to render impracticable an increase of the quantity of material available for experiment.

The crystals were about a hundred in number, the average thickness of the larger ones being 1/100 of an inch. Many of them are sharply defined cubes; some have their edges truncated by the faces of the dodecahedron; in others the edges are replaced by rounded faces of a tetrakishexahedron.

Their hardness is greater than that of rock salt and less than that of calcite: the streak is black and shining. Of four crystals, two sank to the bottom and two remained near the surface of a solution having a specific gravity of 2·12. The crystals are unaffected by acids: heated in a combustion-tube in a current of oxygen, hydrogen, or chlorine, they are unattacked, even when the glass begins to melt. Heated in a platinum capsule with the table-blowpipe, they slowly disappear without flame. Heated with potassium nitrate in a crucible over a Bunsen burner, they are unaltered; but disappear very slowly, without deflagration, when heated with the table-blowpipe.

In density, colour, and streak, and in its chemical behaviour, the residual mineral thus bears a close resemblance to native graphite, but it is considerably harder, and it presents itself in well-defined crystals which belong, like those of the other crystallized form of carbon, the diamond, to the cubic system: terrestrial graphite, when crystallized, is found only as tabular crystals so indistinctly formed that doubt has long existed as to whether they should be referred to the hexagonal or monosymmetric system.

In a paper entitled "Graphite pseudomorphous after Iron Pyrites," Haidinger, in 1846, described some graphitic crystals which were doubtless similar to those furnished by the Youndegin iron: his observation, however,

[1] Regarding the nature of the pollinia, and their mode of removal in *Pleurothallis*, *vide* Darwin, *loc. cit.* p. 166.

has been forgotten, and is without record in modern meteoric literature. The crystals—of the size, number, and completeness of which Haidinger makes no mention —were obtained by him from a nodule of graphite which had dropped out of the Arva meteoric iron, and chiefly from a study of their form he inferred that they were pseudomorphous after iron pyrites. Even yet no iron pyrites, crystallized or massive, has been found in a meteorite, the meteoric sulphide of iron being, not the bisulphide, but the protosulphide: further, Gustav Rose, after examination of the crystals, expressed the opinion that the replacement of the edges of the cubes was suggestive rather of holosymmetry than of hemisymmetry, an interpretation which would exclude iron pyrites as a possible antecedent mineral.

The Youndegin graphitic crystals support the view entertained by Rose: the existence of the dodecahedron face, of which there is goniometrical proof, is of itself quite sufficient to show that the crystalline form is distinct from that of iron pyrites.

The iron pyrites theory being discarded, and the fact being recognized that no mineral constituent of meteorites has yet been found which crystallizes in forms similar to those of the graphitic crystals, there naturally arises a feeling of doubt as to the correctness of the view according to which they are of pseudomorphic origin, and thus a question as to whether they may not possibly be a third allotropic condition of crystallized carbon presenting the general characters of graphite, but a crystalline form frequent in the diamond.

Bischof denies the possibility of explaining the pseudomorphism of terrestrial minerals by any other process than the slow action of water, of which there is no evidence in meteorites; and though it would be unsafe to argue that only in this way could meteoric pseudomorphs be produced, there is sufficient difficulty in their explanation to demand strong evidence before the pseudomorphism of the graphitic crystals is granted, more especially when we have regard to the fact that no other graphitic pseudomorph has yet been established either in meteoric or in terrestrial minerals.

Examination of the Youndegin crystals under the microscope shows that some of them are hollow, and appear to be built up of successive cubical shells: on several of the crystals there are globular growths covering a large part of a cube-face, and occasionally the globule is broken, and is seen to be merely a thin, now empty, shell, of which the bottom is the face of the cube. The crystals are easily frangible, and no cleavages were observed: they appear to be quite homogeneous in their material.

Although some of these characters suggest a pseudomorphic origin of the crystalline form, it cannot be said that they prove it. Both of the recognized crystalline forms of carbon, graphite and diamond, have long been standing difficulties for the crystallographer. As already pointed out, the crystals of graphite are rarely more than mere tables, of which there is a controversy as to the crystalline system; those of the diamond are often so different in their geometrical characters from the crystals of every other known substance, that it cannot be satisfactorily determined whether they are to be referred to a holosymmetric or to a hemisymmetric type.

Hollow and skeleton crystals are often the result of a hurried crystallization, as is so well seen in the artificial crystals of bismuth and of common salt. The diamond, too, when in cubes, has faces more uneven than those of the Youndegin crystals, and shows usually the same replacement of its edges by rounded faces of tetrakishexahedra.

It thus might be argued with some force that the Youndegin crystals have been the result of a hurried crystallization of carbon, and that, while striving to reach a dignity which has been assigned to cubes of diamond, they have been overtaken by misfortune and come out in cubes of the less honoured mineral, graphite. The obtuse, almost flat, square pyramid seen on some of the cube-faces, the hollow globular growths, the occasional parallelism of the grouping of the cubes are distinct, however, from what is met with in the diamond.

And after consideration of all the observed characters of these crystals it will be seen that the explanation of the occurrence of the crystals in the interior of a mass of iron by means of pseudomorphism is untenable. Though the easy frangibility, the absence of evidence of cleavage, the hollowness, and the occasionally crust-like structure, are more or less characteristic of pseudomorphic crystals, they are not incompatible with an independent crystallization: on the other hand, while the superior hardness distinguishes the crystals from those of native terrestrial graphite, the separateness, completeness, and general excellence of the crystals, the delicacy of various acicular projections, and more especially of the obtuse, almost flat, square pyramid seen on some of the cube-faces, are sufficient to prove that the crystalline form never had a previous tenant. The delicacy of the acicular projections is such that the crystals must have been formed *in situ*. In case of pseudomorphism the elements of the original mineral ought to be in the vicinity of the crystals, and there ought to be an excess either of the original mineral or of the replacing amorphous graphitic carbon: both are, however, conspicuous by their absence, and in this fragment of the iron the whole of the graphitic carbon is present as cubic crystals.

On examination of a large graphitic nodule from the Cocke County meteoric iron, now in the British Museum, crystals of graphitic carbon, cubo-octahedral in form, are to be seen in some of the crevices.

There can be absolutely no doubt that the graphitic crystals are the result of crystallization of the meteoric graphite, and that they represent a third allotropic condition of crystallized carbon, the general characters being those of graphite, and the crystalline system that of the diamond.

As this form of graphitic carbon is unknown among terrestrial minerals, and has so important a bearing on the formation of meteoric graphite, it may conveniently receive a special name; I suggest the term "cliftonite," after Prof. R. B. Clifton, F.R.S., who has long been interested in the physical characters of minerals, and has done much to encourage their study.

A full description of the meteoric iron itself and of the graphitic crystals will appear in the forthcoming number of the Journal of the Mineralogical Society.

L. FLETCHER.

NOTES.

WE are glad to learn that at the Naval Review (some lessons suggested by which we may refer to in a future number) 120 official invitations were sent out to men of science, while many were hospitably entertained by the Peninsular and Oriental, the Orient, the British India, and the Cable-Laying Companies. Some time next century we may hope that the existence of science, of a Royal Society, and of eminent scientific men employed in the public departments, may dawn upon the then Lord Chamberlain.

THE Jubilee dinner of the Electric Telegraph, which is going on as we go to press, is a brilliant affair, to which we shall refer at length next week.

WE print to-day the text of the Technical Education Bill. It was absolutely necessary that some such measure should be introduced, and we may hope that as it has no relation to party politics it will be passed without much difficulty. One change in the Bill ought certainly to be made. According to the fourth clause, there is to be no payment out of the local rate in respect of a scholar unless or until he has passed the sixth standard. This may be a very proper provision so far as boys are concerned;

but it must not be applied to the case of adults, many of whom should be encouraged to take advantage of the new system of technical instruction. A man of thirty would be extremely unwilling to go in for an examination in reading, writing, and arithmetic, but there is no reason why there should not be payment out of the local rate on his behalf, if he is disposed to enter upon a regular course of technical education. The more adults who can be induced to attend technical schools, the better for the working classes and for the country.

WE have received a circular bearing the signatures of W. E. Ayrton, Michael Carteighe, Alfred E. Fletcher, G. Carey Foster, Michael Foster, J. H. Gladstone, H. Forster Morley, William Odling, Sydney Ringer, H. E. Roscoe, W. J. Russell, and P. J. Worsley, who have either been pupils of Dr. A. W. Williamson during the thirty-eight years that he has been Professor of Chemistry in University College, London, or have been otherwise intimately associated with him. In this circular it is suggested that Prof. Williamson's resignation of his Chair affords a fitting opportunity for recording, in some permanent manner, the high appreciation of his influence as a scientific teacher, and the feeling of personal regard for him as a man, which are so generally entertained by those who know his work and character. It is accordingly proposed to ask him to sit for a portrait to be presented to University College, and subscriptions are invited for this purpose. As it is expected that this proposal will be widely responded to, one guinea is suggested as the ordinary amount of a subscription. Dr. W. J. Russell, F.R.S., 34 Upper Hamilton Terrace, N.W., has agreed to act as honorary treasurer of the fund to be collected, and Michael Carteighe, Esq., 36 Nottingham Place, W., and Dr. H. Forster Morley, University Hall, Gordon Square, W.C., as honorary secretaries.

THE Council of King's College, London, has elected Mr. J. W. Groves—Demonstrator of Practical Biology—to the Chair of Botany, rendered vacant by the resignation of Prof. Robert Bentley.

MR. W. L. SCLATER, B.A., of Keble College, Oxford, has been appointed by the Trustees of the Indian Museum, Calcutta, to be Deputy Superintendent of their Museum in succession to Mr. Wood Mason, who has become Superintendent upon the resignation of Dr. Anderson. Mr. Sclater, who was a pupil of Prof. Moseley, took a first class in the final Examination for Natural Science in 1885, and has since been working under Prof. Ray Lankester and Mr. Sedgwick, and for the last three years has prepared the report on mammals for the "Zoological Record." Last winter Mr. Sclater passed several months in British Guiana, under the hospitable roof of Mr. E. F. im Thurn, and made collections in several branches of natural history, which have been described in the Zoological Society's Proceedings.

THE summer meetings of the Institution of Naval Architects were opened on Tuesday in the hall of the Literary and Philosophical Society, Newcastle-on-Tyne. Lord Armstrong began the regular business of the conference by reading a paper by himself and Mr. J. Vavasseur on the application of hydraulic pressure to gunnery. A paper was also read by Mr. F. C. Marshall on recent developments in marine engineering. After the meeting the members were conveyed in brakes to the Elswick Works, where they were shown over the ordnance and ship-building departments, and were entertained to luncheon by Lord Armstrong.

AN Electrical Exhibition will be given in New York in the autumn by the New York Electrical Society. The Exhibition will be open from September 28 to December 3. It will include, says *Science*, "all that relates to the science and application of electricity in its broadest sense." As no electrical exhibition has ever been held in New York, it is expected that this o will attract a large number of visitors.

WE learn from *Science* that the American Committee of t International Congress of Geologists will present a report at t approaching meeting of the American Association concerni the positions to be taken by the representatives of Americ geologists at the next session of the Congress in London (1888 upon the more important questions of nomenclature, classificatio and colouring, which will there be discussed. The Committ requests that a day may be set apart by Section E for the co sideration of these questions, and it proposes that all Americ geologists (whether members of the American Association not) shall be invited to attend this session and participate the work.

WITH respect to the recent small but exceedingly fierce a destructive cyclone, which literally effaced a station on the coa of the Bay of Bengal, called False Point (NATURE, pp. 110 a 136), a correspondent writes to us from Calcutta :—"The stor was an exceedingly interesting one, and some of its features a quite different from those previously recorded. It is very notic able in the fact that at the centre of the storm a lower pressu was recorded than during any storm that I have read of, f pressure fell to nearly 27 inches at sea-level. The rapidity wi which the pressure fell was also extraordinary."

PROF. PEDLER, Principal of the Presidency College, Calcutt who is in charge of the Bengal Meteorological Department, ga notice of the existence of this terrible storm in the middle of th Bay of Bengal five or six days before it broke over the land. H was also able to give twenty-four hours' notice of the precise pa of the coast which the storm would (and did) cross. He hoiste warning signals in the river at Calcutta to prevent ships from lea ing. Unhappily one steamer went out in spite of the signals, an foundered, with about 900 people on board, every one of who was drowned. In obedience to the signals, six or seven othe vessels remained in safety. Among these vessels were tw steamers going to the same port as the one which foundered and having about as many people on board. A large numbe of persons, therefore, owed their lives directly to meteorologic science. It would be hard to conceive a more striking illustra tion of the practical value of meteorology.

WE have received the concluding part of the Quarterl Weather Report of the Meteorological Office for the year 1878 This volume is the third of the new series begun in 1876 and contains charts showing mean meteorological conditions fo each month, a general summary of the weather for each quarter and the usual tables giving the results derived from the record of the seven observatories then co-operating with the Office together with continuous curves of the self-recording instruments In an appendix is a paper by General R. Strachey, R.E., F.R.S. Chairman of the Meteorological Council, which will be very avail able for agriculturists. By the use of the tables the amount of th excess or defect of the daily temperature above or below an fixed minimum, below which active vegetation does not begin may be easily obtained during the year from the ordinar temperature observations usually made; this could be effecte previously only by a laborious calculation. The values of suc "accumulated temperature," published in the Weekly Weathe Report of the Meteorological Office, are calculated by thes tables. The Monthly Weather Report, which began in 1884, and which is published nearly up to date, now takes the place of the Quarterly Reports.

THE *Meteorologische Zeitschrift* for July contains the conclud- ing portion of Dr. Köppen's article on the classification o clouds (NATURE, June 30, p. 208). We are glad to see that in this second portion full justice is done to the recent researches of the Hon. R. Abercromby, and a lengthy report is given of the results of a conference between that gentleman and M. Hildebrandsson at Upsala at the end of 1886. This report points out that the study of the forms of clouds may be undertaken with different objects in view. If the object be weather-prediction, a detailed terminology is necessary, and for this purpose M. Hildebrandsson thinks Mr. W. C. Ley's classification of the higher clouds is unsurpassed. One of the principal objects is the determination of the directions of the wind in the higher regions of the atmosphere, and for this it is not necessary to distinguish so many forms; but we must be sure (1) that these forms are, generally speaking, everywhere the same, and (2) we must determine the mean heights of the various forms by direct measurements. With the view of settling the first point, Mr. Abercromby has made two voyages round the globe. The second question has been partially solved by the researches of MM. Ekholm and Hagström at Upsala (NATURE, June 30, p. 206). It is, however, necessary that such measurements should be made at various other places, and the same gentlemen intend to make further experiments elsewhere during this summer. Dr. Köppen concludes his article by some remarks on the history of the development of clouds, and recommends a series of observations in balloons similar to the celebrated ascents made many years ago by Mr. Glaisher. The same number of the *Zeitschrift* contains interesting articles on the results of meteorological observations during solar eclipses, by Mr. Winslow Upton, and on the method of counting the number of rainy days in various countries, and its influence on the resulting period of rain-frequency, by Dr. E. Brückner, of Hamburg. The amount of rainfall which is taken as representing a rainy day differs considerably in different countries. The author recommends the general adoption of 0·005 inch, without reference to whether it be caused by rain, snow, dew, &c. The amount has not yet been definitely fixed by the Meteorological Congresses, but that adopted by this country is 0·01 inch (or 0·005 inch where the rainfall is measured to thousandths of an inch). The International Polar Committee have adopted 0·1 millimetre (=0·004 inch) as representing a rainy day in all their publications, while for Prussia twice that amount is taken as the minimum quantity.

TIME-SIGNALLING on the German coasts began (we learn from a recent paper by Prof. Foerster) twelve years ago, and there are at present seven time-balls in action; viz. at Bremerhaven, Cuxhaven, Swinemünde, Neufahrwasser, Wilhelmshaven, Kiel, and Hamburg. In this respect, our country stands first. We began some thirty years ago, and have at present fourteen time-balls on our coasts, also five other arrangements for the same end. In our colonies and dependencies there are twenty-six time-balls. France possesses four time-balls (and two other arrangements); Sweden and Norway, Austria-Hungary, Holland with Belgium, and the United States, have five each; Denmark has two; Spain and Portugal one each. Italy has none as yet.

THE list of examples illustrating the law of isomorphism has just received a strong reinforcement at the hands of M. Charles Fabre, who describes in the last number of the *Comptes rendus* the result of his attempt to prepare a series of selenium alums isomorphous with the corresponding double sulphates. Following up the work of Wohlvil, Wöhler, and Pettersson, Fabre has succeeded in preparing double selenates of the general formula $Al_2(SeO_4)_3 . M_2SeO_4 . 24H_2O$, in which M represents respectively potassium, sodium, cæsium, rubidium, thallium, ammonium, ethylamine, di- and tri-ethylamine, and propylamine. Each of these alums crystallizes in the cubic system, generally in colourless octahedra; and some of them, notably the double selenate of aluminium and thallium, form exceptionally beautiful crystals. Further, the French chemist finds, as might be expected, that chromium forms a similar series of isomorphous double selenates, most of which build up splendid octahedra, black by reflected and violet by transmitted light. These alums are comparatively easy to obtain crystallized if the temperature be kept low, but at slightly elevated temperatures the small amount of chemical attraction by which the two constituent selenates are constrained to combine together in molecular proportions is overcome, and the alum can never be formed.

THE last numbers of the Journal of the China Branch of the Royal Asiatic Society (vol. xxi. Nos. 1 and 2) contain an interesting "symposium" on the question whether the Chinese should be taught Western science through the medium of their own or a European language. If the latter, no doubt the language would be English. The stumbling-block in the way of teaching science to the Chinese is the difficulty, not to say impossibility, of finding Chinese equivalents for the terms of our science. The Japanese have made the attempt at translation, but do not appear to be quite satisfied with the result. The missionaries who take part in the discussion appear to be of opinion that the Chinese language is the best medium, while on the other side it is contended that as long as it is taught by foreigners it had better be taught in a foreign language, "and probably by foreigners who have not had their faculties paralyzed by the task of mastering the Chinese language." Most of the laymen appear to be of this opinion. The question, after all, appears to be one of terminology; for if this difficulty can be overcome there is, we presume, no dispute that men, whether Orientals or Europeans, can best acquire knowledge through the medium of their native tongue. In the terminology the question appears, in Japan at least—and the same is doubtless true of China—to be whether the terms of Western science should be translated approximately or transliterated approximately. Should there be, for example, an attempt to reproduce by transliteration the words *hydrogen*, *nitrogen*, *logarithm*, &c., or an attempt to translate their meanings into concise terms which will take their places in Chinese and Japanese science? In either case the student will have to learn a new terminology, exactly as students in the West do. This is a point for Oriental scholars to decide, but it certainly does seem at first sight that transliteration is preferable to translation, for in the latter there is room for dispute and differences of opinion and practice, while the former has severe simplicity to recommend it.

SEVERE earthquakes were noticed on July 11 in the Hungarian districts of Arad, Temesvar, and Torontal.

ON July 17 shocks of earthquake were felt at Catania, Lecce, Ischia, Livorno, and Parma. Oscillations were felt in Rhodes, Crete, and Chios, and at Smyrna. Several houses were damaged at Canea, and in Rhodes a part of the fortress-wall and some chimneys were destroyed.

M. BURCH says, in *Cosmos*, that in America he saw six wild geese, when flying in a storm, killed by lightning.

THE French Academy of Sciences has received the Giffard legacy of 50,000 francs, and has resolved to employ the interest in grants to learned men in pecuniary difficulties.

PROF. LUNGE, of Zurich, has re-written and added to the treatise on "Coal-Tar Distillation" which he brought out in 1882. The new edition, with many new working drawings, will be ready very soon, and Messrs. Gurney and Jackson, Mr. Van Voorst's successors, are to publish it.

IN the *Moniteur Belge* of the 3rd inst., a Royal decree was published nominating the Vice-Presidents, Chief Secretary, and staff of the Scientific and Industrial Competition which is to be held in Brussels next year. M. Charles Mourlon is the Chief Secretary.

MR. HILCKEN, Librarian of the Bethnal Green Free Library, writes to us that the Library is greatly in need of one or two microscopes. "We have received," he says, "a present of interesting 'objects,' but they are useless without microscopes. Many of our readers would gladly avail themselves of the use of such instruments."

DR. R. H. GUNNING, of Rio de Janeiro and Edinburgh, has made the following munificent gifts in connexion with Her Majesty's Jubilee:—To the Council of the Royal Society of Edinburgh, a triennial prize of £105, to be named "The Victoria Jubilee Prize for the Advancement of Science." To the Council of the Society of Antiquaries of Scotland, £40 yearly, or £120 every three years, as they may prefer, to be named "The Victoria Jubilee Gift," the object of the founder being to assist experts to travel, with the view of "examining other collections, and keeping the Edinburgh Museum as completely furnished with information and examples as possible." To the Senatus of the University of Edinburgh, £200 per annum, to provide eleven post-graduation triennial prizes of £50 each. These are to be named the Monro, Sir Charles Bell, Edward Forbes, Hutton Balfour, Joseph Black, Christison, Lister, Gregory, John Thomson, Simpson, and Alison Prizes, and are to be administered by the Senatus, the incumbent of the Chair in connexion with which the prize is to be awarded having a wide choice in the subjects of examination. To the Royal College of Physicians of Edinburgh, £100 triennially, for a prize to bear the title "Dr. Gunning's Cullen Prize for the greatest benefit done to Practical Medicine." To the Royal College of Surgeons of Edinburgh, £120 triennially, for a prize to be called "The Liston Victoria Jubilee Prize," which shall be open to all Fellows and Licentiates of the College, and shall be awarded for the greatest benefit done to practical surgery. To the Edinburgh Association for the University Education of Women, £40 annually for a bursary to be called "The Victoria Jubilee Bursary." In addition to the above, Dr. Gunning has intimated, through Lord Maclaren, a gift of £100 for the Ben Nevis Observatory. Dr. Gunning, who was long resident in Brazil, is a Dignitary of the Brazilian Empire, a Fellow of the Royal Society of Edinburgh, and a Fellow of the Society of Antiquaries of Scotland.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♂) from India, presented by Mr. Francis Yare; a Cape Zorilla (*Ictonyx zorilla*) from Cape Colony, presented by Mr. J. A. Willet; a Spotted Ichneumon (*Herpestes nepalensis*) from Nepal, presented by Mr. T. C. Bacon; two Spotted Cavys (*Cœlogenys paca*) from South America, presented by Mr. William F. Kirton; an Arizona Squirrel (*Sciurus arizinensis*) from New Mexico, U.S.A., presented by Dr. R. W. Shufeldt; a Common Cuckoo (*Cuculus canorus*), British, presented by Mr. W. M. Alexander; a Lesser Kestrel (*Tinnunculus cenchris*), South European, presented by Mrs. M. Travers; two Corn Crakes (*Crex pratensis*), British, presented by Mr. S. C. Hincks; two Cardinal Grosbeaks (*Cardinalis virginianus*) from North America, presented by Mr. Samuel Nicholson; two Hybrid Herring Gulls (between *Larus argentatus* and *Larus dominicanus*), presented by Lord Lilford; two Viperine Snakes (*Tropidonotus viperinus*) from North Africa, a Bordeaux Snake (*Coronella girondica*), South European, presented by the Rev. T. W. Haines; a Grey Ichneumon (*Herpestes griseus*) from India, an Aldrovandi's Skink (*Plestiodon auratus*) from North Africa, deposited; a Crested Porcupine (*Hystrix cristata*), born in the Gardens; two Slender Ducks (*Anas gibberifrons*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE TOTAL SOLAR ECLIPSE OF 1886.—Mr. W. H. Pickering, who observed the total solar eclipse of August 1886 at Grenada, W.I., communicates to *Science*, vol. x. No. 230, a brief account of his results, in order that it may be published in time to be of service to the observers of the approaching eclipse on August 18. It was found that, by using rapid gelatine plates, an exposure of one or two seconds was sufficient to show the details of the inner corona satisfactorily with an ordinary telescope-lens. With a portrait-lens, the ratio of whose aperture to its focus was as one to five, the same exposure showed the outer corona satisfactorily as far as a distance of 15′ to 30′ from the limb of the moon. Beyond that the light was very decidedly fainter, and was shown best by exposures of from eight to forty seconds. The corona showed the usual short rays proceeding from the sun's poles, and from the south-western quadrant a very conspicuous ray, appearing like a hollow cone, projected to a distance of about 20′. A number of prominences were seen near the equator, on both sides of the moon; but the most conspicuous one was situated in the north-western quadrant. It extended to the height of about 100,000 miles, and had apparently a somewhat spiral structure. The spectra of the various prominences were shown very clearly by the prismatic camera. In the equatorial ones the hydrogen and H and K lines were prominent, superposed on a background of continuous spectrum; but in the large prominence the hydrogen lines were absent, although the H and K lines were strongly marked. The position of the maximum density in the continuous spectrum of the prominences was found to be quite different from that of the corona; in the former it is not far from G, whilst in the latter it lies between G and F. A large number of persons observed the shadow-bands, which appeared before and after totality. The general result of their observations indicated that the bands were about 5 inches wide and 8 inches apart, that they were coloured like the spectrum, and that they moved with a velocity comparable with that of an express train; at all events much faster than a man could run. Before totality the bands lay N. 12° W. and S. 12° E., and travelled west; after totality they lay N. 60° E. and S. 60° W., and travelled north-west.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 JULY 31—AUGUST 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 July 31

Sun rises, 4h. 24m.; souths, 12h. 6m. 9·1s.; sets, 19h. 48m.: decl. on meridian, 18° 18′ N.: Sidereal Time at Sunset, 16h. 24m.

Moon (Full on August 3) rises, 17h. 15m.; souths, 21h. 36m.; sets, 1h. 56m.*; decl. on meridian, 19° 37′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian ° ′
Mercury	4 26	11 46	19 6	14 35 N.
Venus	8 41	14 53	21 5	1 42 N.
Mars	2 2	10 20	18 38	23 35 N.
Jupiter	11 56	17 10	22 24	9 45 S.
Saturn	3 26	11 24	19 22	20 47 N.

* Indicates that the setting is that of the following morning.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Aug. 1,	21 30	*m*
			,, 6,	21 9	*m*
Algol	3 0·8	40 31 N.	,, 1,	22 54	*m*
V Boötis	14 25·2	39 23 N.	,, 5,		*M*
δ Libræ	14 54·9	8 4 S.	,, 5,	22 24	*m*
V Coronæ	15 45·5	39 55 N.	,, 4,		*M*
R Ursæ Minoris	16 31·5	72 30 N.	,, 5,		*m*
U Ophiuchi	17 10·8	1 20 N.	July 31,	4 2	*m*
			and at intervals of	20 8	
W Sagittarii	17 57·8	29 35 S.	Aug. 2,	1 0	*m*
T Herculis	18 4·8	31 0 N.	,, 2,		*M*
η Aquilæ	19 46·7	0 43 N.	,, 2,	2 0	*M*
			6,	21 0	*m*
S Sagittæ	19 50·9	16 20 N.	,, 5,	21 0	*m*
δ Cephei	22 25·0	57 50 N.	July 31,	4 0	*M*
			Aug. 3,	22 0	*m*

M signifies maximum; *m* minimum.

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

August.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
1 ...	21 Sagittarii	... 5 ...	0 23 ...	1 29 ...	99 323
6 ...	70 Aquarii	... 6 ...	1 14 ...	1 38 ...	35 6

August 3.—Partial eclipse of the Moon. First contact with shadow 19h. 36m.; middle of eclipse 20h. 49m.; last contact with shadow 22h. 2m. Magnitude of eclipse = 0·419 of moon's diameter. The moon will rise at Greenwich at 19h. 35m.

GEOGRAPHICAL NOTES.

THE rumour as to the death of Mr. Stanley is universally discredited in geographical circles, and among those directly interested in the Emin Pasha Expedition. The rumour seems quite inconsistent with the news as to Mr. Stanley's having left the Aruwimi River on June 3 for Wadelai. Had he been shot, as reported, it must have been after this date, and during the land journey, whereas one version of the rumour gives out that he was killed on the Congo. He may meet with Emin Pasha sooner than he expected. Emin, it seems, is at present exploring on the south of the Albert Nyanza, endeavouring to find the connexions of the great affluent he discovered on the south side of the lake, and ascertain whether it may proceed from the Mwuta Nzige. So that he and Mr. Stanley may meet half way. Letters from Mr. Stanley are expected in this country early in August.

THE Report of Dr. Hans Schinz on his exploration of the German colony known as Luderitzland (South-West Africa) has just been published. Dr. Schinz made two journeys: the first, in 1884, from Angra Pequena to Am-Hub on the Xamob, a sub-affluent of the Orange; and the second, in 1885, across Namaqua-land and Damara-land, and the little-known region which separates Damara-land from the Cunene River. The Report contains much valuable information, especially on the flora and the people of the region visited. The region is quite as sterile and hopeless as it has been painted by previous visitors. It is only on the north of Etosha (18° S. lat.) that the flora and fauna become anything like abundant—bauhinia, palms, cassia, baobab. The population becomes more dense as we approach the Cunene. But three-fourths, if not four-fifths, of the new German colony is unworkable and uninhabitable.

IN the new number of *Timehri* the valuable serial published in British Guiana, will be found a condensed translation of Père de la Borde's "History of the Origin, Customs, Religion, Wars, and Towns of the Caribs of Antilles," the first of a series of reprints of the literature of West India and Guiana red men, which it is proposed to publish from time to time in the journal. A large part of the number is devoted to Mr. Im Thurn's notes on the plants observed during the Roraima expedition.

THE last Annual Report of the Russian Geographical Society for 1886, which has just reached us, contains a good deal of useful information. An account of several interesting journeys is given. The publications of the Society were numerous and valuable. Seven fascicules of the Memoirs appeared during the year, containing the work on the geology of Lake Baikal, by M. Tchersky; a hydrological inquiry into the Upper and Middle Amu-daria, by the late M. Zuboff; on the landslips at Odessa, by M. Jarintseff; on the exposure of thermometers, by M. Savelieff; on a journey to North-West Persia and the Transcaspian region, by M. Nikolsky; on the province of Olonets, by M. Polyakoff; and on the Votyaks, by M. Sokolovsky. The Society published, moreover, a volume of the "Works of the Siberian Expedition," containing Fr. Schmidt's "Miocene Flora of Sakhalin," and three volumes of observations of the Polar stations on the Lena and on Novaya Zemlya. It is good news that the addenda to the capital "Geographical Dictionary of Russia," by P. Semenoff, are being rapidly prepared for the press. The great gold medal of the Society has been awarded to M. Potanin for his twenty years' geographical work; and that of Count Lütke to M. Tchersky for his remarkable geological explorations around Lake Baikal and in East Siberia altogether. Other gold medals have been awarded to MM. Nalivkin for their work "On the Position of Woman amidst the Settled Population of Ferganah," published last year at Kazan; to M. Yastreboff for a work on Turkish Servians; to M. Makaroff for his researches into the double currents in straits; to MM. Skassi and Bolsheff for cartographical work; and to M. Eigner for his work at the Lena Polar station. Many silver medals have been awarded for works of less importance. The Committee of the Russian Geographical Society for Pendulum Observations and the Meteorological Committee have done most useful work.

THE TECHNICAL EDUCATION BILL.

I.

THE following is the text of the Bill to facilitate the provision of technical instruction:—

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. This Act may be cited as the Technical Instruction Act, 1887.

2. Any local authority as defined by this Act may pass a resolution that it is expedient to provide for supplementing by technical instruction the elementary education supplied in its district, and for that purpose to put in force the provisions of this Act.

3. (1) A local authority shall, before proceeding to carry into effect a resolution under this Act, cause the resolution to be published in the prescribed manner, and within the prescribed time, not being less than two months after the publication, fifty persons entitled to vote at the election of members of the local authority, or one-third of the total number of those persons, may, by a written requisition, require a poll of those persons to be taken as to carrying the resolution into effect, and thereupon the poll shall be taken in the prescribed manner, and in accordance with the prescribed regulations.

Provided that—

(*a*) the poll shall, so far as circumstances admit, be conducted in like manner in which the poll at a contested municipal election is directed by the Ballot Act, 1872, to be conducted; and, subject to any exceptions or modifications contained in any order of the Department of Science and Art made in pursuance of this Act, the Ballot Act, 1872, shall apply accordingly; and

(*b*) all persons entitled to vote at the election of members of the local authority shall be entitled to vote at the taking of the poll; and

(*c*) each of those persons shall be entitled to one vote only.

(2) If the resolution is negatived at the poll it shall not be carried into effect, and shall not be again proposed until the expiration of not less than twelve months after the taking of the poll.

(3) This section shall not apply to the metropolis as defined in the Elementary Education Act, 1870.

4. (1) For the purpose of supplementing by technical instruction the elementary education supplied in its district, a local authority may in pursuance of a resolution under this Act—

(*a*) Provide technical schools for its district; or

(*b*) Combine with any other local authority for the purpose of providing technical schools common to the districts of both authorities; or

(*c*) Contribute towards the maintenance, or provision and maintenance, of any technical school; or

(*d*) Make such arrangements as to the local authority seem expedient for supplementing by technical instruction the instruction given in any public elementary school in its district.

(2) The expenses incurred by a local authority for the purposes of this Act shall be defrayed out of the local rate.

(3) Provided that no payment shall be made under this Act out of the local rate in respect of a scholar unless or until he has obtained a certificate from the Education Department that he has passed the examination in reading, writing, and arithmetic prescribed by the standard set forth in the schedule to this Act (being the Sixth Standard fixed by the minutes of the Education Department in force at the passing of this Act) or an examination equivalent thereto.

(4) Two or more local authorities may, with the sanction of the Department of Science and Art, enter into any agreement which may be necessary for carrying into effect any resolution under this Act; and any such agreement may provide for the appointment of a joint body of managers, for the proportion of the contributions to be paid by the respective authorities, and

for any other matters which, in the opinion of the Department of Science and Art, are necessary for carrying out the agreement.

5. (1) Every school provided under this Act shall be conducted in accordance with the conditions specified in the minutes of the Department of Science and Art in force for the time being, and required to be fulfilled by such a school in order to obtain a grant from that Department.

(2) Those conditions shall, amongst other things, provide that a grant shall not be made by the Department of Science and Art in respect of a scholar admitted to the school unless or until he has obtained such a certificate from the Education Department as is herein-before mentioned.

(3) A minute of the Department of Science and Art not in force at the *passing of this Act* shall not be deemed to be in force for the purposes of this Act until it has lain for not less than one month on the table of both Houses of Parliament.

6. (1) Every local authority providing a school under this Act shall maintain and keep efficient the school so provided.

(2) For the purposes of providing and maintaining any such school a local authority shall have the same powers as a school board has for providing sufficient school accommodation for its district, but for the purposes of this Act the provisions of the Elementary Education Acts with respect to the exercise of those powers shall have effect as if the Department of Science and Art were substituted therein for the Education Department.

(3) Where a local authority has provided or maintains any such school, it may discontinue the school or change the site thereof, if it satisfies the Department of Science and Art that the school to be discontinued is unnecessary or that the change of site is expedient.

7. (1) The managers of any technical school in the district of a local authority may make an arrangement with the local authority for transferring their school to that authority, and the local 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 in pursuance of that section, shall apply in the case of arrangements for the transfers of schools in pursuance of this section, with this modification, that for the purposes of this section references to the school board shall be construed as references to the local authority, and references to the Education Department as references to the Department of Science and Art.

8. In this Act—

The expression "technical instruction" means 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, or in any other subject which may for the time being be sanctioned by that Department; and 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 "local authority" means a school board and the council of a borough for which there is no school board.

The expression "local rate" means—

(*a*) in a district for which there is a school board, the school fund;

(*b*) in a borough for which there is not a school board, the borough fund or borough rate.

The expression "the Education Department" means the Lords of the Committee of Her Majesty's Privy Council on Education.

The expression "prescribed" means prescribed by the Department of Science and Art.

9. In the application of this Act to Ireland the expression "borough" means a borough subject to the Act of the session of the third and fourth years of the reign of Her present Majesty, chapter one hundred and eight, intituled "An Act for the regulation of municipal corporations in Ireland," and the Acts amending the same.

SCHEDULE.

Standard VI.

Reading.—To read a passage from one of Shakspeare's historical plays, or from some other standard author, or from a history of England.

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 substituted for composition.

Arithmetic.—Fractions, vulgar and decimal, simple proportion, and simple interest.

II.

WE reprint from the *Times* of July 21 the following article on the Bill:—

The measure introduced late on Tuesday night by Sir William Hart Dyke, the Vice-President of the Council, may prove to be of far greater practical importance than many a measure that may for the moment loom larger in the public eye. It is a Government Bill for organizing throughout England and Wales at least the beginnings of a system of technical education. The Scotch Office is meanwhile preparing an analogous Bill for Scotland, which it is hoped will proceed *pari passu* with the English Bill through the House; and the Government intends, if possible, to carry both measures this session. It is quite time. There has been plenty of talk about technical education; and we want action in the matter. The need is admitted on all hands. It is a crying need, as much recognized in such authoritative statements as the Report of the late Commission as in the reports of examiners appointed by the Technical Institute at South Kensington. The former admits the great superiority of foreign nations over ourselves in this matter, and shows how both France and Germany make much more serious and successful attempts than we to train their workmen in the theory as well as in the practice of their trades. One result is the increased severity of foreign competition, from which British industry is suffering in all directions. What we lately stated, on the authority of the Committee of the London Chamber of Commerce, with regard to the competition of German with English clerks in London and the north may be applied, with little change, to the foreign workmen. They are not above learning their trade. They know that their bread depends on their excelling, and they strive to excel, with their Governments behind them, showing them, by carefully organized instruction, what is the best way. As yet, in England, we have done little more, by way of meeting this activity of our competitors, than to build a fine Institute at South Kensington. Not that that Institute is not doing good. Its very existence is a protest against the inveterate English belief in rule of thumb. It has as yet only touched the fringe of the questions before it; but, while it has done something positive by such means as teaching teachers, it has also done not a little to test the actual state of technical knowledge in many trades. Two months ago we called attention to the reports of its examiners, and pointed out how unfavourable on the whole they were. In the bleaching, dyeing, cotton-spinning, paper-making, carriage-building, and other industries, very few candidates showed any theoretical knowledge to speak of; on the one hand they were ignorant of the rudiments of chemistry, on the other of the rudiments of drawing. In a word, they failed to link the primary education which they might be supposed to have received with the business of their handicrafts.

The Government Bill proposes to do much to render this state of things less common. So far as can be judged from the Vice-President's speech, the Bill being not yet printed, it is a Bill for enabling local authorities—generally School Boards—acting in concert with the Science and Art Department, to provide technical instruction for pupils who have left the elementary schools, and in certain cases for those who have not yet left them. What the mover calls the operative clause enables local authorities to provide technical schools, and at the same time to combine with other local authorities by way of saving expense. The power of rating is given, but at the same time the ratepayers are to have a veto on "any proposal under the Bill." The combination clause, which permits the joint action to which we have referred, is that on which Sir William Hart Dyke relies to convince the public that his Bill will be cheaply and easily worked. Another, with the same object, is the clause which enables the local authority to make any arrangement which it may deem expedient for supplementing the technical instruction at present given in the schools. As to the agricultural districts, and the teaching of agricultural subjects, the Vice-President admits that his Bill will not do very much, and indeed, it would seem that the provision of that instruction, as was urged earlier in the evening, is beyond the power of the Science and Art Department. The question of London, and the London vote when debatable questions arise, is one which the Government have foreseen, but on which they can on vouchsafe some rather mysterious information. It would be "very wrong," says the Vice-President of the Council, to bring into force the "enormous voting power" of London on the question of forbidding some scheme of the local authority; and consequently he has put himself into communication with the London School Board, or rather with Sir Richard Temple, its Vice-Chairman, to devise a way out of the difficulty. With the result he seems particularly pleased, but, as the proposal of Sir Richard Temple is not made public, it is lawful to reserve our opinion. Then there is the question of the directing authority. It is not to be the Education Office; it is to be the Science and Art Department. Whether this will create any possible conflict of authorities it is difficult to say; but as those two bodies have the same head—the President and the Vice-President of the Council—it may be hoped that the conflicts will not be common or easy.

It is not to be supposed that such a Bill as this, which creates a new rating authority, and therefore threatens the pockets of the ratepayers, will pass into law without a good deal of criticism, or that it will be universally popular. Our correspondent, Mr. Daniel Watney, this morning gives utterance to a protest of which the language is strong, though the arguments are unconvincing. He admits that the old apprenticeship system has broken down, and that some substitute must be found; but anything like a general system of technical instruction, directed by the local authorities and the Science and Art Department, is condemned out of hand. Mr. Watney seems to think that the new proposal would give too much power to Professors, for whom he entertains the contempt of the "practical man." The practical man is commonly little more than an imperfect theorist; and just now, in England, his success in maintaining the commercial supremacy of the country is not such as to invest him with commanding authority. For our part we do not see where the Professors are to come in under Sir William Hart Dyke's Bill; but if they did come in, perhaps it might not be a bad thing for the improvement of our theoretical, and therefore our practical, knowledge. As to the immediate prospects of the Bill, it would seem from its reception on Tuesday night that the House is favourable to it. Mr. Mundella made two objections: one to the delegation of all power of initiation to the localities, and one to the exclusion of all pupils below the sixth standard. The objections stand on different grounds. The former is one of principle, the latter one of detail. It is not likely that the Government will venture, so late in the Session, and at a time when other difficulties have to be met and faced, to propose a sweeping measure for imposing technical instruction by the act of a central Department. The ratepayer must be humoured if his assent is to be won. As to the second objection, we think Mr. Mundella is probably right. The choice lies between retaining all children at school till they have passed the fifth standard, and admitting fifth-standard children to whatever technical classes may be available. It would be unjust to deprive them altogether, after they have left school, of the opportunity of learning whatever can be learnt about their trades.

SCIENTIFIC SERIALS.

Bulletin de la Société des Naturalistes de Moscou, 1887, No. 1.—The *Scaphirhynchus*, being an elaborate comparative anatomical description (in German) of the genus and its species, by N. Iwanzow (with two plates).—On the great comet (43) of 1886, by Th. Bredichin (with a plate).—Enumeration of the vascular plants of the Caucasus, by M. Smirnoff (in French). In this third paper the author discusses the relative moistness of the air in the Caucasus; he gives most valuable tables from twenty-three Caucasian stations, and shows the dependency of moisture upon the prevailing winds; he then gives tables as to the amount and frequency of rain in different parts of Caucasia, and discusses this climatic factor in comparison with the distribution of rains upon the Mediterranean region generally. This most valuable paper is to be continued.—On calorimetric methods for determining minimal quantities of iron in mineral waters, by E. Kislakovsky.—Comparative discussion of the data collected in Russia as to the epochs of the blooming of plants which are freely growing or cultivated between the 44th and 60th degrees of latitude, by A. Dœngingk, being a most valuable paper (in German), containing a list of the times of blooming of 270 different species at Pyatigorsk, Kishineff, Sarepta, Orel, Moscow, and St. Petersburg. This is followed by a note on the blooming of 225 plants at Pyatigorsk and Elizabethpol in the Caucasus, as also on trees and bushes, endemic and exotic, in the Caucasus, showing the origin of the exotic plants.—On the parasitical pteromalines of the Hessian fly, by Prof. Lindeman. Five parasites, all new species, are described (in German) and figured.—Entomological notes, by the same, on the *Haltica vittula* of Russia, the *Scotylus amygdali* of Transcaucasia, and the *Cleigastra flavipes* from Moscow.—On the tooth-plates of the *Gulnariæ*, by Dr. W. Dybowski (in German).—On remains of the *Ursus spelæus* in Transcaucasia, by N. Anutschin (in German).—On the species of *Taraxacum* and *Glycyrhiza*, and *Alhagi camelorum*, by A. Becker.

No. 2.—Comparative anatomical inquiry into the structure of the cord of fishes and its cuticular envelopes, by W. Lvoff (with three plates). A most elaborate inquiry into, preceded by an historical sketch of the literature of, the subject (summed up in German).—A study on the palæontological history of the Ungulatæ in America and Europe, by Mary Pavlow (in French). After having summed up the ideas developed on this subject by MM. Cope, Wortman, and Schlosser, the author studies the group of *Condylathra*, and shows that its separate members may have been predecessors of some orders of Mammalia; that it is a mixed group containing species which have the characters of Ungulatæ as well as of Unguiculatæ; and that it may be considered as standing at the head of the genetic tree of the Ungulatæ and Carnivores. Madame Pavlow shows, moreover, that the *Condylathra* have also representatives in Europe.—Notes on the remains of man and *Ursus spelæus* in Transcaucasia, by N. Anutschin.—The Hessian fly (*Cecidomyia destructor*) in Russia, by Prof. Lindeman (in German), being an elaborate paper on the history of its spreading, its habits and devastations, and its development (to be continued).

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, July 6.—Dr. D. Sharp, President, in the chair.—Mr. McLachlan remarked that at the meeting of the Society in October 1886 he exhibited a quantity of the so-called "jumping seeds" from Mexico, containing larvæ of *Carpocapsa saltitans*, Westw. The seeds had long ceased to "jump," which proved that the larvæ were either dead, had become quiescent, or had pupated; about a fortnight ago he opened one of the seeds, and found therein a living pupa. On the 4th inst. a moth (exhibited) was produced.—The President, on behalf of the Rev. H. S. Gorham, exhibited the following Coleoptera, lately taken in the New Forest: *Anoplodera sexguttata*, Fab., wholly black variety; *Grammoptera analis*, Fab.; *Colydium elongatum*, Fab.; and a specimen of *Tachinus elongatus*, Gyll., with brownish-red elytra.—Mr. S. Stevens exhibited a specimen of *Orsodacna humeralis*, Latr. (*lineola*, Panz., var.), taken by him at Norwood; he also exhibited a specimen of the same beetle taken by him fifty years ago in Coombe Wood; during the interval he had never seen it alive.—Mr. G. T. Porritt exhibited, on behalf of Mr. N. F. Dobrée, of Beverley, a series of about thirty specimens of a *Tæniocampa* he had received from Hampshire, which had previously been referred to as a red form of *T. gracilis*. Mr. Dobrée was inclined to think they were not that species, but *T. stabilis*.—Mr. A. C. Horner exhibited the following species of Coleoptera from the neighbourhood of Tonbridge:—*Compsochilus palpalis*, Esp. (5); *Acrognathus mandibularis*, Gyll. (4); *Homalota atrata*, Mann., *H. vilis*, Er., and *H. difficilis*, Bris.; *Calodera rubens*, Er.; and *Oxytelus fulvipes*, Er. He also exhibited a *Rhizophagus* from Sherwood Forest, which appeared to belong to a new species; and several specimens of *Holopedina polypori*, Först., also from Sherwood Forest, where he had found it in company with, and probably parasitic on, *Cis vestitus*.—Mr. Elisha exhibited two larvæ of *Zelleria hepariella*, Stn. Mr. Stainton remarked that as the greater part of the larvæ of *Zelleria* were attached to the Oleaceæ, it seemed strange that certain species had recently been found on Saxifrage.—Mr. Slater read a paper on the presence of tannin in certain insects, and its influence on their colours. He mentioned the facts that tannin was certainly present in the tissues of the leaf- wood- and bark-eating species, but not in the tissues of the carnivorous beetles, and that black colour on the elytra of certain beetles appeared to be produced by the action of iron on tannin. A discussion ensued, in which Prof. Meldola, Mr. Poulton, Dr. Sharp, and others took part.

PARIS.

Academy of Sciences, July 18.—M. Janssen in the chair.—On the transition between the aromatic and fatty series, by MM. Berthelot and Recoura. By the synthetic process this transition is effected very clearly in the polymeric transformation of acetylene into benzene, and in the allied pyrogenous reactions. Some light has also been thrown on the more obscure problem of the transition in living organisms by Prunier's experiments with quercite, and Maquenne's with inosite. These studies are here subjected to further investigation by the measurement of the heats of formation of the various principles, themselves deduced from the heats of combustion. In all cases the passage of a body belonging to the fatty series to one of the aromatic series by deshydratation is shown to be accompanied by a considerable liberation of heat; that is to say, by a loss of energy corresponding to the excess of stability acquired by the fundamental hydrocarbonated nucleus.—Comparative locomotion: action of the pelvic member in man, the elephant, and the horse, by MM. Marey and Pagès. Their recent researches on the locomotion of the horse and elephant enable the authors to establish certain analogies and differences presented by the posterior member of these quadrupeds compared with the movement of the lower member in man. The parallelism, which is illustrated by several diagrams, bears both on the slow and rapid motion (walking and running) of the three types here under consideration. Contrary to the general opinion, there appears to exist in the step or pace of the quadrupeds a period of double rest more prolonged in the hind than in the fore-quarters. It is also shown that the trot in the horse corresponds unquestionably with the running action of man, but that elephants have no such action, just as man lacks the gallop of the horse, which in this respect thus stands at the head of the series. But, when urged to quicken their speed, the elephants broke into an action somewhat approaching that assumed by man when passing from a walk to a run. In general, both in slow and rapid motion, the action of the pelvic member remains essentially the same in all three types. The difference between them lies in the action on the concurrent limbs, which is slight between man and the elephant, much greater between these two and the horse.—On the habits of Phylloxera, and on the present state of the French vineyards, by M. P. Boiteau. During the year 1886 the author continued his experiments on the reproduction of Phylloxera, which he has cultivated for six consecutive years. In 1885 he had reached the nineteenth generation by the parthenogenetic process, all necessary precautions being taken to prevent fertilized insects from coming in contact with those derived directly from the winter egg. At present he has reached a second generation for 1887, or a total of 24 or 25 altogether, all these agamous generations being very healthy, lively, and prolific. The condition of the vines, which suffered so much last year, is described as highly satisfactory, with every prospect of a good vintage in most of the wine-growing districts.—Comparison of the energies radiated by platina and silver in fusion, by M. J. Violle. By the process here described the total radiation of platina is found to be 54 times that of silver in fusion. Yet this relation, great as it is, is far less than that of the luminous intensities, which is superior to 1000.—Solidification of liquids by pressure, by M. E. H. Amagat. Theoretically, J. Thomson's formula implies that at a given temperature solidification becomes possible under sufficient pressure, provided the density be greater in the solid than in the fluid state. But no instance has hitherto been known of any liquid properly so called being solidified by pressure alone. Now, however, the author, after numerous experiments, has succeeded in solidifying the bichloride of carbon (C_2Cl_4), obtaining some crystals which are here figured, and which appear evidently to belong to the cubic system. This substance is solidified at the temperatures of $-19^\circ\cdot5$, 0°, 10°, and $19^\circ\cdot5$ C. under the respective pressures of 210, 620, 900, and 1160 atmospheres. This and other results would seem to imply that every fluid has a critical point of solidification; that is, a temperature above which solidification will take place under no pressure: just as there appears to be a temperature below which the body remains solid under the slightest pressures.—On the calorific conductibility of bismuth in a magnetic field, by M. A. Righi. The considerable increase of electric resistance, and the intense rotation of the equipotential lines (Hall's phenomenon) which occur when bismuth is introduced into the magnetic field, naturally led to the inference that a decrease of calorific conductibi[lity] and a rotation of the isothermal lines should take pl[ace] under the same conditions. The author has now comple[ted] a series of extensive experiments, which completely confirm [this] supposition, and the summary results of which have been p[ub]lished in the *Resoconti dell' Accademia Reale dei Lincei* for J[uly] 12; that is, eight days before the analogous communicat[ion] recently sent by M. Leduc to the *Comptes rendus*.—On *Chlorœma dujardini* and *Siphonostoma diplochaitos*, by Joyeux-Laffuie. In reply to M. Kunstler, it is pointed out t[hat] there is no ground for supposing that these two organisms [are] identical, the former being from 15 mm. to 20 mm., the la[tter] 8 cm. long.—On the earthquake of June 9, 1887, in Cent[ral] Asia, by M. Venukoff. A detailed account is given of [the] disastrous effects of this disturbance, especially in Vernoï, [a] town of 17,000 inhabitants, where 1700 out of 2500 buildings [of] brick and stone were levelled with the ground, while 8[00] wooden houses remained almost uninjured. As many as 2[..] persons perished in Vernoï, and over 800 in the surround[ing] district, chiefly in the Ala-tau Mountains. The first great sho[ck] of June 9 has been followed by several others, which still contin[ue], obliging the inhabitants to take shelter under tents on the op[en] plains.—On a hailstone inclosing a stony nucleus, by M. Tissandier. This specimen fell during a violent thunder a[nd] hailstorm in the Tarbes district on June 20. The nucleus co[n]sisted of some gypsum, which had clearly been worked, and [no] doubt sucked up by a water-spout to a thunder-cloud, where [it] became incrusted with ice.

BOOKS, PAMPHLETS, and SERIALS RECEIVE[D]

Course of Practical Instruction in Botany, part ii.: Bower and Vine (Ma[c]millan).—The Teaching of Geography: A. Geikie (Macmillan).—Sunlig[ht], Second Edition, 1887 (Trübner).—Morality and Utility: G. P. Best (Trübne[r]).—The Scenery of Scotland, Second Edition: A. Geikie (Macmillan).—T[he] Forms of Nasal Obstruction: G. Macdonald (A. P. Watt).—Report of t[he] Royal Commission for the Colonial and Indian Commission, 1887 (Clowes).—Smithsonian Report, 1885, part i. (Washington).

CONTENTS. PA[GE]

THURSDAY, AUGUST 4, 1887.

THE JUBILEE OF THE ELECTRIC TELEGRAPH.

IT is something to have lived to take part in an epoch-making event. Many monarchs have celebrated their jubilees: printing, steam, gas, have passed through this period in silence and disregard, but the first practical application of electricity has commemorated the fiftieth anniversary of its birth with an *éclat* and success that reflect the highest credit on the managers of the banquet who brought together such a distinguished gathering on July 27.

It is remarkable that of all who were present not one took part at the birth of the electric telegraph. The pioneers are gone, and their memory was silently toasted. Of those associated with Cooke, William Watkins, who carried out his early experiments, and who put up the first overhead wires between Paddington and Slough, alone remains, but the inexorable duties of the law compelled his attendance on a special jury at Exeter on the day when he ought to have been present at the Holborn Restaurant. No collaborateur of Wheatstone in his early work exists. John Greener, who had charge of the telegraph on Bidder's celebrated rope railway between Fenchurch Street and Blackwall in 1842, was there, and many like Henry Weaver and J. R. France can date their telegraphic career from the incorporation of the first telegraph company in 1846.

One of the most interesting features of the meeting was the gathering around Mr. Edwin Clark of his old lieutenants. Edwin Clark's reforms in the early days of telegraphy (1850-54) still bear fruit. The footprints on the sands of telegraphic time are nowhere so deeply impressed as on the ground traversed by Clark. His mode of insulation, his underground work, his instruments, his test-boxes, still remain a type of English telegraphy everywhere. His work was well carried out by his brother and successor, Latimer Clark, and it is continued even in the present day by his pupil, Preece.

The success of telegraphy in this country is due essentially to the superposition of scientific method on to the rude rules of practice. The rule-of-thumb principles of the early engineers were inoperative in telegraphy, for the exact laws of Ohm, Ampère, and Coulomb, the experimental skill of Faraday, Joule, and Grove, the mathematical genius of Helmholtz, Thomson, and Maxwell, have kept our electricians in the straight path, and prevented them from wandering in the wilds of guess-work and in the labyrinth of tentative troubles. It is impossible to say how much this influence has been reflective. The science of electricity has been indebted as much to practice as practice has been indebted to science. Submarine telegraphy chronicles no failure. The first Atlantic cable raised the curtain. The conditions were evident. Thomson stepped in, and all was light.

To telegraphy "all the world's a stage." The inventor has no nationality. Alongside of Wheatstone we find Morse and Siemens, Meyer, Hughes, and Edison, La Cour, Varley, Leclanché, and Minotto. This polyglottism is seen in the nomenclature of the units of measurement, ohm, farad, ampere, and coulomb, the only universal system of measurement, excepting that of time, extant.

Telegraphy, without which railway traffic would be impossible, has followed the growth of railways, and it has revolutionized the procedure of commerce. Hence the great commercial nations, England and the United States, show the greatest development of its progress.

One regretted to hear so little said about the great commercial spirits who set the ball a-rolling. John Pender, Cyrus Field, Tom Crampton, deserve all that was said of them, but where were Ricardo and Scudamore in England, Orton and Vanderbilt in America?

The story as told by the Postmaster-General reads like a romance of fairyland. The first five-needle instrument of Cooke and Wheatstone required five wires to transmit at most five words a minute: now five wires can transmit 2500 words in the same time.

We can pride ourselves in England on being in advance of all other nations not only in the development of the business of telegraphy, but also in the invention and perfection of apparatus. It is something to have in ten years increased the capacity of the wires for the transmission of messages *tenfold*, and to have done that without patent, or any reward but the consciousness of having done well. Government officials are unfortunately placed in this respect. It is improper to patent an invention developed in the discharge of duty, while they are singularly liable to be assailed by the daily Press for their supposed shortcomings. The work they do is only known by their own writings, when they are allowed to write; and even then they are subject to unfair and dubious criticism. The Press takes no trouble to find out what is done. The feeling is, "What good can come out of Nazareth?" Yet the introduction into the Post Office system of high-speed repeaters and of shunted condensers marks two epochs as successful, eventful, and meritorious as the introduction of duplex, of quadruplex, or of multiplex working. We were told that the rate of working between London and Dublin had gone up from 50 to 462 words a minute. *One* cable will do the work of *ten*. What has been the reward? We venture to say, *nothing*; and that the Lords of the Treasury are profoundly ignorant of the good work that is being done in the service over which they preside—work which they are just as likely to reward with a kick as with a half-penny.

The jubilee is now over, and we have every reason to feel proud that Mr. Raikes, the present Postmaster-General, Sir Lyon Playfair and Mr. Shaw-Lefevre, his predecessors, had such excellent tales to tell, and so gracefully assisted at so successful a gathering.

THE CLASSIFICATION OF ALGÆ.

Till Algernes Systematik. Nya bidrag af J. G. Agardh. (Femte afdelningen.) Transactions of the University of Lund, Tom. XXIII., 4to, pp. 180, 5 plates.

THE indefatigable Dr. Agardh has recently issued the fifth instalment of his work on the systematic classification of Algæ. Although it bears a Swedish title, the work is in Latin. The subject treated is the interesting group of the Siphoneæ.

Dr. Agardh mentions that but few observations have been made upon the fruit of the Siphoneæ; but the little that is known on this subject proves that great differences exist in the fructification of these Algæ. Thus the organs of reproduction in Vaucheria differ from those of Botrydium, while those of the latter vary from those of Bryopsis, Codium, Dasycladus, and Acetabularia. Of many other genera observations are deficient or in various respects uncertain and inconclusive. For interesting general remarks on the fructification of the Siphoneæ the reader is referred to page 10, and for special details to the observations on each genus and on the fruit of such of the species as are best known.

Setting aside the true characteristics of the fruit, it becomes a question by what characters of the structure the Siphoneæ are to be distinguished from the Conferveæ and Ulvaceæ, and their affinities determined. In Dr. Agardh's opinion these characters are to be found in the filaments or utricles of the frond, being tubular, and not, as in the Confervaceæ and Ulvaceæ, consisting of subdivided cells. Good examples of the former are afforded by Caulerpa and Valonia, whose fronds, roots, stems, branches, and ramuli, though distinct, consist of a single cell. These remarks are followed by observations on the comparative structure in different genera and their affinities with each other.

With the exception of Vaucheria and Botrydium, says Dr. Agardh, the Siphoneæ inhabit the sea. This is unquestionably the case as regards Botrydium, but it may be asked whether it be quite true as to Vaucheria, several of the British species of which are recorded by Dr. Nordstedt ("Remarks on British Submarine Vaucheriæ," Lund, 1886) as growing at the lowest tide-marks, and one species (*V. piloboloides*, "probably in quite salt water." The greater number of the Siphoneæ are natives of the warmer seas, and are especially abundant on the shores of rocky islands of which the principal constituent is lime. They spread their fibrous roots among the sandy *débris*, and are thus useful in holding together the particles of sand.

Some of the Siphoneæ have creeping stems, as have the Caulerpeæ. These plants, by extending the network of their creeping stems and roots over the sand, seem to exercise on the coast, within tide-marks, the same functions as the Maram (*Psamma arenaria*). This plant grows on the coast of Norfolk, and is found so useful in holding together the particles of sand, and thus aiding in the formation of land and preventing the inroads of the sea, that strict regulations are in force to prevent its destruction. In the same manner as the Maram spreads over the dry sands, the Caulerpeæ extend on the seashore within tide-marks, and are thus uncovered at low water. When the tide is out, they resemble green meadows. The utility of these plants in protecting the land was, a few years ago, unexpectedly proved in the neighbourhood of Adelaide, South Australia. A farmer suffered his sheep to stray upon the coast where the Caulerpeæ were exposed at low water. The sheep devoured the Algæ; the sea consequently broke in and established itself, and land was thus permanently lost. Many species, as Halimeda and Penicillus, have roots which are occasionally as large as small hens' eggs, formed of innumerable branched fibres which penetrate deeply into the sand. Some of the stipiform species emit flagelliform creeping "propagula," from which spring new plants; hence, observes Dr. Agardh, the Siphoneæ may be said to be social plants. Some Algæ, as Anadyomene, grow in shallow water exposed to the full influence of light, while others, like Bryopsis, prefer deep water to which light scarcely penetrates.

Many, but not all, of the Algæ belonging to the Siphoneæ have, like the Corallina family, the power of absorbing lime from the water. Young plants are generally green, but the incrustation of lime, in certain species, increases with age. In some genera it is entirely absent, as in Codium; in others it is extremely slight; while in some species of Halimeda the whole frond is frequently cased with a hard coating of lime, and looks like a gigantic frond of Corallina.

The disposition of the families of which the Siphoneæ are composed must be attended with some difficulty until the fruit is more perfectly known. In the interim Dr. Agardh proposes the arrangement adopted in the work. The group has been considerably enlarged, including as it now does the Dasycladeæ and Valoniaceæ. With regard to the former, the author observes that the Dasycladeæ are quite distinct from all the other genera, with their verticillate stems and external sporangia; hence he considers that they undoubtedly form a natural family. The fructification nevertheless varies in different genera.

There appears no doubt as to the limits of the Caulerpeæ. These are set forth in the first part of the present work ("Till Algernes Systematik"), and, although some observations are still wanting as to the fructification, the Caulerpeæ form a very distinct family.

The limits of the Valoniaceæ are very difficult to determine. In the form and size of their cells they for the most part are very near the Caulerpeæ. They are little more than ramulose prolifications; hence the ramification, although it is in some species more or less obscured, may be said to have a common character. But little is known of the fruit of these plants. In some genera the structure varies from that of the other Valoniæ, and approaches near to that of Ulva, as may be seen in Dictyosphæria and Anadyomene.

Among the remaining genera are some whose fronds consist almost entirely of compound tubes incrusted with lime. In these the normal ramification is di-trichotomous, but in Bryopsis and Codium—which, however, are not coated with lime—it may be said to be pinnate. Of the fructification of these plants few observations are recorded. In *Udotea Desfontainsii* and *Halimeda tuna*, true sporangia have been observed. Both belong to the Udoteaceæ. Whether the Spongodieæ, with their *quasi*-composite fronds, and the Bryopsideæ, with their free filaments, should be separated from each other, or united into one family, may be subject for consideration.

Vaucheria and Botrydium are not treated in the present work: neither does Dr. Agardh know to which family of the Siphoneæ they should be attached.

The whole group, in which the Dasycladeæ and Valoniaceæ are now included, is thus arranged by Dr. Agardh:—

I. BRYOPSIDEÆ.

(1) Bryopsis; (2)? Derbesia.

II. SPONGODIEÆ.

(3) Codium, ?? Cladothele.

III. UDOTEACEÆ.

(4) Chlorodesmis; (4*a*)? Avrainvillea; (5) Espera; (6) Penicillus; (7) Rhipocephalus; (8) Callipsygma; (9) Udotea; (9*a*)? Rhipidosiphon; (10) Halimeda.

IV. VALONIACEÆ.

(11) Valonia; (12) Siphonocladus, ? Ascothamnion, ? Trichosolen; (13) Apjohnia; (14) Struvea; (15) Chamædoris; (16) Dictyosphæria; (17) Anadyomene.

V. CAULERPEÆ.

(18) Caulerpa.

VI. DASYCLADEÆ.

(19) Dasycladus; (20) Chlorocladus; (21) Botryophora; (22) Cymopolia; (23) Neomeris; (24) Bornetella; (25) Halicoryne; (26) Polyphysa; (27) Acetabularia, ? Pleiophysa.

It will be observed that the position of Derbesia, Cladothele, Avrainvillea, Rhipidosiphon, Ascothamnion, and Trichosolen are not yet finally determined. Neither does the author yet see his way to include Chlorodictyon (*Ast. Holm.* 1870, Öfversigt No. 5, p. 427, tab. iv.) in the present arrangement. Codiolum is also excluded. The genus Balbisiana is mentioned (p. 10) once, but is not again referred to. Has *Bryopsis Balbisiana* been formed into a distinct genus under this name?

Under Avrainvillea Dr. Agardh includes the Fradelia of Chauvin, the Chloroplegma of Zanardini, and the Rhipilia of Kützing. This genus has the habit of Udotea, a cylindrical stem, a coriaceous, flabellate frond, of a very dark colour, with lacerated apex, forming irregular lobes, in which the zones of Udotea are absent. Of the fruit nothing appears to be known.

Of Rhipidosiphon there is no description, and very little seems to be actually known about this Alga. To Dr. Agardh it appears to be a young plant of Udotea.

Our knowledge of Ascothamnion (*Valonia intricata*, C. Ag.) is very limited, although the plant has been found in most of the warmer seas.

Trichosolen is a native of the Antilles, where it was found by Montagne. With Pleiophysa Dr. Agardh is acquainted only through Kützing's figure (*Tab. Phyc.*, vol. xvi. tab. 1). The habit and form of the sporidia agree with those of Halicoryne.

Penicillus Phœnix now appears as *Rhipocephalus Phœnix*. The new genus Callipsygma is founded on an Australian Alga which bears a certain resemblance to the last-mentioned plant. The former has an undivided terete stem incrusted with lime, while in the latter the stem is two-edged, without incrustation, and from the margins issue pinnate ramuli. The fruit of both genera is unknown.

Chlorocladus is between Dasycladus and Botryophora = *Dasycladus occidentalis*. These three genera are especially distinguished from each other by their fruit.

Of the whole group of the Siphoneæ three genera only have representatives on the British shores. These three genera are Bryopsis, Derbesia, and Codium. They have all a wide range. Of the nineteen species of Bryopsis, two are natives of these shores. Derbesia ranges from the Adriatic to the Faroe Isles and Norway. Dr. Agardh does not seem to be aware that *D. tenuissima* has been found on the British coast. Although Codium has so extensive a range, no species has yet been recorded from the east coast of the United States. That remarkable plant, *C. bursa*, which is found on the southern coast of Britain, the Mediterranean and Adriatic, has recently been obtained from Victoria, Australia. On the Sussex coast it may sometimes be picked up after storms. Its range in depth of water is about the same as that of *Rytiphlœa pinastroides*, with fragments of which, when hollow and torn, the frond is sometimes filled. *Sphacelaria plumula* grows on it occasionally. Dr. Agardh mentions that three or four fronds often grow together. The writer possesses a specimen from Brighton, which consists of a group of ten fronds, one of which is fixed to a piece of chalk; the others grow upon one another, a few filaments attaching the young plants to the older ones. In 1870 the Rev. E. S. Dewick was fortunate enough to pick up a specimen at Eastbourne, which, on examination, proved to be in fruit. He stated at a meeting of the Eastbourne Natural History Society (November 18, 1870) that "the Coniocystæ are produced on the outer surface of the clavate filaments, and differ from those of *C. tomentosum* only in being nearer the top of the filaments, and smaller in proportion to their size."

Codium tomentosum was reputed to be nearly cosmopolitan. Dr. Agardh, however, shows that several species have been included under this name, and that the so-called Australian forms belong to *C. Muelleri*, *C. Galeatum*, and *C. mucronatum*. In the last two species the utricles are mucronate, as represented in Plate 1, Figs. 1, 2, 3. *C. elongatum*, in which the frond, instead of being cylindrical, as in *C. tomentosum*, is compressed, is recorded by Dr. Agardh from Ireland. This fact is worthy the attention of British algologists. *C. latum*, found by M. Suringar on the coast of Japan, is not referred to in Dr. Agardh's work, neither is the plant, apparently allied to Codium, called by M. Suringar Acanthocodium (see "Alg. Jap.," p. 23). This also is a native of Japan, and probably but very little known.

Although so many points in the history of the Siphoneæ are still undetermined, this work of Dr. Agardh's will be found full of interest and instruction.

MARY P. MERRIFIELD.

AMERICAN MINING INDUSTRIES.

Report on the Mining Industries of the United States (*exclusive of the precious metals.*) By R. Pumpelly, 4to, pp. xxxviii.–1025. (Washington: Government Printing Office, 1886.)

THIS, the fifteenth and final volume of the Reports illustrating the results of the census of the United States taken in 1880, is in great part devoted to descriptions of the principal districts producing iron ores in the United States, the condition of the mines during the census year being studied in considerable detail, and in many cases illustrated by sketches of the workings. A very large number of samples of the ores of the different mines were collected by specially appointed agents, who visited every district and almost every mine of importance, and these were examined by a chemical staff at a special laboratory at Newport, Rhode Island. It was originally intended to make complete analyses of the greater number of the 1400 samples so collected, but the early exhaustion of the funds voted for the census necessitated an extensive curtailment of the plan, and only the more important minerals from the older rocks were completely analyzed; while for the bulk of the remainder, the properties of the more important constituents, iron, phosphorus, and sulphur, were alone determined, and the presence of titanium and manganese noted incidentally. The total number of samples investigated was 1250, 53 being completely and 1157 partially analyzed. The description of the methods of analysis adopted, and the tabulation of the results, occupy about a hundred pages, in addition to the 500 devoted to the geology and topography of the iron ore mines and their statistics.

The section devoted to coals, occupying eighty-seven pages, is mainly statistical, and has a very valuable introduction by Dr. Frederick Prime, Jun., which is perhaps the best condensed account of the nature and distribution of American coals that has yet appeared. A third section on the Cretaceous coals and lignites of the North-West is the result of an extensive exploration of the country traversed by the Northern Pacific Railway, made by the author subsequently to the completion of the census work proper, in 1882. This work, under the title of the Northern Transcontinental Survey, was suddenly stopped after about £20,000 had been expended upon it; and in order that the results might not be lost the observations have been reduced, analyses of the coals have been made, and a systematic memoir on the whole subject has been produced, which, although not exactly in the place where we should expect to find it, is too valuable an addition to American geology not to be welcomed in spite of its incongruous surroundings. The statistics of the base metals and minor minerals, occupying the remainder of the volume, are now of comparatively little interest, as these subjects have been treated from year to year in the returns published by the United States Geological Survey, and are available up to 1885. It must, however, be remembered that it is only in census years that returns from individual establishments can be obtained, and that therefore the figures for those years may be regarded as more authoritative than those of other dates. In any case, statistics five years old are tolerably ancient history.

In conclusion, we must call attention to the author's introductory paper on the geographical and geological distribution of the iron ores of the United States. This is a masterly abstract of the main subject of the book, and will be particularly useful to those who may wish to acquire some knowledge of the basis of the American iron industry without searching through the great mass of reports and surveys in which most of the detailed information is to be found. A plate of comparative sections of the strata in the principal iron-ore producing States is especially interesting as showing how the most important ore deposits are confined to the older rocks, such as the Archæan regions of New York and New Jersey, the Huronian of Michigan and Wisconsin, and the great stratified belt of hæmatite or "fossil ore" in the Clinton group of the Upper Silurian; while the most important iron-bearing strata of this country and Western Europe, the Lias and Lower Oolitic series, are entirely absent. Although the great activity of the iron trade in 1880-81 was the cause of very energetic explorations, very few discoveries were made in the older producing districts, and it became evident that to make these it was necessary to go into new fields, and in any case the author considers that the accessible rich ores may perhaps be practically exhausted within the life of the present generation. It will then be necessary to fall back upon the leaner kinds, containing from 30 to 45 per cent. of iron, which are known to exist in vast quantities, though generally far removed from coal suited for smelting purposes.

H. B.

OUR BOOK SHELF.

Theory of Magnetic Measurements. By F. E. Nipher, Professor of Physics in Washington University. (London: Trübner and Co., 1887.)

THIS little work is intended to furnish information as to the practical details of a magnetic survey. The description of the instruments used is poor. Full details as to the necessary calculations are given. The directions for the use of the instruments involve in a few cases unnecessary precautions, while in others the method suggested appears rather rough. Thus the statement that it is advisable not to make any observations with a dip needle till ten minutes after magnetization, is not, we think, borne out by experience. On the other hand, the suggestion that the vibrations of a declination needle may be checked by the finger would be likely to mislead beginners. It would have been better to describe the method of bringing the magnet to rest by means of a small auxiliary magnet. On the whole, English students will probably find all that they want, and with more direct reference to the Kew pattern instruments, in Stewart and Gee's "Practical Physics," and are thus not likely to make much use of Mr. Nipher's work. A. W. R.

Studies in Life and Sense. By Andrew Wilson, F.R.S.E. (London: Chatto and Windus, 1887.)

PREVIOUS works of this kind by Dr. Andrew Wilson are so well known, that a very few words will suffice to introduce the present one to the notice of our readers. It consists of a re-publication of essays on biological and psychological topics, which the author has from time to time contributed to sundry magazines. Although there is little or no attempt at originality, the collection is well calculated to prove of use and interest to general readers. The style is everywhere entertaining, and the following is a list of the subjects treated:—"Human Resemblances to Lower Life," "Some Economics of Nature," "Monkeys," "Elephants," "Past and Present of the Cuttle-Fishes," "Migration of Animals," "The Problems of Distribution," "Songs without Words," "The Laws of Speech," "Body and Mind," "The Old Phrenology and the New," "The Mind's Mirror," "What Dreams are made of," "Coinages of the Brain," "The Inner Life of Plants," "An Invitation to Dinner."

Fermenti e Microbi. Saggio di Igiene Antimicrobica di Italo Giglioli. (Napoli, 1887.)

THIS book may be considered as marking a new departure in the teaching of hygiene. The enormous advances that have been made of late years in the recognition of pathogenic microbes, their life-history, and the conditions affecting them one way or another, have added a large and important chapter to the study of sanitary science. It is this particular subject in all its bearings on sanitary science which is treated in the volume by Prof. Giglioli. The study of ferments, like yeasts, forms the introduction: their life-history, physiological and chemical action, are described, and, owing to the accurate knowledge that we possess of them—thanks in a great measure to the researches of M. Pasteur—they form a fit starting-point in the study of schyzomycetes, bacteria, or microbes proper.

The book treats of microbes from every aspect, morphological and physiological. The relation of microbes in general to the nutritive media, their chemical products, and the relation of these to the microbes themselves; the production of soluble ferments by them; the influence of light, heat, &c., are passed in review and treated fully.

The pathogenic organisms are next considered. Their relation to the animal body; the means by which they gain access to the animal system; the various influences commonly understood to constitute "predisposition"; the relation of pathogenic bacteria to food, air, soil, and water; the adverse influences, such as heat and light, disinfectants and antiseptics, &c., are all discussed with great lucidity and thoroughness.

There is hardly any aspect under which the study of pathogenic microbes—including the question of attenuation—presents itself, which is not discussed in this volume. The arrangement of the subject-matter is systematic, and the method of treatment does great credit to the author, inasmuch as he is, as far as possible, objective. He carefully weighs and sifts evidence, and does not disdain to make references to the literature of England and France. He has, in fact, carefully read the literature of this country on infectious diseases, and thus attests that he is not guided by that spirit of narrowness which one often meets with in modern German works.

An English translation would, we have no doubt, be a valuable addition to our own literature. E. KLEIN.

Photography of Bacteria. By Edgar M. Crookshank, M.B. (London: H. K. Lewis, 1887.)

SINCE Koch first employed photography in bacteriology ("Biol. d. Pflanzen," 1877, ii. 3) various attempts have been made in this country and on the Continent to advance the methods of photographing microscopic objects, such as Bacteria, with high magnifying powers. About fifteen years ago Dr. Woodward, of Washington, published photographic plates of histological objects taken under tolerably high magnifying power (400 and 500 diameters). These plates were brought out by the Surgeon-General's Office, Army Medical Museum of the United States: they attracted at the time a good deal of attention owing to their comparatively high excellence. That good photographs of histological and other microscopic objects are of great value in themselves, owing to their exactness, and the various advantages for purposes of publication, may be taken as requiring no further proof, and it seems equally obvious that indifferent photographs are of less value than accurate drawings.

Now, comparing Dr. Crookshank's photograms of histological and bacteriological objects, published in the present volume, the former with those of Dr. Woodward, the latter with those of Koch, there can be little doubt that no real advance has yet been made in producing photograms that are to take the place of accurate drawings. By saying this I do not mean to convey the impression that in Dr. Crookshank's volume there are not some good photographs—*vide* his Plate XVI., further his Figs. 7, 8, 30, 35, and 45, all of which are really fine in many respects—but taking photography as a whole, as applied to the representation of microscopic objects under high powers, I think that the time has not yet come when it can be said to have supplanted good and accurate drawings. In connexion with this it must certainly appear remarkable that in the numerous and important publications on Bacteria by Koch and his pupils since 1877 to the present time we do not find a single illustration represented by micro-photography. All their published illustrations are drawings.

With the new apochromatic objectives and projection eye-pieces by Zeiss better results may be looked for, and Dr. Crookshank, with his great skill in, and knowledge of, the technique, will, we have little doubt, be able to produce them.

As a clear and detailed account of practical microphotography, Dr. Crookshank's book is of great merit, and will prove very useful and important. As the first treatise on the subject in any language it is sure to command a high place. E. KLEIN.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Sense of Smell in Dogs.

IT is, I think, of some interest to supplement the very striking and exact experiments of Mr. Romanes on the scent of dogs, by an account of some experiments of a like kind made with a very different kind of dog, viz. a pug bitch. She was taught to hunt for small pieces of dry biscuit in a good-sized dining-room. The dog was put out of the room and a small piece, not much bigger than a shilling, of dry Osborne biscuit, was hidden; and as long as the hiding-place was accessible to the dog she never failed to find it. Sometimes the biscuit would be placed under a heap of a dozen or more newspapers on a dinner waggon, sometimes under a footstool, or sofa-cushion, or fire-shovel, and on two or three occasions in the foot of a boot which had been just taken off, the hiding body being always carefully replaced before the dog was admitted into the room, and without exception the biscuit in a very short time was discovered. It was over and over again proved that the dog did not follow the trail of the person who had hidden the biscuit; often the dog went by a different route, and in some cases one person hid the biscuit and another opened the door.

The experiment which has now special interest is the following one. A small piece of biscuit was placed on the floor under the centre of a footstool which was one foot square and six inches high, and standing on feet which raised it one inch from the ground. The dog, from the way in which she would set about moving the stool, not a very easy thing to do, as it stood in an angle of the wall, was evidently certain that the biscuit was beneath, and as scent seemed the only means by which she could have come at this conclusion, I thought to entirely mask this scent and prevent her finding the biscuit by pouring eau-de-Cologne on the stool. I found, however, it had no such effect, the biscuit was as readily and surely found when the eau-de-Cologne was there as when absent. It seems, then, that not only well-worn boots leave behind a recognizable odour, as Mr. Romanes proved, but also that to us at least so odourless a substance as dry plain biscuit emits so much and so characteristic a smell that it immediately spreads, even through considerable obstacles, to a distance of several inches in a few seconds, for in most cases the biscuit was found in thirty to sixty seconds after it had been hidden; thus time was not allowed, one would think, for all the surroundings of the hiding-place to become saturated with the scent.

W. J. RUSSELL.

Units of Mass, Weight, and Force.

MIGHT I venture to suggest to Prof. Greenhill that it would be very interesting to mathematicians, and probably would throw great light on the above subject, if he would give us quotations from some work by a practical engineer in which the idea of *inertia* distinctly appears. Or, failing this, perhaps Prof. Greenhill could give practical instances (other than problems in gunnery) in which *mass* quite apart from weight enters into the engineer's calculations.

It seems to me that many practical engineers never have occasion to deal with acceleration, except that of circular motion, and consequently only need to consider the *weight* of stuff, and have no use for the dynamical unit of force.

Gonville and Caius College, July 23. JOHN B. LOCK.

Chemical Affinity and Solution.

IN continuation of my inquiry into the relation between chemical affinity and solution (NATURE, vol. xxxiii. p. 615, and and vol. xxxiv. p. 263) I would direct attention to some remarkable facts in connexion with the heats of formation of the sulphates. Take H_2SO_4Aq, and assume that SO_3 acts on the O of water with the average energy with which the S acts on O_3, and we have the following :—

$$[H_2,O] = 68360 \qquad [H_2S,O_4Aq] = 210770$$
$$[S,O_3] = 103240$$
$$[S,H_2] = 4740$$
$$[SO_3,O] = 34413$$
$$210753 = 210770$$

Now consider $BaSO_4$. We have $[Ba,S,O_4] = 338070$ and—

$$[Ba,O] = 124240$$
$$[S,O_3] = 103240$$
$$\text{Difference} = 110590$$
$$338070 = 338070$$

The difference 110590 is almost exactly equal to $[Ba,S] = 109600$, so that the heat of combination of BaO with SO_3 is practically equal to [Ba,S], and the whole of the affinity of S is used up so that it has no power to act on the O of water, and hence the salt is insoluble.

Take again in the same manner $SrSO_4$, and the result is even more striking—

$$[Sr,O] = 128440 \qquad [Sr,S,O_4] = 330900$$
$$[S,O_3] = 103240$$
$$\text{Difference} = 99220$$
$$330900 = 330900$$

Difference $99220 = [Sr,S] = 99200$, and again we have an insoluble salt. This seems to me pretty strong evidence that the cause of these combinations is the affinity of S for the metal, and that the S cannot act on the water to cause solution, because of its intense affinity for the metal. Further, the heat of neutralization is the difference between the heat of solution of the oxide and SO_3 on the one hand, and the heat of [MS] on the other, thus :—

$$[SrO,Aq] = 29340$$
$$[SO_3,Aq] = 39153$$
$$\text{Neutralization} = 30710$$
$$99203 = [Sr,S] = 99200$$

and so on in other cases.

Now examine $CaSO_4$, which is a sparingly soluble salt, and note the difference, we have—

$$[Ca,O] = 130930 \qquad [Ca,S,O_4] = 318370$$
$$[S,O_3] = 103240$$
$$\text{Difference} = 84200$$
$$318370 \qquad 318370$$

This difference, 84200, is not equal to [Ca,S], which is =92000, or 7800 units more, and accordingly we find this salt slightly soluble with a heat of 4440 units, because the S is somewhat free to act on water. Further, we have the remarkable fact that $CaSO_4$ combines with $2H_2O$, and evolves in so doing 4740 units of heat, which is exactly equal to $[S,H_2]$. Evidently the whole of the affinity of S for Ca not being used up in CaO,SO_3 the S can act with its full energy on the H of the water. $MgSO_4$, which is a still more soluble salt, shows entirely analogous results, the freedom of the S to act on water being much greater than with $CaSO_4$.

Take now an example of a somewhat different nature ; consider the following :—

$$[Na_2,O] = 99760 \qquad [Na_2,S,O_4,10H_2O] = 347810$$
$$[S,O_3] = 103240$$
$$\text{Difference} = 144810$$
$$347810 = 347810$$

The heat of $[Na_2,S]$ is only 88200 units, but the heat of solution of Na_2O is 55500, and these two make up very nearly the difference of 144810 units. Thus we have the affinity of the S entirely used up, but the affinity of the Na_2 for the oxygen of the H_2O is so great that it can combine as a crystal with ten molecules, in addition to combining with the SO_3.

If space permitted, these facts might be extended and gone into more minutely, and their complete agreement in every particular with my theory of solution pointed out.

I may add further that the amount of salt dissolved in saturated solutions which I have examined is in complete harmony with that theory, as the following example will show :—

Heat of Combination. $[M,Cl_2] - [M,O,Aq]$	Amount of Salt in Saturated Solution. $MCl_2]$
Ca = 20560	63 grains
Sr = 26770	46 ,,
Ba = 35980	35 ,,

It is evident at once that the amount of salt in solution is almost exactly inversely as the difference of heat of $[M,Cl_2]$ and [M,O,Aq]. WM. DURHAM.

Early Perseids.

FROM my observations in preceding years I found the great shower of Perseids commenced on about July 25, and that the last visible traces of it were seen on August 22, after a duration of 29 days.

This year a series of very clear nights occurred on July 16, 18, 19, 20, 21, 22, 23, 27, 28, and 29, and I watched the sky attentively throughout each one, with the idea of tracing, if possible, the earlier stages of this famous shower. On the 16th there were certainly no Perseids visible, but on the 18th, at 11h. 1m., I saw a brilliant streak-leaving meteor in Andromeda, which must have belonged to this stream. On the 19th I recorded 4 Perseids (2 of which were brilliant), and the radiant-point was sharply defined at $19° + 51°$. On the 20th and 21st I noticed several other Perseids, but they were too distant from their radiant, and the paths too few to indicate a good centre. On the 22nd, however, I saw 5 Perseids (one of which was as bright as Jupiter), and the radiant now appeared at $25° + 52°$. On the 23rd I registered 4 Perseids, apparently from the same point of the heavens.

The few ensuing nights were cloudy, but on the 27th the sky became partly clear, and in 3 hours I counted 38 meteors, of which 5 were Perseids from a radiant at $29° + 54°$. On the 28th in $3\frac{3}{4}$ hours I saw 47 meteors, though clouds were very prevalent all night. On this occasion 10 Perseids were seen from a centre at $30° + 55°$, and there were 15 Aquariads from $337° - 12°$. On the 29th the sky was almost uninterruptedly clear, and in $3\frac{3}{4}$ hours I recorded 52 meteors, including 10 Perseids from $31° + 54\frac{1}{2}$. On the 30th, clouds prevailed.

Between July 16 and 29 I observed 287 meteors, of which 43 were Perseids. These observations prove that the display really begins a week earlier than that (July 25) given in my paper in the *Monthly Notices* of the Royal Astronomical Society, vol. xlv., p. 97. The displacement of the apparent radiant-point as there described is well confirmed by my new observations. During the interval from July 18 to August 22 this point advances from $19° + 51°$ to $77° + 57°$.

I subjoin the observed paths of a few bright meteors seen during my recent observations :—

1887.	h. m.	mag.	Path.			Radiant.
			° °		° °	° °
July 19 ...	11 43 ...	1 ...	$358\frac{1}{2} + 38$	to	$351 + 30$ ...	streak ...	19 + 5[illegible]
	11 43 ...	1 ...	$298 + 56$	,,	$14 + 65\frac{1}{4}$ ...	slow ...	271 + 2[illegible]
	12 25 ...	1 ...	$350 + 62$	,,	$330\frac{1}{2} + 64$ ...	streak ...	19 + 51
	12 52 ...	1 ...	$9 + 20$	,,	$17 + 20\frac{1}{4}$ ...	swift ...	333 + 1[illegible]
,, 22 ...	10 59 ...	♃ ...	$16 + 41$	,,	$16\frac{1}{2} + 51$ ...	streak ...	16 + 31
	12 21 ...	♃ ...	$356 + 45$	,,	$332 + 29$ ...	streak ...	25 + 52
	12 25 ...	♃ ...	$344 + 33$	,,	$320 + 29$ ...	streak ...	16 + 31
	13 15 ...	♃ ...	$323 + 37$	,,	$355\frac{1}{2} + 10\frac{1}{2}$ ...	slowish ...	271 + 48
	13 35 ...	1 ...	$111 + 50$ seemed stationary.			
,, 27 ...	10 $40\frac{1}{2}$...	♃ ...	$325 + 6$	,,	$330 + 8$ ...	slow ...	322 + 4
	13 21 ...	♃ ...	$319\frac{1}{2} + 16\frac{1}{2}$	,,	$308 + 32$ ...	slowish ...	337 − 12
,, 29 ...	11 28 ...	♃ ...	$66 + 72\frac{1}{2}$	,,	$114 + 70$ ...	swift, streak	21 + 57

Many others were seen of 1st mag. A perfectly stationary meteor of the 2nd mag., and sparkling like a star, was visible on July 29 at 14h. 17m. at $337° - 12°$, so that it was an Aquariad travelling directly in the line of sight.

On the 22nd I registered some brilliant meteors, of precisely the same visible type as the Perseids, from a radiant at $16° + 31°$ or 3° south of β Andromedæ. Many meteors have also been falling from the points $269° + 49°$, $310° + 9°$, $333° + 12°$, 335° + 49°, and 351° + 38°. All these are swift and short, and generally devoid either of streaks or trains.

Bristol, July 31. W. F. DENNING.

P.S.—In 1885 and some other years I have seen, on about July 13, a very definite shower of bright streak-leaving meteors from the point 11° + 48°, and it is a very probable conjecture that this radiant represents the earliest manifestation of the stream of Perseids.—W. F. D.

Floating Eggs.

REFERRING to the remarks of Mr. E. E. Prince in NATURE, July 28, p. 294, on the above subject, I wish to add that the spawn found by me had "the light violet-gray tint" and crape-like appearance he describes. I am very much on the water in harbours frequented by *Lophius*, but never saw any of this spawn before.

We found it in a swirl of the tide off Bantry Bay, where the sea was over 40 fathoms deep, and in the midst of innumerable jelly-fish, which seem to congregate wherever the current is most swift. W. S. GREEN.

Carrigaline.

THE "METEOROLOGISKE INSTITUT" AT UPSALA, AND CLOUD MEASUREMENTS.

THE Meteorological Institute at Upsala has gained so much fame by the investigations on clouds which have been carried on there during the last few years, that a few notes on a recent visit to that establishment will interest many readers.

The Institute is not a Government establishment; it is entirely maintained by the University of Upsala. The *personnel* consists of Prof. Hildebrandsson, as Director; M. Ekholm and one other male assistant, besides a lady who does the telegraphic and some of the computing work.

The main building contains a commodious office, with a small library, and living apartments for the assistant. The principal instrument-room is a separate pavilion in the garden. Here is located Thiorell's meteograph, which records automatically every quarter of an hour on a slip of paper the height of the barometer, and the readings of the wet and dry thermometers. Another instrument records the direction and velocity of the wind.

This meteograph of Thiorell's is a very remarkable instrument. Every fifteen minntes an apparatus is let loose which causes three wires to descend from rest till they are stopped by reaching the level of the mercury in the different tubes. When contact is made with the surface of the mercuries, an electric current passes and stops the descent of each wire at the proper time. The downward motion of the three wires has actuated three wheels, each of which carries a series of types on its edge, to denote successive readings of its own instrument. For instance, the barometer-wheel carries successive numbers for every five-hundredth of a millimetre—760·00, 760·05, 760·1, &c.; so that when the motion is stopped the uppermost type gives in figures the actual reading of the barometer. Then a subsidiary arrangement first inks the types, then prints them on a slip of paper, and finally winds the dipping wires up to zero again.

An ingenious apparatus prevents the electricity from sparking when contact is made, so that there is no oxidation of the mercury. The mechanism is singularly beautiful, and it is quite fascinating to watch the self-acting starting, stopping, inking, and printing arrangements. We could not but admire the exquisite order in which the whole apparatus was maintained; the sides of the various glass tubes were as clean as when they were new, and the surfaces of the mercuries were as bright as looking-glasses.

The University may well be proud that the instruments were entirely constructed in Stockholm, by the skilful mechanic Sörrenson, though the cost is necessarily high. The meteograph, with the anemograph, costs £600, but the great advantage is that no assistant is required to sit up at night, and that all the figures wanted for climatic constants are ready tabulated without any further labour. But the Institute is most justly celebrated for the researches on the motion and heights of clouds that have been carried on of late years under the guidance of Prof. Hildebrandsson, with the assistance of Messrs. Ekholm and Hagström.

The first studies were on the motion of clouds round cyclones and anticyclones; but the results are now so well known that we need not do more than mention them here. Latterly the far more difficult subjects of cloud heights and cloud velocities have been taken up, and as the methods employed, and the results that have been obtained are both novel and important, we will describe what we saw there.

We should remark, in the first instance, that the motion of the higher atmosphere is far better studied by clouds than by observations on mountain-tops; for on the latter the results are always more or less influenced by the local effect of the mountain in deflecting the wind, and forcing it upwards.

The instrument which they employ to measure the

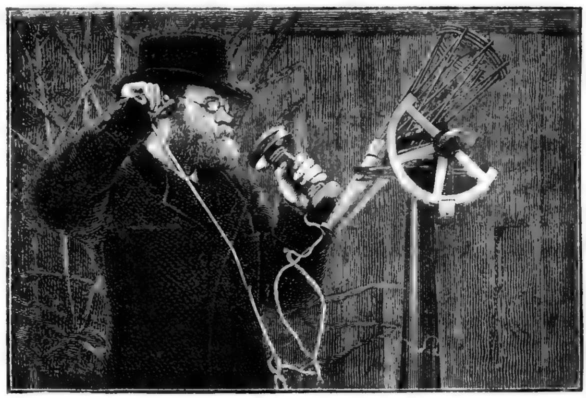


FIG. 1.—N. EKHOLM MEASURING CLOUDS.

This figure shows the peculiar ocular part of the altazimuth, with the vertical and horizontal circles. It also shows the telephonic arrangement.

angles from which to deduce the height of the clouds is a peculiar form of altazimuth, that was originally designed by Prof. Mohn, of Christiania, for measuring the parallax of the aurora borealis. It resembles an astronomical altazimuth, but instead of a telescope it carries an open tube without any lenses. The portion corresponding to the object-glass is formed by thin cross-wires; and that corresponding to the eye-piece, by a plate of brass, pierced in the centre by a small circular hole an eighth of an inch in diameter. The tube of the telescope is replaced by a lattice of brass-work, so as to diminish, as far as possible, the resistance of the wind. The vertical and horizontal circles are divided decimally, and this much facilitates the reduction of the readings.

The general appearance of the instrument is well shown in the figure, which is engraved from a photograph I took of M. Ekholm while actually engaged in talking through a telephone to M. Hagström as to what portion of a cloud should be observed. The lattice-work tube, the cross-wires in place of an object-glass, and the vertical circle are very obvious, while the horizontal circle is so much end on, that it can scarcely be recognized except by the tangent screw which is seen near the lower telephone. Two such instruments are placed at the opposite extremities of a suitable base. The new base at Upsala has a length of 4272 feet; the old one was about half the length. The result of the change has been that the mean error of a single determination of the highest clouds has been reduced from 9 to a little more than 3 per cent. of the actual height. At the same time the difficulty of identifying a particular spot on a low cloud is considerably increased. A wire is laid between the two ends of the base, and each observer is provided with two telephones—one for speaking, the other for listening. When an observation is to be taken, the conversation goes on somewhat as follows:—First observer, who takes the lead: "Do you see a patch of cloud away down west?" "Yes." "Can you make out a well-marked point on the leading edge?" "Yes." "Well then; now." At this signal both observers put down their telephones, which have hitherto engaged both their hands, begin to count fifteen seconds, and adjust their instruments to the point of cloud agreed on. At the fifteenth second they stop, read the various arcs, and the operation is complete.

But when the angles have been measured the height has to be calculated, and also the horizontal and vertical velocities of the cloud by combining the position and height at two successive measurements at a short interval. There are already well-known trigonometrical formulæ for calculating all these elements, if all the observations are good; but at Upsala they do far more. Not only are the observations first controlled by forming an equation to express the condition that the two lines of sight from either end of the base should meet in a point, if the angles have been correctly measured, and all bad sets rejected; but the mean errors of the rectangular co-ordinates are calculated by the method of least squares.

The whole of the calculations are combined into a series of formulæ which are necessarily complicated; and even by using logarithms of addition and subtraction, and one or two subsidiary tables—such as for log. $\sin^2\frac{\theta}{2}$, specially constructed for this work—the computation of each set of observations takes about twenty minutes.

Before we describe the principal results that have been attained, it may be well to compare this with the other methods which have been used to determine the height of clouds. A great deal of time, and skill, and money, have been spent at Kew in trying to perfect the photographic method of measuring the height of clouds. Very elaborate cloud cameras, or photo-nephoscopes, have been constructed, by means of which photographs of a cloud were taken simultaneously from both ends of a suitable base. The altitude and azimuth of the centre of the plate were read off by the graduated circles which were attached to the cameras; and the angular measurements of any point of cloud on the picture were calculated by proper measurements from the known centre of the photographic plate. When all this is done, the result ought to be the same as if the altitude and azimuth of the point of the cloud had been taken directly by an ordinary angle-measuring instrument.

It might have been thought that there would be less chance of mistaking the point of the cloud to be measured, if you had the pictures from the two ends of the base to look at leisurely, than if you could only converse through a telephone with the observer at the other end of the base. But in practice it is not so. No one who has not seen such cloud-photographs can realize the difficulty of identifying any point of a low cloud when seen from two stations half a mile or a whole mile apart; and for other reasons, which we will give presently, the form of a cloud is not so well defined in a photograph as it is to the naked eye.

At Kew an extremely ingenious sort of projector has been devised, which gives graphically the required height of a cloud from two simultaneous photographs at opposite ends of the same base, but it is evident that this method is capable of none of the refinements which have been applied to the Upsala measures, and that the rate of vertical ascent or descent of a cloud could hardly be determined by this method. But there is a far greater defect in the photographic method which at present no skill can surmount.

We saw that the altazimuth employed at Upsala had no lenses. The meaning of this will be obvious to anyone who looks through an opera-glass at a faint cloud. He will probably see nothing for want of contrast, and if anything of the nature of a telescope is employed, only well-defined cloud outlines can be seen at all. The same loss of light and contrast occurs with a photographic lens; and many clouds that can be seen in the sky are invisible on the ground glass of the camera. Cirrus and cirro-stratus—the very clouds we want most to observe—are always thin and indefined as regards their form and contrast against the rest of the sky; so that this defect of the method is the more unfortunate.

But even when the image of a cloud is visible on the focusing glass, it does not follow that any image will be seen in the picture. In practice, thin high white clouds against a blue sky can rarely be taken at all, or only under exceptional circumstances of illumination. The reason seems to be that there is very little light reflected at all from a thin wisp of cirrus, and what there is must pass through an atmosphere always more or less charged with floating particles of ice or water, besides earthy dust of all kinds. The light which is scattered and diffused by all these small particles is also concentrated on the sensitive plate by the lens, and the resulting negative shows a uniform dark surface for the sky without any trace of the cloud. What image there might have been is buried in photographic fog.

In order to compare the two methods of measuring clouds, I went out one day last December at Upsala with Messrs. Ekholm and Hagström when they were measuring the height of some clouds. It was a dull afternoon, a low foggy stratus was driving rapidly across the sky at a low level, and through the general misty gloom of a northern winter day we could just make out some striated stripes of strato-cirrus—low cirro-stratus—between the openings in the lower cloud-layer. The camera and lens that I use habitually for photographing cloud-forms—not their angular height—was planted a few feet from the altazimuth which M. Ekholm was observing, and while he was measuring the necessary angles I took a picture of the clouds. As might have been expected under the circumstances, the low dark cloud came out quite well, but there was not the faintest trace of the strato-cirrus on the negative. MM. Ekholm and Hagström, however, succeeded in measuring both layers of cloud, and found that the low stratus was floating at an altitude of about 2000 feet high, while the upper strato-cirrus was driving from S. 57° W. at an altitude of 19,653 feet, with a horizontal velocity of 81, and a downward velocity of 7·2 feet per second. This is a remarkable result, and shows conclusively the superiority of the altazimuth to the photographic method of measuring the heights of clouds.

Whenever opportunity occurs, measures of clouds are taken three times a day at Upsala, and it may be well to glance at the principal results that have been obtained. The greatest height of any cloud which has yet been satisfactorily measured is only 43,800 feet, which is rather less than has usually been supposed; but the highest velocity, 112 miles an hour with a cloud at 28,000 feet, is greater than would have been expected. It may be interesting to note that the isobars when this high velocity was reported were nearly straight, and sloping towards the north-west.

The most important result which has been obtained from all the numerous measures that have been made is the fact that clouds are not distributed promiscuously at all heights in the air, but that they have, on the contrary, a most decided tendency to form at three definite levels. The mean summer level of these three stories of clouds at Upsala has been found to be as follows: low clouds—stratus, cumulus, cumulo-nimbus, 2000-6000 feet; middle clouds—strato-cirrus and cumulo-cirrus, 12,000-15,000 feet; high clouds—cirrus, cirro-stratus, and cirro-cumulus, 20,000-27,000 feet.

It would be premature at present to speculate on the physical significance of this fact, but we find the same definite layers of clouds in the tropics as in these high latitudes, and no future cloud-nomenclature or cloud-observations will be satisfactory which do not take the idea of these levels into account.

But the refinements of the methods employed allow the diurnal variations both of velocity and altitude to be successfully measured. The velocity observations confirm the results that have been obtained from mountain stations—that, though the general travel of the middle and higher clouds is much greater than that of the surface winds, the diurnal variation of speed at those levels is the reverse of what occurs near the ground. The greatest velocity on the earth's surface is usually about 2 p.m.; whereas the lowest rate of the upper currents is about midday.

The diurnal variation of height is remarkable, for they find at Upsala that the mean height of all varieties of clouds rises in the course of the day, and is higher between 6 and 8 in the evening than either in the early morning or at midday.

Such are the principal results that have been obtained at Upsala, and no doubt they surpass any previous work that has been done on the subject. But whenever we see good results it is worth while to pause a moment to consider the conditions under which the work has been developed, and the nature and nurture of the men by whom the research has been conducted. Scientific research is a delicate plant, that is easily nipped in the bud; but which, under certain surroundings and in a suitable moral atmosphere develops a vigorous growth.

The Meteorological Institute of Upsala is an offshoot of the Astronomical Observatory of the University; and a University, if properly directed, can develop research which promises no immediate reward in a manner that no other body can approach.

If you want any quantity of a particular kind of calculation, or to carry on the routine of any existing work in an Observatory, it is easy to go into the labour market and engage a sufficient number of accurate computers either by time or piece-work, or to find an assistant who will make observations with the regularity of clockwork. But original research requires not only special natural aptitudes and enthusiasm to begin with, but even then will not flourish unless developed by encouragement, and the identification of the worker with his work. It is rarely, except in Universities, that men can be found for the highest original research. For there only are young students encouraged to come forward and interest themselves in any work for which they seem to have special aptitude.

Now, this is the history of the Upsala work. Prof. Hildebrandsson was attached as a young man to the meteorological department of the Astronomical Observatory, and when the study of stars and weather were separated, he obtained the second post in the new Meteorological Institute. From this his great abilities soon raised him to the directorship, which he now holds with so much credit to the University. M. Ekholm, a much younger man, has been brought up in the same manner. First as a student he showed such aptitudes for the work as to be engaged as assistant; and now, as the actual observation and reduction of the cloud-work is done by him and M. Hagström, the results are published under their names, so that they are thoroughly identified with the work.

Upsala is the centre of the intellectual life of Sweden, and there, rather than at Stockholm, could men be found to carry out original research. It redounds to the credit of the University that it has so steadily supported Prof. Hildebrandsson, and that he in his turn has utilized the social and educational system by which he is surrounded to bring up assistants who can co-operate with him in a great work that brings credit both to himself, to themselves, and to the Institute which they all represent.

RALPH ABERCROMBY.

A NEW COSMOGONY.[1]

I.

THE volume before us is composed of a series of essays, first published in the Catholic periodical, *Natur und Offenbarung*, in 1885-86. By far the greater part of it is, nevertheless, of a purely scientific character. The opening chapter alone enters upon theological considerations, which we cannot here pretend to discuss, recommending merely, in passing, the broad and wise doctrines it contains to the notice of those well-meaning persons who apprehend danger to creeds from speculations as to origins.

That of our planetary system is very actively in debate just now. The nebular hypothesis, as fashioned by Laplace, no longer fits in with all the known facts. There are so many of them that it would be surprising if it did, since the outside of its claim was to the plausible representation of possible truth. It had a part to play in the history of science, which it played with eminent success. This was to show that thought, safeguarded by right reason, might be trusted to run backward towards the beginnings of things—that, without visible discontinuity or breach of known laws, the present fair scheme of creation might have emerged from the brooding darkness of chaos, along paths not wholly inaccessible to human discursive faculties.

But now the reiterated blows of objectors may fairly be said to have shattered the symmetrical mould in which Laplace cast his ideas. What remains of it is summed up in the statement that the solar system did originate, somehow, by the condensation of a primitive nebula. The rest is irrevocably gone, and the field lies open for ingenious theorizing. It has not been wanting.

The newer cosmogonists are divided into two schools by the more or less radical tendencies of the reforms they propose. Some seek wholly to abolish, others merely to renovate, the Kant-Laplace scheme. The first class is best represented by M. Faye, the second by M. Wolf and Dr. Braun. Dr. Braun is, however, a more thorough-going renovator than M. Wolf. The edifice, as "restored" by him, shows, indeed, little trace of its original aspect. Scarcely the invisible foundations are the same; the superstructure is unrecognizable. We will endeavour to sketch its main features.

In widening the nebular hypothesis to embrace the whole sidereal world, our author demands as little as possible in the way of postulates: simply a co-extensive nebula, structureless, motionless, tenuous, its particles endowed with gravity and atomic repulsion. Such a nebula, if perfectly homogeneous, should give birth to one portentous, solitary sun. But, in point of fact, it would possess innumerable, almost imperceptible, local irregularities, which, forming so many centres of attraction, would eventually lead to the breaking-up of the nebula into a vast multitude of separate fragments.

On one of these, the destined progenitor of the solar

[1] "Ueber Cosmogonie vom Standpunkt christlicher Wissenschaft. Mit einer Theorie der Sonne." Von Carl Braun, S.J. (Münster: Aschendorff, 1887.)

system, we are asked to concentrate our attention. The manner of its development is, however, a widely different one from that traced by the great French geometer. Laplace *assumed* the needful rotation, and left the rest to work itself out spontaneously. He permitted no external interference with the tranquil processes which he discerned as progressing through the ages. Dr. Braun, on the other hand, assumes less to begin with, but invokes adventitious aid in emergencies. No single nebula thrown on its own resources sufficed, he finds, for the production of the solar system. The complicated phenomena which it presents demand a complex origin to explain them. The mass of cosmical matter in which they first began to unfold themselves was accordingly but the nucleus of what it afterwards became. It not only grew by the accretion of similar masses falling towards it from space, but acquired its gyratory movement by eccentric collisions with them. The consequences of such events elsewhere are visibly pictured to us in the spiral lines of light of certain nebulæ. The great whirlpool in the Canes Venatici, for instance, betrays and records the fall of a comet, on the gigantic primitive scale, into an embryo sun. Only thus, in our author's opinion, can the strange peculiarities of its structure be accounted for; and only thus can the first impulse to axial rotation in our own system have been given.

The visits of comets, as we now see them, feebly represent, we are told, the colossal in-rushes from interstellar regions by which the machinery of planetary production was set going and modified. But it is difficult to allow to such bodies the independent origin implied in the claim for them of such illustrative significance. Comets can no longer be set down as mere casual intruders upon the solar system. They certainly share its translatory motion, since, if they were either overtaken or encountered, they should seem to come most numerously from near the apex of the sun's path. But they approach him indifferently from all parts of the sky. A further proof of the absence of relative motion is afforded by the shape of the tracks they pursue. M. Faye has remarked that, of 364 cometary orbits calculated, not one is a decided hyperbola ("Origine du Monde," p. 146); and Laplace's view that they are hyperbolic by nature, and elliptical only through perturbations, is thus seen to be the exact reverse of the truth.

A fundamental objection to Laplace's cosmogony is that it implies a far swifter axial movement in the central bodies of our system than they actually possess. For in the theory of annular separation, the rotation of the generating mass is strictly correlated with the revolution of its offspring by the principle of the conservation of areas, which requires that a rotating homogeneous globe should spin quicker, as it contracts, in the proportion of the square of its radius. Thus, if the solar nebula, when it filled the orbit of Neptune, rotated (as on the hypothesis in question it must have done) in Neptune's period of 165 years, the period of the sun shrunken to its present dimensions, should have shortened in the ratio of the square of 2,780,000,000 (the mean distance of Neptune) to the square of 434,000 miles (the solar radius). In other words, the rotation of the sun should be accomplished in 127 seconds, in lieu of 25 days. Similarly, the terrestrial rotation-period corresponding to the lunar revolution in $27\frac{1}{3}$ days, is no more than $10\frac{4}{5}$ minutes! It is true that in both these estimates (the latter taken from Dr. Braun's pages), the effects of central condensation are neglected, although it must inevitably have made some progress before annulation began; but no allowance on this score, however liberal, can possibly reconcile, though it contributes to lessen, the discrepancy.

Dr. Braun adjusts the balance in this way. The solar nebula had never at any time, in his view, a uniform axial movement. He even ventures to consider the present unequal rotation of the sun as a survival of the primitive state of things to which the central deficiency of rotational momentum is due. For the entire mass was, in the beginning, set gyrating by external impacts. Movement was hence generated predominantly in its outer regions, and was only by degrees and imperfectly communicated to the nuclear parts.

The device is marked by considerable ingenuity, and is at any rate preferable to the eddying movements by which M. Faye evades the same embarrassment. It is, however, scarcely needed by Dr. Braun, since the "ring-theory" of planetary formation is almost, and logically ought to be completely, abandoned by him. Difficulties have of late been thickening round it; they reached a climax when the conviction was attained that, apart from the neutralizing effects of tidal friction, it could only result in the retrograde motion of all secondary systems. The plan of centres of condensation is accordingly substituted by our author. This has the advantage of allowing planets to begin to form anywhere in the nebula. It emancipates them from that strict conformity to the equatorial level which was an inconvenient feature of Laplace's hypothesis; and though they necessarily tended, in the course of their growth, to descend towards it, enough may perhaps remain of their primitive divergence to explain the observed slight deviations from the fundamental plane. Yet Dr. Braun's confidence in this *rationale* of the inclinations of the planetary orbits is so far from being unlimited that he holds in reserve, in case of its failure, other means for bringing about the same end.

Each planet is roughly estimated to have started on its career at about five times its present distance from the sun. In condensing, it descended towards it, sweeping up materials as it went, until finally almost the whole of the diffused gaseous stuff was concentrated in sun and planets, and the intervening spaces were void. By that time, too, tangential velocity had come to balance gravity, and the slow inward approach ceased. But the resistance met with in the earlier stages of its history by the growing and falling planet had had one result of vital importance to its future. It had imparted to it a movement of rotation. As it settled down in close spirals towards its present orbit, its velocity must everywhere have exceeded, by a small amount, the velocity in the same direction of the medium in which it moved. The density of that medium must, however, have increased towards the sun; and the embryo-planet consequently experienced a slight excess of resistance on its inner side, resulting in a direct whirling movement. Dr. Braun endeavours to show that the rotation thus set on foot must have belonged chiefly to the external layers of the planetary nebula. His motive is that of conciliating the swift circulation of satellites yet to be born from it with the comparatively sluggish spinning of the parent mass.

Tidal friction he rejects as an agent of planetary development, attributing to it barely the power to have rendered absolute an already approximate coincidence between the periods of rotation and revolution of satellites. Perhaps he might here be induced to reconsider his position. At least in the case of the lunar-terrestrial system, the evidence is overwhelming that tidal friction was largely concerned in bringing about its present condition. We may further assure him that Prof. G. H. Darwin (whom he evidently identifies with the late Charles Darwin, his father) has not committed the blunder he imputes to him of ascribing to the moon a *shorter* period of revolution than that of the earth's rotation, at the time when it began, under the reactive influence of the tidal wave, to travel slowly outward from near its surface. On the contrary, a slightly inferior angular velocity in the satellite is the assumed starting-point of all his subsequent reasoning on the subject.

For the completion of the solar system in its minor details, Dr. Braun is driven to the expedient of collisions with some of the many nebulous fragments which continued to be drawn towards it from unfathomed depths of space. Most of these became incorporated with the sun, but a certain proportion must have been intercepted by the planets, which, in their forming state, as possessing less mass and velocity, were more sensitive to such shocks than when fully formed. Thus, the plane of the ecliptic might have been altered, we are told, 1° by the impact upon the inchoate earth of a body possessing 1/1000 its present mass. Facilities even greater were offered for changing the elements of rotation; that of the earth, when of seventy times its actual radius, might even have been stopped altogether, by collision, under specially favourable circumstances, with a mass only 1/10600 the terrestrial.

But this method of explanation is radically unsatisfactory. It suggests the *Deus ex machinâ* of an unskilled dramatist, and cannot be admitted without protest into scientific speculation. We have learned to regard cometary impacts as the last resource of the distressed cosmogonist. Such events are not impossible, but to resort to them in difficulties is to throw up the game of ordered inference. The conviction remains unalterable that the results visible to us were brought about by means less apparently fortuitous. Dr. Braun, for example, is obliged to suppose not only that, before the separation of the moon, the axis of the lunar-terrestrial nebula was deviated, by extraneous agencies of the kind indicated, to the extent of 5° from its original position of perpendicularity to the plane of the ecliptic; but that, subsequently to the separation, further shocks continued the process upon the earth alone until the inclination attained its present value of 23½°. Still less admissibly, the solar axis is assumed, after the formation of Venus, to have been tilted 5° by a number of successive impacts. A transcendent degree of improbability seems to be reached by this conjecture.

In the order of planetary production, Dr. Braun follows Laplace. Neptune is his oldest planet. And the fact that it revolves very nearly in the "invariable plane" of the solar system is confirmatory of the view that it really was the first body (instead of being the last, as M. Faye supposes) to become severed from the primitive nebula, the rotation of which is likely to have been conducted in that plane (Wolf, *Bull. Astronomique*, t. ii. p. 228). Neptune alone, owing to the distinction of his retrograde rotation, is allowed by our author to have been formed by the detachment and eventual condensation of a nebulous ring. But Prof. Kirkwood has raised an objection to this orthodox mode of genesis which applies with especial force to the remotest planet. The coalescence into a single globe of the fragments of a broken-up ring, if it happened at all (which is uncertain) would, it appears, have been an unconscionably slow process. Thus, two opposite portions of a ring of the dimensions of Neptune's orbit, could scarcely have come together in less than 150,000,000 years. It must be admitted that this is a startling demand on the time-exchequer even of the cosmos.

Uranus is regarded by Dr. Braun as what Bacon called a "limiting instance" between the annular and the nuclear methods of generation. An abortive ring gave place to a centre of condensation, the result (helped, perhaps, by some well-aimed cometary shoves) being the indecisive character of the Uranian rotation on an axis lying prone in the plane of revolution.

These, then, are the main outlines of the last and newest cosmogony. While dissenting from some of its conclusions, we readily admit that it is, in several ways, a noteworthy effort. Its appearance may be said to mark the definitive abandonment, by sound thinkers, of the annular method of planet and satellite formation. The preciseness of the conditions of that celebrated hypothesis lent it its charm, but has proved its ruin. Had they been less definite, it might have lived longer. But it gave, as it were, hostages to the future which it has not been able to redeem.

It is gradually becoming clear that, while the various members of the solar family owned unquestionably a common origin, they can scarcely be said to have had a common history. Each ran through a cycle of development particular to itself, and appointed, without doubt, to adapt it to a special purpose. The biography of the earth and moon, as narrated by Prof. Darwin, is an example. Here influences predominated which, in every other secondary system, were comparatively unfelt.

This growing persuasion of what we may call planetary individuality is reflected in Dr. Braun's vigorous and original chapters. He has honestly, and with no small ability, worked out *ab initio* the problems that they deal with, and he finds them insoluble by the uniformitarian method of treatment. The expedients by which he seeks to obtain a diversity of results by introducing a diversity of vicissitudes, strike us perhaps as arbitrary and awkward; but the admission of their necessity by an inquirer of such acuteness, and so well abreast of contemporary scientific thought, is highly instructive. We shall return later to the part of his interesting work devoted to solar theory.

A. M. CLERKE.

NOTES.

ON Tuesday Lord Hartington introduced to Sir W. Hart Dyke a deputation consisting of Sir Henry Roscoe, Sir Lyon Playfair, Mr. Rathbone, Mr. G. Howell, Mr. Cyril Flower, Sir B. Samuelson, and other gentlemen interested in education. They urged the desirability of the Technical Education Bill being passed, and of certain changes being made in the measure. Sir W. Hart Dyke replied favourably on both points. He was of opinion that the prospects of the Bill were good, both in the House of Commons and in the House of Lords.

THE list of foreign men of science who have accepted the invitation of the Local Committee to attend the Manchester meeting of the British Association is steadily increasing, and now numbers considerably over a hundred. Amongst those whose names have been received since the list we published on July 7, we note Prof. Cremona, of Rome; Dr. Neumayer, Director of the Hamburg Marine Observatory; H. A. Rowland, Baltimore; Dr. Werner Siemens, of Berlin; Prof. Horstman, Heidelberg; Prof. L. Weber, Breslau; Prof. Capellini, Bologna; Prof. Carvill Lewis, Philadelphia; Prof. O. Bütschli, Heidelberg; Prof. Carnoy, Louvain; Prof. Erb, Heidelberg; Dr. Treub, Director of the Botanic Gardens, Java; Capt. Coquilhat, Brussels; Dr. Godefroi, 's Hertogenbosch; Dr. Ludwig Wolf, Leipzig; Signor Bonghi, the late Italian Minister of Education; Signor Luzzati, Rome; Dr. E. Atkinson, Director of the Massachusetts Bureau of Statistics; and Dr. G. Boissevain, Amsterdam. The King of the Belgians has intimated his intention of nominating two representatives of the Congo Free State to attend the meeting, and of these General Strauch, Administrator-General of the Congo Free State, is expected to be one. A correspondent in Paris writes to us that the Emperor of Brazil, who has lately spent some time in the French capital, will probably attend the meeting of the Association.

MR. A. T. ATCHISON, Secretary of the British Association, is authorized to state that at the Manchester meeting space will be provided in the galleries of the Reception Room for the exhibition of specimens and instruments shown in connexion with and in illustration of papers read in the Sections. To secure admission a brief description of the specimens or instruments must be submitted to the Local Secretaries not later than August 10, together with a statement of the dimensions of the table or other fixture required. No motor power will be available. The objects must be exhibited at the risk of the owners;

and the Local Committee, while it will endeavour to meet all reasonable wishes, reserves to itself the right to exclude all exhibits that may appear to it to be for any reason unsuitable. No objects shown for advertising purposes will be admissible.

THE summer meetings of the Institution of Mechanical Engineers were held this week on Tuesday and Wednesday in the University of Edinburgh, under the presidency of Mr. E. H. Carbutt. After the meeting on Tuesday, the members inspected the Forth Bridge and Works.

ON Saturday, July 30, the statue of Paul Broca, close to the Medical School, Paris, was unveiled. Addresses were delivered by different persons connected with the Anthropological School.

A CHAIR of Sanitary Engineering has been established in the Imperial University of Japan. It is believed to be the only Chair of the kind at present in existence. The Professor appointed to fill it is Mr. W. K. Burton, lately senior Sanitary Engineer to the London Sanitary Protection Association.

THE geographical habitat of *Peripatus leuckarti*, Sänger, is said (*Archiv für Naturg.* 1871, p. 407) by Prof. Leuckart to be "New Holland." Owing to the vagueness of our knowledge of the subject, it may be of interest to state that two specimens of what appears to be this species have been found in the Queensland scrubs, near Wide Bay. These specimens have been presented to the British Museum by Mr. E. P. Ramsay, of the Australian Museum, Sydney.

THE announcement of the discovery of more than a dozen new elements appears at first sight rather sensational, and were it not that the names of Krüss and Nilson are sufficient guarantees of its authenticity the intimation would probably be received with more than a little caution. Such, however, is the startling result of the long, laborious, and exceptionally difficult researches of the Swedish chemists upon the nature of the rare earths contained in several sparsely-distributed minerals, and a detailed account of their labours will be found in the number of the *Berichte* just issued. A precise measurement of the wavelengths of the lines and bands in the absorption-spectra of the nitrates of the rare earths contained in thorite of Brevig and Arendal, wöhlerite of Brevig, cerite of Bastnäs, fergusonite of Arendal and Ytterby, and in euxenite of Arendal and Hitterö, has resulted in the surprising observation that only a particular few of the lines supposed by former observers to belong to the nitrate of any one metal are present in the absorption-spectra of the nitrates derived from certain minerals, the other lines being absent in these, but present in the nitrate spectra of other minerals, while some that are present in the former are absent or very faint in the latter. For example, only one of the lines supposed to belong to the nitrate of holmium is present in any intensity in the spectrum of the nitrates from thorite of Brevig, while in the spectrum of nitrates derived from other minerals it is but an insignificant line among several holmium lines much more intense. The conclusion from most exhaustive spectral measurements is inevitable, that most of the so-called elements in these minerals are compound substances, the various ingredients of which are present in certain minerals and absent in others; further, the fractionation of the nitrates has led to the partial separation of a large number of the components themselves. Holmium, the metal which Soret called X, and which Lecoq de Boisbaudran separated into two components, is now shown to consist of seven distinct elements—$X\alpha$, $X\beta$, $X\gamma$, $X\delta$, $X\epsilon$, $X\zeta$, and $X\eta$; erbium of two—$Er\alpha$ and $Er\beta$, which it is possible to separate by fractionation; thulium, named by Cleve in 1879, also of two—$Tm\alpha$ and $Tm\beta$; didymium which was shown by von Welsbach to consist of two components, praseodym and neodym, must, in the light of these spectral differences, consist of not less than nine distinct elements, while samarium, the name given by Lecoq de Boisbaudran to Marignac's $Y\beta$, is composed of at least two components—$Sm\alpha$ and $Sm\beta$. Hence, in place of holmium, erbium, thulium, didymium, and samarium, we are constrained to accept the existence of more than twenty elements, in the work of completing the isolation of which Drs. Krüss and Nilson urgently invite assistance.

THE United States *Monthly Weather Review* for March and April contains interesting notes, *e.g.* (1) Average storm-tracks over the United States during March, compiled from observations for the years 1873–85. The paths pursued by these storm-centres are divided into four distinct classes, and are traced until the disappearance of the storms at various points on the Atlantic coast. (2) Rain frequency and wind rose for April, with charts constructed from all observations available from the commencement of the records until the end of 1886. The charts have been prepared for use in the current weather predictions of the service. The Reviews also contain descriptions of the storms which occurred over the North Atlantic during each month, and their approximate paths are shown on charts, together with the distribution of icebergs reported.

THE *Jahres-Bericht* of the Central Meteorological Office of the Grand Duchy of Baden, for 1886, contains the results of meteorological observations at sixteen stations of the second and third orders, and twenty-nine rain stations, of which the positions and heights above sea are given. The Central Office has taken part in two Conferences during the year—one at Munich, relative to the investigation of the frequency, direction of propagation, and intensity of hailstorms; and the other at Friedrichshafen, with respect to a physical survey of the Lake of Constance. The proposals made at both Conferences are awaiting the decisions of the various Governments concerned. The Report is accompanied by a chart giving the distribution of the rainfall during the year 1886, and shows three districts with maxima exceeding 55 inches, in positions corresponding with those on the chart of the previous year. The greatest amount was 79·56 inches at Todtnauberg, and the least 30·28 inches at Diedesheim. It also contains hydrological observations at various stations on the Rhine and its larger tributaries.

DR. K. WEIHRAUCH, Director of the Observatory at Dorpat, Russia, has published the mean values of the meteorological observations at that place for the twenty years 1866–85, giving (1) the results of the individual months and years, and (2) the results for the twenty years combined. This Observatory was established in December 1885 by the late Dr. A. von Oettingen, and is one of the few stations that have persistently published wind *components*, under each of the points N. E. S. W. (in addition to the usual components N. – S., and E. – W.), whereby a better knowledge of the general distribution of the wind is obtained than when only two components are given. The highest mean monthly shade temperature, for the twenty years, is 63°·2 in July, the lowest 19°·6 in January, and the mean for the year 39°·9. The greatest mean monthly rainfall is 3·64 inches in July, the lowest 1·02 inches in March, and the total for the year 16·21 inches. The fourth volume of the actual observations, for the years 1881–85, is now being printed, the publication having been delayed hitherto for want of funds.

THE work done at the Melbourne Observatory in connexion with meteorology and terrestrial magnetism expands a little every year. The importance attaching to rainfall and water-supply renders it necessary to spread rain-gauges wherever trustworthy observers can be secured, and we learn from the latest Report of the Observatory that 272 monthly returns are now received, most of the observers being volunteers. A complete register of Victorian rainfall has been prepared, showing at a glance the annual and monthly rainfall, as well as the averages for a series of years and months. The issue of weather-maps and forecasts for Southern and Northern Victoria has been regularly continued, and this service appears to be fully appreciated.

Mr. R. H. Scott, of the Meteorological Office, sends us some notices of earthquakes observed at North Unst and Sumburgh Lighthouses. These notices he has come across while examining Journals of Shetland Lighthouses, and it may be worth while to put them on record. The following are records from logs at North Unst Lighthouse: 1879, January 4, 5 minutes past 1 p.m. mean time, felt smart shock of earthquake, lasted about 4 seconds; 1880, July 18, 20 minutes after midnight last, a smart shock of earthquake lasted from 34 to 35 seconds, then a second shock not so strong or of so long duration,—whole rock and building oscillated; 1885, September 26, at 10 p.m., we felt the tower shake very suddenly,—men in bed as well as the man on the watch cannot account for it, unless a slight shock of an earthquake,—no heavy sea, and the wind light from north. At Sumburgh Head Lighthouse the following observation was made: 1876, November 28, at 6 p.m., a slight shock of earthquake was felt at this station. For two or three seconds the lamp-glass in the tower shook violently. As my attention was taken up with the lamp, and fearing that the glass would fall, I therefore did not observe any other movement. The wind at the time was north by east, and light, accompanied with showers, and dark clouds hanging about.

Some time ago (vol. xxxiii. p. 491) we gave an account of some excellent papers on "Technical Education, Applied Science, Buildings, Fittings, and Sanitation," by Mr. Edward Cookworthy Robins. These papers, revised and admirably illustrated, have now been brought together in a handsome volume entitled "Technical School and College Building," and published by Messrs. Whittaker and Co. The book is dedicated to Prof. Huxley. It will be very welcome to all who are engaged, or who look forward to being engaged, in the construction of technical school buildings.

A book on "Canada and Newfoundland," by Ernst von Hesse-Wartegg, is about to be published in Freiburg-im-Breisgau. The author has repeatedly visited the country he describes, and his work is the more likely to be appreciated in Germany, because the northern part of the American continent has hitherto been almost wholly neglected by German writers of books of travel.

The first number of the American *Journal of Psychology* will appear early in October. The object of the *Journal* is to record psychological work of a scientific as distinct from a speculative character.

Prof. Geo. H. Palmer, of Harvard College, has published the results of some inquiries lately made by him as to the annual expenditure of Harvard undergraduates. Of the members of the graduating class, about one-fourth had spent from 400 to 650 dollars; another fourth, from 650 to 975 dollars; another, from 975 to 1200 dollars; and another, upwards of 1200 dollars. The lowest sum reported was 400 dollars; the highest, 4000. Addressing parents, Prof. Palmer says:—"If your son is something like an artist in economy, he may live at Harvard under 600 dollars a year. If he is able to live closely, carefully, and yet with due regard to all that he requires, he may easily accomplish it on between 600 and 800 dollars. If you wish him to live here at ease, from 800 to 1000 dollars may be well expended. I should be very confident that every dollar given him over 1200 dollars was a dollar of danger."

It seems, from a paper by M. Jammes, who lives in Camboja, that animals, as well as human beings, are liable to become addicted to opium-poisoning. In countries where the use of opium is prevalent, many animals remain beside their master while he takes a whiff at his pipe. Breathing an air containing a good deal of opium vapour, they become quite intoxicated, and appear to relish the sensation. This has been noticed concerning cats, dogs, and monkeys. The latter, according to M. Jammes, like the sensation so much that some of them take to eating opium.

The Foreign Department of the Chinese Government (the Tsung-li-Yamên) has just addressed a very striking memorial to the Emperor proposing the introduction of mathematics and physics into the metropolitan and provincial competitive examinations for public employment. It is suggested that this should be done in all the provinces of the Empire, the successful candidates being sent to Pekin to compete for higher grades. They are to be examined in the capital, in addition to the preceding subjects, in civil and military engineering, international law and history. Those who are successful in obtaining the highest degree will receive an honorary official status, equivalent to a Fellowship, in the Foreign Language College at Pekin, together with official appointments in the capital or the provinces. This scheme has now received the Imperial sanction, and it is difficult for those unacquainted with China to appreciate the vast change which it will produce in time. Hitherto the competitive examinations which must be passed by every intending official, have been confined to the ancient Chinese classics, exercises in prose and poetical composition, and Chinese history, and they have been of the same kind for centuries. They were the ark of the covenant, which it was sacrilege to touch; but the Chinese have now introduced mathematical science into the curriculum. It will be interesting to see how the new and old subjects will fare respectively, now that they are brought together for the first time in the long history of China.

The first volume of a new periodical specially devoted to botany has been issued in St. Petersburg. The periodical is published in connexion with the botanical garden of the St. Petersburg University by Profs. Beketoff and Gobi, under the title of "Scripta Botanica Horti Universitatis Petropolitanæ." The first volume contains an important work by Prof. Beketoff, on the flora of Yekaterinoslav, which not only gives a list of 1032 species of flowering plants (instead of the 536 species formerly found in the province), but contains a most interesting inquiry into the flora of the steppes of South Russia, as compared with those of the Hungarian *pusztas* on the one side, and the Caspian steppes on the other. The same volume contains a note by Prof. Gobi on a new form of fungi, *Cæoma Cassandræ*, which is found in the peat-marshes of Finland as a parasite on the *Andromeda* (*Cassandra*) *calyculata*; and a paper on the vegetation of the Altai, by A. Krasnoff, containing the enumeration of plants found by the author. These plants were found on the *Artemisia* steppes, on the salt steppes, on black-earth, on meadows inundated during the spring, in forests, and on the higher Alps. While comparing the present flora of the Altai with the Pliocene flora of the same area, characterized by the predominance of the beech and other trees of the temperate region, the author points out that only feeble vestiges of the old flora survive in the present flora of the meadows inundated during the spring. The vegetation of the other sub-regions has completely changed since the Tertiary period, and continues still to change. Thus larch forests disappear with astonishing rapidity, and are succeeded by herbaceous steppes, while *Artemisia* steppes have taken the place formerly occupied by the lakes and brackish marshes which covered the bottoms of the valleys. A feature of the "Scripta Botanica," most welcome to European botanists, is that the papers in Russian are followed by short abstracts in French or German. In the bibliographical section there are analyses of botanical works published in Russia since January 1, 1886. The works analyzed under this head are: Prof. Schmalhausen's "Flora of South-West Russia" (Kieff, Volhynia, Podolia, Poltava, Tchernigoff, and neighbouring regions), published at Kieff; Prof. W. Zinger's work on the "Flora of Central Russia;" Prof. Maximowicz's "Diagnoses Plantarum Novarum Asiaticarum," fascicule vi.; M. Kamenski's "Comparative Researches into the Development and Structure of Urticularia;" and several botanical papers published in Russian scientific periodicals.

THE additions to the Zoological Society's Gardens during the past week include two White-eared Bulbuls (*Pycnonotus leucotis*) from North-West India, presented by General W. H. Breton; a Magpie (*Pica rustica*), British, presented by Mr. H. Stacy Marks, R.A., F.Z.S.; two Turtle Doves (*Turtur communis*), British, presented by Mr. N. Brooks; a Daubenton's Curassow (*Crax daubentoni*) from Venezuela, presented by Capt. Rigaud, s.s. *Larne;* a Loggerhead Turtle (*Thalassochelys caouana*) from the Atlantic Ocean, presented by Mr. R. T. Ward; two Green Lizards (*Lacerta viridis*); two Marbled Newts (*Molge marmorata*), European, presented by the Rev. F. W. Haines; a Crested Pigeon (*Ocyphaps lophotes*) from Australia; a Secretary Vulture (*Serpentarius reptilivorus*) from South Africa; an Elliot's Pheasant (*Phasianus ellioti* ♂); a Temminck's Tragopan (*Ceriornis temmincki* ♂) from China; four Spotted Tinamous (*Nothura maculosa*) from Buenos Ayres; two Indian Crocodiles (*Crocodilus palustris*) from India, deposited; eight Ocellated Sand Skinks (*Seps ocellatus*) from Malta, purchased; a Bennett's Wallaby (*Halmaturus bennetti* ♂) born in the Gardens; a Common Crowned Pigeon (*Goura coronata*), a Cockateel (*Calopsitta novæ-hollandiæ*) bred in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *AUGUST* 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 August 7

Sun rises, 4h. 35m.; souths, 12h. 5m. 33·8s.; sets, 19h. 36m.; decl. on meridian, 16° 27′ N.: Sidereal Time at Sunset, 16h. 40m.

Moon (at Last Quarter on August 11) rises, 20h. 58m.*; souths, 2h. 29m.; sets, 8h. 9m.; decl. on meridian, 6° 14′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	3 36	11 7	18 38	16 31 N.
Venus	8 44	14 42	20 40	1 14 S.
Mars	1 59	10 13	18 27	23 8 N.
Jupiter	11 32	16 45	21 58	10 3 S.
Saturn	3 3	11 0	18 57	20 37 N.

* Indicates that the rising is that of the preceding evening.

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

August.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
8	B.A.C. 81	6½	3 53	4 57	88 353
8	26 Ceti	6½	23 57	1 2†	105 240
9	29 Ceti	6½	3 24	4 5	172 240
13	48 Tauri	6	2 18	3 24	76 244

† Occurs on the following morning.

August.	h.	
8	1	Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Aug. 11,	20 49	*m*
R Persei	3 22·8	35 17 N.	,, 9,		*M*
S Virginis	13 27·1	6 37 S.	,, 10,		*m*
U Coronæ	15 13·6	32 4 N.	,, 9,	3 11	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 11,	1 42	*m*
			,,	21 50	*m*
X Sagittarii	17 40·5	27 47 S.	,, 10,	21 0	*m*
T Serpentis	18 23·3	6 14 N.	,, 12,		*M*
β Lyræ	18 45·9	33 14 N.	,, 10,	23 0	*m*
S Sagittæ	19 50·9	16 20 N.	,, 8,	21 0	*M*
S Cygni	20 3·1	57 40 N.	,, 13,		*M*
δ Cephei	22 25·0	57 50 N.	,, 10,	21 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

The present season is generally the richest in the year for meteors, being the season of the *Perseids*, and the neighbouring showers.

	R.A. °	Decl. °	
Near δ Andromedæ	7	32 N.	Swift; streaks.
Perseids	44	56 N.	Swift; streaks.
From Aries	44	25 N.	Swift; streaks.
Near β Persei	48	43 N.	Very swift; streaks.
From Camelopardus	96	71 N.	Slow.
Near θ Cygni	292	52 N.	Rather slow.

THE JUBILEE OF THE ELECTRIC TELEGRAPH.

ON December 12, 1837, William Fothergill Cooke, on behalf of himself and Charles Wheatstone, set his hand and seal to Patent No. 7390, the subject of the specification being: "Certain apparatus or mechanism which is constructed according to our said improvements for giving signals and sounding alarums in distant places by means of electric currents transmitted through metallic circuits." This, the first English patent dealing with the electric telegraph, contains the elements of a thoroughly practical apparatus, as the historical experiment of July 25, 1837, made between Euston and Camden Town, had proved. Unlike many other developments of practical science, the commencement of the epoch when electric telegraphy became a practical success in this country can be sharply defined, and what will become an historical event, viz. the commemoration of July 27, 1887, can strictly be said to be the true jubilee of the electric telegraph.

To say that the invention of which Cooke and Wheatstone were the pioneers has done more to transform the conditions of human existence than any other except the steam-engine, and some would add "gunpowder," is but to restate an acknowledged fact. The electric telegraph has so changed the conditions of our social existence as to become indispensable to the same, and we could almost as soon do without food and clothing as dispense with the power that has annihilated distance. The evolution of the electric telegraph as a means of transmitting intelligence from a distance did not, of course, commence from the year when Her Majesty began her reign. The names of Ronald, Schilling, Watson, Sömmering, Schweigger, Weber, Lesage, and very many others, will at once occur to those who give a moment's thought to the subject, as workers in the field long before Wheatstone made his famous experiment, but few, we think, will question the assertion that electric telegraphy as a commercial success distinctly dates from the year 1837.

The commemorative dinner was held in the Venetian Hall at the Holborn Restaurant on Wednesday evening, July 27, the Right Hon. H. C. Raikes, M.P., the Postmaster-General, being in the chair. A large number of representative men were present (the company mustering about 250), though during the last ten years or so death has sadly thinned the ranks of the "old hands," of the Electric, U.K. and Magnetic Companies. Amongst the men of science and others who attended were Capt. Fonseca Varz, Mr. S. W. Silver, Dr. J. H. Gladstone, Mr. C. B. Bruce, the Marquis of Tweeddale, Mr. William Crookes, F.R.S., Mr. Edward Graves, Prof. W. G. Adams, M. Caël, Mr. Jacob Brett, Mr. H. Weaver, Mr. John Pender, Mr. C. H. B. Patey, Mr. G. Shaw-Lefevre, M.P., Lord Onslow, Prof. Stokes, Sir Lyon Playfair, M.P., Sir William Thomson, Sir Frederick Goldsmid, Sir Frederick Abel, Sir Douglas Galton, Mr. J. C. MacDonald, Mr. Edwin Clark, Sir David Salomons, Sir George Elliott, Colonel Andrews, R.A., Mr. Matthew Gray, Sir James Anderson, Mr. Norman Lockyer, Mr. H. C. Fischer, Prof. Hughes, Mr. W. H. Preece, Sir C. Bright, Major-General Webber, Mr. C. E. Spagnoletti, and Mr. Latimer Clark. Letters expressing regret at non-attendance were received from the Marquis of Salisbury, Lord John Manners, Viscount Cross, Mr. W. H. Smith, Sir H. Holland, Sir W. Grove, Sir D. Gooch, Sir A. Borthwick, M.P., Dr. von Stephan (Berlin), Dr. William Siemens (Berlin), Mr. Cracknell (Sydney), Mr. C. Todd (Adelaide), and others.

The Chairman having proposed the usual loyal toasts, proposed the toast, "To the Memory of the Pioneers of Telegraphy," asking the company to join in an expression of reverence for those great and illustrious men who have ceased to be among us, by drinking the same in solemn silence.

The Chairman then said:—We have most of us perhaps read of that tumultuous sensation which the great Wheatstone confesses to have experienced when the message which he sent on that little journey from Euston Square to Camden Town was sent back to him by Mr. Cooke. I am perhaps not exaggerating the importance of that occasion when I venture to say that that evening as Wheatstone sat in the small cupboard of an office communicating with his colleague at a distance of some two miles, was one of the great epochs in the history of human progress; and if ever a spirit of prophecy has filled a man with something of a divine enthusiasm, it may well be that the man with whose name the system of the electric telegraph must ever be inseparably connected, may have felt his heart throb with something almost supernatural when he realized that the great work had been achieved, that the demonstration had been reached, and that the future of the science was assured. I venture to believe that when we look back upon the progress of those fifty years, we shall find in them the materials for a greater hope of the future of humanity than in almost any other record of any other period in the history of our race. I would remind you that the instrument which was employed by Wheatstone and Cooke displayed five needles, and that it was from the movements and combinations of those five needles that the whole of the alphabet was made up. Those five needles, we are told, were united by means of five copper wires laid in a groove in a triangular block of wood, and I am sure you will be interested to know that a piece of that block is in my hand at the present time, and well deserves to be preserved among the archives of science. Well of course we are with the experience of this half century well aware that this system in the first instance was crude and imperfect. Demonstrations had been arrived at, but perfection had to be reached. The difficulty that was immediately encountered in popularizing the system was obviated by the development of the railway enterprise of this country and the necessity which arose for rapid and certain communication along our lines of railway. However, some time elapsed before the real development of telegraphy in this country began. The London and Blackwall Railway was, I believe, the first to utilize the system in a practical way. In 1844 the Government of Sir Robert Peel were the first to realize how far the telegraph might be applied to the service of the State; and that year saw the establishment of a telegraph line from Waterloo to Gosport, and that I think you may say constituted the first public recognition of the value of the electric telegraph. In 1846 the first telegraph company was formed—the Electric Telegraph Company. In 1850 the first attempt was made to lay a submarine telegraph cable. A gutta-percha wire, without any metallic covering, was laid between Dover and Calais in August of that year, and you will be interested to know that I have also here a portion of that cable, which was fished up by a ship in the Channel so recently as the year 1875. In 1851 a cable was laid in substitution of this gutta-percha cable, which was protected by iron wires, and which was the commencement of a regular system of inter-communication between England and the Continent, and this cable I believe I am not wrong in associating with the name of one of those gentlemen who is happily still spared to be among us—I mean Mr. Crampton—and it must indeed be a great satisfaction to anyone who has been connected with great works of this sort to have lived, as Mr. Crampton has done, to witness their enormous development in the service of mankind. The first Atlantic cable was laid in 1858, and other companies arose during those years to compete with the first electric telegraph company, and multiplied throughout the length and breadth of England the agency of the telegraph. In 1870 the multiplication of the companies had become so great that their competition, though in some respects advantageous to the public, was yet so imperfectly regulated by State requirements, that the Government of the day determined to acquire the whole of their enterprises, and to place the telegraphs of the Kingdom under the direction of the Post Office. Now, I should like to say one or two words with regard to the instruments of telegraphy. We are aware that the first telegraphic apparatus employed by Wheatstone and Cooke was one which required five wires through which to transmit their message. It was found gradually that two wires would suffice to forward a message, and after that the progress of science enabled the operators to depend upon one. But after a time it became ascertained that a wire could be used for sending messages in two directions, and after that time four messages came to be transmitted on a single wire, two in either direction; and, as I dare say most gentlemen who are present to-night are aware, at the present time a single wire is being used at the central telegraph station in such a manner as to admit of six messages being sent in one direction, or one in one direction and five in the other, or any other combination of six messages. The first five-needle instrument was succeeded by the double needle, and the double needle by the single needle; all those systems were visual. Then came in the system of printing on a band of paper. At first the signs representing the letters were embossed on the band. This was further improved by Prof. Hughes's printing instrument, by which the actual letters were printed in ink. Then came yet another—the sounder instrument, by which messages are transmitted by sound without any record. With regard to speed, when the first electric telegraph was established the speed of transmission was from four to five words a minute on the five-needle instruments. In 1849 the average rate of transmission of a certain number of messages addressed to the *Times* newspaper was 17 words a minute. The present pace of the electric telegraph between London and Dublin, where the Wheatstone automatic instrument is employed, amounts to 462 words a minute, and thus what was regarded as miraculous fifty years ago, has multiplied a hundredfold in the course of one half century. Now you may perhaps like to know, though it is rather descending from the higher walk of this great subject, the number of telegrams which were sent through the Post Office in the United Kingdom last year. The number was 51,500,000; that is nearly 1,000,000 per week, and that number is still steadily increasing. 41,000,000, or rather more than that number, of these were inland messages, and of course a very great proportion of them were Press messages. I think you should realize the immense boon which the electric telegraph has bestowed upon the Press. I gather from such information as I have been able to obtain that the rate for Press messages, which as everybody is aware is very much less than the rate for other messages, is on the average not much more than 2*d.* per 100 words; and it is owing to these extraordinary facilities, afforded by the Post Office to the transmission of Press news, that the whole of the United Kingdom is put in possession at its breakfast table every morning of everything which it is necessary or important for anybody to know, as well as of a great many things which are neither necessary nor important. I believe that I am not wrong in saying that the cost to the public revenue of this reduced rate to the Press is not less than £200,000 a year, and that the newspapers of this country practically receive a subsidy of £200,000 a year in order to enable them to assist in the diffusion of intelligence. I imagine that the country is well satisfied that this should be so, and that there are very few people who would wish to abridge that privilege, having regard to the enormous importance to all classes of the community of being placed at the earliest moment in possession of the fullest knowledge of what is going on. But it is a fact that, owing to the recent reduction in the tariff of telegrams, the value of the telegram on the average to the State is now only 8*d.*, whereas two years ago it was 1*s.* 1*d.*; and before the State took over the telegraphs it amounted to as much as 2*s.* 2*d.* I think you may measure something of the enormous gain which the public has achieved by the acquisition by the State of the telegraph system when you look at these figures and reflect that the average price of a telegram at the present time is only about a third of what it was only seventeen years ago. I am saying this as if I were one of the public; but as Postmaster-General you must be aware that I have to regard this result with somewhat mixed feelings, and I am endeavouring, as far as I can, to denude myself of any official prepossession in putting before you from the popular side the advantages which you have obtained by the State employment of the telegraphs. I would add that if you would wish to obtain further knowledge of this most interesting subject, put in the most terse and pregnant way, you cannot do better than study a paper communicated to the Society of Arts by my friend Mr. Preece. The great agency of telegraphy which seems to form the vital principle of this planet upon which we live, this great agency which has not only gone so far towards annihilating space, but which seems at the present time to be regenerating light and revolutionizing motion, has all the future before it. Those who are enrolled in its service are probably the disciples and the apostles of a new and absolutely beneficial dispensation, and with them rests the future, in no small degree, of the human race, and the means of linking not merely ourselves to our distant colonies—and my noble friend who is beside me (Lord Onslow) reminds me how important is the connexion between England and the younger Englands beyond the sea—but by going forward in connecting the various races of mankind by binding island to island and continent to continent. The telegraph is doing in its own quiet, its own noiseless, and its own unobtrusive way, more than all the noisiest missionaries of peace and universal brotherhood have ever accomplished.

Mr. Edwin Clark, in acknowledging the toast on behalf of "The Past," described the origin of the Electric Telegraph Company, in the organization of which he took a prominent part, the difficulties he had to encounter in curtailing expenditure, and in putting into a thoroughly sound state the wires and the system of insulation which then prevailed. He pointed out that the railway companies were really in its early stages the greatest benefactors of the telegraph. He had been deputed to examine the telegraph system prevalent in Europe in those early years in connexion with the railways, and he had recommended what had now become universal on the railway system of this country—the block system.

Mr. John Pender, on behalf of "The Present," said:—My mission to-night is to tell you what submarine telegraphy has done. I am one of those few commercial men who at an early period of telegraphy saw that there was in it the promise of a beneficent instrument for the future progress of the world. I have gone into submarine telegraphy, not as an expert, but as one of those who have come forward and taken science by the hand, and led it up to the glorious results which we have seen. Twenty years ago there was only something like 2000 miles of submarine cables; now there are 115,000 miles; and it has cost something like £38,000,000 or £39,000,000 sterling to put that amount of telegraph cable to the bottom of the sea. There was a prophecy long ago that the earth was to be girdled round in forty minutes. Why, we have got as much submarine telegraph cable as will go round the world five times, and we can send a message round the world in twenty minutes at the present moment. You ask me where does all this submarine telegraphy extend, and I reply in those beautiful lines of the poet:—

"Far as the breeze can bear the billows' foam
Survey our Empire, and behold our home!"

Wherever the British ship goes, or the British flag flies, there we have the submarine telegraph; and at the present moment, while I am speaking to you, human thought is travelling like lightning to every part of the world. The future of that no man can tell. Of the 100,000,000 words which are now carried by submarine telegraphy, nine-tenths are for commercial purposes. When the history of these fifty years of Her Majesty's glorious reign is written, telegraphy, and more especially submarine telegraphy, will be shown to have done more than anything else to federate the great colonies with the mother country, to spread civilization throughout the world, and to make this great world of ours as near as possible one common country.

Sir William Thomson (who was warmly received) said:—I feel that when the telegraph has been so important a bond for all the nations of the world we ought to go even beyond our fifty years jubilee and think for a moment of the great names from other countries to whom the possibility of the jubilee of the electric telegraph has been due—the great apostles of electric science in France, Coulomb and Ampère,—Ampère, whose work and whose discoveries constitute the foundation of the most important of modern telegraphic and electrical instruments generally; Ampère, whose name has become Anglicized and is invariably used in measuring the currents which produce the electric light. Then Gauss and Weber, who made the first electric telegraph. The telegraph of Gauss and Weber, and the Munich telegraph of Steinheil, and the Steinheil key, which is the manipulating telegraph key of the present day—those were the elements of telegraphy. We justly rejoice that in England so much was made of the work of those grand pioneers in science. In America the race of practical work commenced almost simultaneously with our own in the splendid telegraph of Morse. In speaking of the telegraph we almost forget time and space, and I must go back to the previous work of Henry, who anticipated in some points some of the finest discoveries of Faraday, and laid a large part of the theory of current induction, which is at the very root of some of the most splendid realizations of modern electric science, not merely for the electric telegraph, but for electric lighting. By the work of 1857—a few years before the half of the jubilee—the two brothers, Edward and Charles Bright, and Whitehouse, those three men, with Mr. Cyrus Field, reduced to practice that brilliant dream of Cyrus Field to connect England and America by means of submarine telegraphy. Then there were the brothers Werner and William Siemens working in the same direction, and the great navigator Moriarty, who was out in the *Agamemnon* in 1857, and navigated the *Agamemnon* in 1868, and was on the *Great Eastern* as navigator with Sir James Anderson. In 1865 he picked up the cable where it was broken, and in 1866, coming back a year after to the same place, hit upon it just a quarter of a mile away, by his splendid navigational powers. Canning and Clifford were also engaged in the work; then there were Varley and Jenkin (who was my special partner) with both of whom I worked for many years. I alone am here to speak for the three. Willoughby Smith, who did such fine work in 1865–66 in testing the cable, applying the newest developments of science, many of them his own inventions, to do what had never been done before, to test a submarine cable with the delicacy that was necessary under circumstances so peculiar, so utterly new. I am exceedingly sorry he is not with us this evening. But I can never forget that we scientists alone could not have done what has been done. To two men, I believe, is due the existence of the 1865 cable, and all the consequences that followed from the 1865–66 cable—John Pender and Cyrus Field. But I must remember that there are other things besides ocean telegraphs. You have told us how splendidly the land telegraphs are worked; you have pointed out how admirably under the influence of the Government system, the application of science to telegraphy has been developed. I think you may feel proud, sir, in knowing that under Government management within these last seventeen years the applications of science to telegraphy have not stood still, but, on the contrary, have been pushed forward with every possible energy and with the most marvellous success. You have told us that the rate of working between Dublin and London has reached 462 words per minute, I think we may say 500 words per minute, and that is ten times what it was ten years ago. That is something for a Government department to be proud of, and for a Government I must say there is some little political importance in the fact that Dublin can now communicate its requests, its complaints, and its gratitudes, to London at the rate of 500 words per minute. It seems to me an ample demonstration of the utter scientific absurdity of any sentimental need for separate Parliaments in Ireland. I should have failed in my duty in speaking for science if I had omitted to point out this, which seems to me a great contribution of science to the political welfare of the world.

Sir Lyon Playfair, M.P., proposed "The Scientific Societies." The scientific Societies, he pointed out, did not profess invention; they professed to lay down the laws of science and to advance natural knowledge. Men who had contributed to the advancement of natural knowledge, like Oersted and Ampère, were as much discoverers of the electric telegraph as if they had themselves actually aided in the invention. The duty of the scientific Societies was to cultivate the tree of knowledge. A great invention never came, as Minerva did, panoplied in complete armour from the brain of Jupiter. But even the brain of Jupiter could not produce this wonderful product of evolution until he had swallowed her mother, Metis, while in the first month of gestation. Our great inventors swallowed science, the mother of invention, and then produced their progeny, always, however, in a state of infancy. The steam-engine, which had done so much for human progress, has had so many inventors that a Court of law, reviewing the steps of invention, came to a solemn decision that Watt had done nothing for the improvement of the steam-engine. Scientific Societies, looking to the advancement of science for its own sake, laid the surest foundations for industrial applications. Oersted and Faraday were as true discoverers of the electric telegraph as Wheatstone, Cooke, or Morse. It was not the annihilation of space and time which was the most wonderful result of the telegraph, but it was the profound change which it had produced in our systems of government and of commerce. Who at its introduction would have supposed that the whole system of commerce would have been transformed by it, that capital would have to change the channels of its usefulness, and that the system of exchanges would undergo such profound alterations? If telegraphy had one lesson which we should lay to heart it was this—that science should be studied for the sake of knowledge, because discoveries in natural knowledge led inevitably to industrial inventions. We should, in regard to all discoveries, however unimportant they seemed, do everything in our power to promote their growth, and the growth of natural knowledge throughout the world.

Prof. Stokes said :—Scientific men know well how fascinating is the pursuit of science. Some have even gone so far as to consider that it would be polluted, if I may so speak, by being applied practically. An eminent foreign mathematician delighted in the theory of numbers because one could not conceive that it could have any practical application. An eminent English mathematician I heard express a somewhat similar sentiment. All honour be to those who are so immersed, if I may so speak, in abstract science, that they disregard and even dislike its application. They pursue science with all the more zest, they pursue it in directions which possibly otherwise might not have been followed out, and possibly in the end their own investigations may admit of applications which they never dreamt of. But for my own part my tastes do not quite lie in that direction. I like to see science connected with applications thereof, no matter to what purpose. Now, when we apply abstract science to physical subjects, we are not only enabled to investigate natural phenomena in a manner which could not otherwise be done, but the study reacts on the most abstract parts of science, and enables us sometimes to see, as if it were by intuition, truths of an abstract nature, such as, for example, propositions in pure mathematics, which we perhaps should never have arrived at if we had not viewed them through the spectacles, so to speak, of their physical application. But this is not all. When science comes to be applied to the wants of life, scientific men are placed by the practical man in the condition of making experiments which oftentimes would otherwise be impossible. When science comes to be applied to commercial purposes, it then becomes possible to construct instruments on a scale the expensiveness of which would have been utterly prohibitory to the purely scientific man. But when these instruments are constructed, it may be, for commercial purposes, the scientific man on his part is able by the favour of those who have constructed them, or of those for whom they have been constructed, to make experiments with them which oftentimes are of great interest from a purely scientific point of view.

Dr. Gladstone, responding on behalf of the "Royal Institution, said :—At the Institution there were not merely memories binding them to all those who had passed away, but they had also many relics. They preserved the log-books of Davy, Faraday, and others, and not only that, but there a great number of pieces of wire, sealing-wax, and card, all damaged, and many other things which Faraday especially used to delight to work with and to carry out in the first experiments which were suggested by the ideas that were working in his brain, and these were preserved as germs of some of their great discoveries. And here he wanted to point out one of those germs connected with telegraphy. Those relics preserved at the Royal Institution were only worth originally a few pence or shillings. How different the monster wealth which had now become the capital of those great enterprises! As far as the Royal Institution was concerned, its connexion with the electric telegraph was to a certain extent not direct, and yet it was very real. Sir Humphry Davy worked there of course largely on galvanic electricity, but he belonged to the pre-telegraphic era. Faraday himself commenced to work early on these matters, and continued to try and image in his own mind what was taking place in these phenomena. It was an important point with him that the glories of science should conduce to the benefit of man. They knew his influence with Sir Charles Wheatstone was very great, and he got him into the dark chambers at the Royal Institution and talked to him about his investigations, and in the theatre brought before him some of those experiments which were afterwards performed with so much success in public. In one way and another Faraday had to do with the industrial applications of electricity, as well as with scientific discovery. They had the combination of the purely scientific man with the practical man, and then the two going together with slow, careful, conscientious investigation, followed by the energetic carrying out of the discovery in a form which benefited mankind.

Mr. Shaw-Lefevre, M.P., in proposing the "Societies representing Applied Science," said :—When I was a boy, at Eton, I recollect well the extension of the telegraph from London to Slough, and an incident of which you are all aware, the arrest of Tawell, which I believe brought the telegraph more into notice than anything else at that time. It might have been expected that the authorities of Eton, seeing a great invention of this kind brought to their door, might have been desirous of explaining it to the boys of the school, and might have been drawn out of their deep slumber of ages and done something for the scientific education of the boys then at Eton. But no thought of the kind ever entered into their minds, and the only notice taken of it at the time was that they set it as the subject for Greek verses. I and all the boys of my class commenced racking our brains to write some Greek iambics upon a subject of which we knew nothing, and to bring it into connexion with the mythology of the ancients, of which we knew a good deal. I refer to this for the purpose of showing you how little scienee was promoted then at our public schools. I am glad to say that things have been changed since then, but much has to be done in this direction, and there cannot be a doubt that if this country is to hold its own in the great industrial competition it must give a greater place to science in our schools, and equalize the endowments between the classical and scientific studies; and it is only by doing that I am persuaded that we can hold our own.

Mr. Bruce, President of the Institute of Civil Engineers, and Mr. Latimer Clark, past President of the Society of Telegraph Engineers, replied; and the latter, after alluding to the work done by the brothers Brett in submarine telegraphy, said :—I feel that we, as the representatives of applied science, have been somewhat neglected by the world. I feel that the politicians, some of whom have honoured us by coming here this evening, have very much neglected us. I don't allude to honours, for we shall be very well content with the position of things in that regard; but I feel that they have robbed us of much of our credit for the fact that the great effects of the jubilee which we are now assembled to commemorate have been due to the agency of the applied sciences. I do feel that politicians have been permitted to claim for themselves the credit for the wondrous benefits civilization has received from the efforts of applied science. We hear that so many shillings have been taken off a quarter of wheat, we hear that all the prosperity of the country has been due to free trade, but I say it is not so; I say they have robbed us of our honours in saying that; I say as guild and craft we ought to proclaim loudly to the world that to our efforts most of all the prosperity of the last fifty years has been due. The population of this great city and of this country when it eats its breakfast to-morrow morning will be consuming food a very large proportion of which has been brought to this country by means of applied science. It is that which has given us our prosperity. They have not taken 5*s.* or 10*s.* a quarter off wheat and corn and eatables, but they have taken off 60*s.*, 80*s.*, and 100*s.* Wheat will be brought to-morrow from places from which it could not have been brought fifty years ago for ten times what it now costs. As a guild and craft we ought to proclaim loudly that the benefits which we have conferred are the real cause of the prosperity of the great Victorian era which we meet here to celebrate.

The Earl of Onslow having proposed the health of the Chairman in a suitable address, and the Chairman having responded, the proceedings terminated.

The Postmaster-General, during the evening, despatched the following telegram to Sir Henry Ponsonby, at Osborne :—"A large dinner-party celebrating the jubilee of the electric telegraph, remember with gratitude and pride that all the progress has taken place in the happy and prosperous reign of Her Majesty and under her fostering care."

To this the following reply was received :—"The Queen thanks you for your telegram. It gives Her Majesty much pleasure to reflect on the improvements which have been made in Wheatstone's great invention, which was first practically tested in her reign."

THE CASE FOR A LONDON TEACHING UNIVERSITY.

THE questions connected with the proposal for the establishment of a Teaching University in London were discussed in a speech delivered by Sir George Young at the distribution of prizes in the Medical Faculty of University College, London, on June 1, and in a speech delivered by Dr. J. E. Erichsen at the distribution of prizes in the Faculties of Arts and Laws and of Science, at the same institution on June 30. As the case for a London Teaching University was stated by these two eminent authorities, we reprint part of what they had to say on the subject from the point of view of University and King's Colleges.

I.

Having referred to the drawbacks connected with "the system of separation between institution and institution concerning University matters" in London, Sir George Young went on to say:—

I will touch upon some of these drawbacks—drawbacks which, as I have said, I do not impute as matters of fault to any man or to any set of men, but to the mischiefs of the system; and I will draw my instances (and you are to consider that I could give you many others), as in duty bound upon this occasion, from the medical side of the question.

Well, gentlemen, in the first place we are brought face to face with a very serious and very unpleasant condition of things in the fact that several of our students, we find, are in the habit of leaving us from time to time in order to finish their course of study at other institutions where degrees are conferred, in order to qualify for those degrees. We have always been, as Broke said of the *Shannon*, "an unassuming ship," and I am not going to boast. Let us admit that there may be elsewhere teachers as eminent as those I see around me. Let us admit that there may be elsewhere possibilities of study comparable to those which are to be obtained in this place. But I will not admit—it is my duty to deny, and the point is conceded by others outside our limits—that there is anywhere a more eminent body of professors and instructors than that which has now, for two generations, led the van in matters of medical instruction of a university kind in this College. I will not admit that there is anywhere, in any part of the world, a field of study presenting greater opportunities to the student than that of London with its numerous medical schools, and with its numerous facilities for scientific study.

Next let me mention an evil, for which the University of London is not responsible—for which nothing, indeed, can be said to be responsible except the non-existence of that university which the University of London is not. Not only are medical schools, as we know well, dependent upon hospitals, but also, what is not so generally known to the public, hospitals are dependent for their administration upon medical schools. As London has spread and as one general hospital after another has been founded, each has attached to itself its own separate medical school. Each school must provide, in order to satisfy professional requirements, not merely that clinical teaching for the sake of which it is founded, but also scientific teaching of a multifarious and expensive character. In some of these schools, as is well known, it has been found impossible to provide this scientific teaching in a manner sufficiently effective for the purposes of the school. There need be no delicacy, gentlemen, in mentioning this, because, in fact, it has been most honourably acknowledged by several of these medical schools in their recent action. It was lately brought to our notice that in the case of several of them, they have practically, in some branches, given up the scientific training of their students, and have entered into an arrangement with the Government school at Kensington, by which their students should there receive that teaching which they found themselves unable to give. Well, gentlemen, at the Council of this College we had something to say—we had some objection to take—to that arrangement. With that I need not trouble you further than to say that we thought a Government department ought not to lend itself to an exclusive arrangement of this kind. We thought that it would have been better for the students themselves and for the public if the matter had been left open whether they should go to South Kensington for their chemistry and physiology, or come, if they so preferred it, to ourselves. But at the same time, gentlemen, you must not consider me in this matter to be impeaching the conduct of the other schools. As men of the world, we are quite aware that medical schools are to a certain extent rivals, and we cannot expect, merely because we asked it, that the natural jealousies of rivals should be allayed, and that a medical school in so delicate a matter should freely accept our offers of instruction for its students who, they might suppose, would perhaps be detached from their affection for their own school by frequenting this place. Well, gentlemen, what is the remedy that we should look to? I think that you will agree that we ought not, as a Council, to sit down and seek no remedy for such a state of things as this. Why, surely the remedy is that some central authority should be provided—some institution where we can meet our sister schools upon equal terms, not that wholesome emulations should be extinguished, but that the mischiefs which arise from their excess should be obviated, where, in fact, teachers and administrators might meet together for the purpose of arranging for improvements in medical education upon a common footing and without fear of mutual injury.

This, among other instances of the same kind, many of which I could give you, led some of us, as much as three years ago, into a long inquiry into the matter, and eventually into a movement for the foundation of a Teaching University in and for London. The year before last, at the meeting of the sister Faculties, the Dean of the Faculty of Science, Prof. Graham (whom, I hope, we shall see before long among us restored to health), called our attention to the movement and expressed his sympathy with it on the part of the Faculty. The President of the College, Lord Kimberley, expressed also his warm sympathy with the movement, and said (I am quoting his words, which will be found in the Report of the College of that date):—"There is nothing more dissatisfying to the minds of students and of educational men, than that in this great city there should not be some more complete establishment of some universal system. We may not see it accomplished. I do not suppose that anyone sees at present how the end is to be attained, but I am quite certain that it would be for the benefit of all the institutions of this great city that they should be gathered together and the teachers and managers brought into a close and immediate contact." With that encouragement we, many of us, took up the movement warmly, and it has now been brought to the practical stage of definite proposals and of a formal programme. We ask, in short, that the same privilege which has already been conceded to country colleges through the Victoria University shall be conceded also to us in London. Gentlemen, we cannot go to Manchester. We cannot so far ignore our position and our history as to seek for admittance from the offspring of our offspring. Besides, we ought not to be compelled to go to Manchester. The system of the Victoria University, as I have indicated, is that of an imperfect university, arising from its being scattered over several cities at great distances from each other. There is within our reach the more complete system of a localized university; for who will have the face to say that in this great population of something like four millions residing within limits admitting of daily intercourse there is not material enough—there is not ground enough—to support a local teaching university of its own?

There were working with us for a long time, in the course of this inquiry, several active members of the Convocation of the University who had themselves been interested in similar movements, and who desired to see the development of that University in the direction to which our hopes and wishes also pointed. By their exertions, and as a consequence of our movement, the Convocation and the Senate of the University of London have been brought separately to consider this matter, and have put forward from time to time certain proposals for what I must call a compromise. Those proposals have been officially communicated to us through our President, and have been, I need not say, carefully considered by the Council. They do not amount to that which we desire. They did not cover that which we claim. They are limited to this: in the first place, that there should be introduced in the Senate of the London University eight representatives of the four Faculties of the University—two to each—such representatives to be chosen by assemblies of the Faculties, and the assemblies of the Faculties to consist of representatives of the institutions throughout England which send up candidates to the University. There is also a provision for Boards of Studies to be constituted out of these same Faculties to exercise purely consultative and deliberative functions. I think that is all—all, that is to say, of a practical character. There is no doubt—and it is an important matter considered from the point of view of our argument—a proposal that in the preamble, in the objects of the University, there should be included a statement, that its purpose is to encourage education, especially in London; and there is also a proposal that as time and opportunity are given, it shall have power to found Professorships, provided that such Professorships shall not compete unfairly with our own. But, passing over these two latter proposals as not exactly of a practical character, I say that the proposal to admit eight gentlemen, representatives of assemblies the constitution of which is not very clear to me, upon the Senate of the University, and to appoint these Boards of Studies composed of representatives so gathered from all parts of England, is not what we want. It does not meet our views; and for several reasons.

In the first place, the representation which would be given to this College on such a system in the governing body of the

University, if it is to be a teaching university and not merely an examining one, is entirely insufficient. In the next place, the Boards of Studies, the Faculties, and the new members of the governing body, would represent, not London, but all England, and therefore would constitute an organization entirely different from that effective organization which we contemplate, which is to meet often, to take count of teaching-matters in London, and to do a great deal of work in the development of University education in this place. We therefore are obliged to reject these proposals, and to proceed, without any feelings of hostility and without any bitter words towards the University of London, to ask for that which we consider necessary for the effective carrying on of our own work.

Let us keep clearly before us what a teaching university really is. We need not go farther than this institution to see it, provided only that were added to us which we want—the power to confer degrees. It has not merely to provide for—it has to commend to its students the best possible teaching under the best teachers obtainable in all the subjects of the University. A university which by its very constitution is indifferent to methods of teaching and does not care how a man has obtained his knowledge, cannot be said to commend to its students any particular methods or ways of obtaining that knowledge. It rather has a contrary effect. Under these circumstances it is useless to try to set up an institution which shall combine a mixture of two principles—the principle which considers degrees merely as the marks of a liberal education, tested, no doubt, by an examination, but covering very much more than the mere showing of knowledge in examination, and the other, the rival system, which, giving up the testing of methods of education—giving up the marking of a regular education as beyond its scope—confines itself to the setting of a mark upon performance in the answering of examination-papers.

No, gentlemen, if the University of London were to move in this direction, it would spoil its own thoroughly good and honest work without doing ours. There will still remain when we have obtained this Charter, plenty of candidates for its degrees—plenty of work for it to do—plenty of honour to those who obtain them. But surely there is room for us by the side of it. There is room for an institution which shall comprise not merely this College, but King's College and the Medical Schools of London, and which, organizing them together as a Teaching University, shall give us that which we want for the efficiency of our work—an institution in which the teaching which we give is duly honoured—is not placed in an inferior position beside the teaching which is given by other universities and in other university colleges.

We ask, therefore, that a Charter to confer degrees upon all persons who have undergone a regular course of study in a college or medical school of the University and have passed the required examinations, shall be granted to a suitable governing body, upon which the governing bodies of this College and of King's College shall be properly represented, and upon which the teaching staff not merely of this College and of King's College, but also of the other Medical Schools of London, shall have their representatives. In order that the interests of the medical profession may be properly considered—in order that we may not seem to claim that which it is by no means our intention to claim—an unfair position for our own medical schools, we ask an alliance with the Royal College of Physicians and the Royal College of Surgeons, the official representatives of the medical profession in London, in order that by their means that representation may be secured upon the governing body of the University. It is obvious that from their number it would be difficult to represent directly the separate interests of eleven medical schools upon the governing body of a University; but, in so far as the teachers of the various schools have their voice—and that voice, I can assure you, we do not intend should be a small one, in the councils of the University—in so far, we shall consider the teachers of other medical schools entitled to rank on equal terms with our own. . . .

There is a movement at present on foot in the College of Physicians for a single-facultied University in London, or an institution in the nature of a University, for conferring medical degrees alone. That movement appears to us to be a part of our movement. By itself, and if the movers insist upon its being considered as essentially a separate movement, we could not look upon it with approval; for we believe that it would be fatal to the prosperity of our medical school. I will put it to you, gentlemen, How would you, the students in this College, regard a state of things under which you were called upon to work for a degree, either at the University of London at Burlington House, or at the Royal College of Physicians? If it were the case, as seems to be indicated, that the degree at Burlington House is to be connected with a very high, a somewhat unusually high, standard, and if the degree which is contemplated by the College of Physicians is to be that creditable average degree which I have indicated as one which, personally, I think ought to be established, do you not see that those medical schools, which like our own, aim at the highest teaching, would have serious difficulties in the matter? Here would be two systems in neither of which we had the least voice, two systems of examining Universities outside us competing for our students; and what would our Professors do? They would be called upon, now to train for one system and now for another, and perhaps to keep up double sets of classes, so constituted as to fit the arrangements of two rival bodies.

That is the position in which we should be placed. But if the movement on the part of the Royal College of Physicians (the Royal College of Surgeons joining in it) can be brought into accord with our own, then we shall have already obtained a part of what we seek. I will just mention one reason why I think it most desirable that you, the members of the medical profession, should take this matter seriously into consideration, and should exercise your influence with your colleagues in other institutions, in order that this point may be pressed home to them at the present stage. The visit which it was recently my duty to pay to the Privy Council Office, in order to obtain the forms necessary for carrying out our own proposals, revealed to me the fact that there exists already in that office a pile of petitions as high as this table against the proposal of the Royal College of Physicians. Now, gentlemen, against our proposals there is no petition and there is no movement. So far as I know, there is no objection in the world.

We do not conceal from ourselves that it is possible opposition may be offered as we go on. That opposition which above all others we should deprecate would be the opposition of the University of London. I have endeavoured to preserve a tone of friendship, such as I sincerely feel, towards that University. I most earnestly deprecate opposition on the part of that distinguished body to the movement which is now on foot for obtaining a University in and for London such as London ought to have. I trust it will not be led into the fatal track of the older Universities, which, by their interference, did not prevent, indeed, the foundation of the University of London, but undoubtedly spoiled it, fifty years ago. That such opposition may be apprehended by some of us we cannot ignore in consideration of the very serious matter to which I have lastly to call attention, the resignation, namely, of our President and of several members of the Council among us. Gentlemen, that these resignations have been to some extent a surprise to us, that they have been a serious cause of anxiety to us, must be obvious; but I think that they have been partly due to a misunderstanding of our aims. I think that the objections which have led to them will, to a large extent, vanish when our proposals come to be more carefully looked into. In the meantime, as for us who remain, we are not disheartened, we are not discouraged. We have at least the satisfaction, such as it is, that the Council of this College is now unanimous in the matter. We have the source of satisfaction which is afforded to us by the unanimous support of the general meeting of the College. We have at our backs the unanimous support of our distinguished body of Professors. We have at our side the unanimous assistance of the great College once our rival, but now our cordial ally. Besides King's College we have friends in every medical school in London, who are corresponding with us and working in the same direction as ourselves. We have friends and well-wishers, I may say further, in every University in England. We have friends in the Press, and we have supporters in the public, and we have received the most encouraging intimations that it will not be long before we are able to fill our depleted ranks in the Council with names which will inspire confidence, and which will materially assist us in carrying our work to a conclusion.

Finally, gentlemen, we have this more than any other as a source of encouragement—that we see our way—that we know the work that has to be done and realize the way in which we hope to effect it. Three years of study and perhaps scores of meetings and conferences have not left us entirely ignorant of the ground. We intend to make this institution greater, more splendid, more efficient, than it has been hitherto, and we expect to succeed, because we are labouring not merely for our own aggrandisement, but for the foundation of a University in and for London which will be of incalculable benefit to University education in this mighty centre of population where we live.

II.

Dealing with the objection that a new University cannot be necessary in the capital, since we have already got the University of London, Dr. Erichsen said:—

I wish to speak with the very greatest respect of the University of London, and I entertain the highest respect for the work that has been done in that great institution during the half century that it has been in existence; and I think that everyone connected with University College must always speak and think of the University of London with that affection with which a parent looks at his child, the University of London being the outcome of University College. We may sometimes look upon it with that feeling of mixed affection and regret with which we contemplate a child that we think has not always been so grateful as it might have been for the favours received in its early life. But, however that may be, we all speak of the University of London with, and we all feel towards it, the greatest respect and a certain affection.

But the University of London is, in truth, not a university in any sense of the term. The title is misleading and is a misnomer. By a "university" is meant an association of teachers and of students, properly organized, destined for the increase and the transmission of all learning, of knowledge in all its branches, and containing complete Faculties of Arts and Laws, Science and Medicine, and empowered to grant degrees to those of its pupils who are found to be sufficiently qualified for such a distinction. The University of London never has pretended to be a teaching institution, and, so far as its present constitution is concerned, never can be a teaching institution. If it were to become a teaching university it would require to become so completely altered in its constitution as practically to become a new institution. The University of London has only performed one of the functions of a university—that of examining candidates for its degrees. It has performed that function admirably well. The examinations have been carried to a very high standard, so much so that the ordinary pass-examination in some subjects is almost an honours examination. Yet it is only a degree-giving institution, and not a university in the sense in which a university is generally known.

Nor is it "of London"; for, as was truly said by the Dean of the Faculty of Science in that admirable report that we listened to at the commencement of these proceedings, it is an Imperial University, which draws its candidates from almost every part of the habitable world. It has nothing to do with London except that its head-quarters are situated in Burlington House; but, so far as London is concerned, it might just as well be situated anywhere else. The University of London, then, does not, in any way, supply the want that we wish to fill. With regard to London itself, I may say this, that even as an examining institution the University of London does not supply the desire that has sprung up of late years for academic distinctions. It does not supply the desire amongst the inhabitants of London itself. I can speak of my own profession. Of late years there has been a craving in the medical profession for the possession of degrees. As Sir George Young stated very truly in distributing the prizes in the Medical Faculty about a month ago, if there were no degrees at all we should be none the worse for it; but one may also state something like the converse of that proposition, that if everybody has got a degree nobody is a bit the better for it, and what is common to all can be an honour to none. However, that there exists a great desire for degrees and for academic distinctions there can be no doubt. Well, do the students of the medical schools in London go to the University of London for those degrees? Not at all. They go elsewhere. They go to Edinburgh; they go to Cambridge; they go to Oxford. At the present moment there are about nineteen hundred medical students at the University of Edinburgh, and nearly seven hundred of them are English. They are attracted there not so much by the superiority of teaching, because—and I say it with all respect to the University of Edinburgh, to which I have reason to be very grateful—the teaching, high as it is, and excellent as it is in all its departments, is not better than the teaching in four or five of the principal medical schools in London; but the students go there simply in order to obtain a degree, because at the end of their studies, instead of coming out as simple Mr. So-and-So, they come out as Dr. So-and-So. Well, the others who do not go to Edinburgh, go to Cambridge or go to Oxford; and there is a very large medical school now at Cambridge also, frequented by young men who are desirous of obtaining the degree of that distinguished University. The following incident will show how little the University of London supplies the need for degrees which is felt by London medical men. A few weeks ago there was a vacancy at one of the large hospitals of London for an assistant physician. There were no less than twelve or fourteen candidates. They were all graduates of British universities, and out of this large number of candidates, all London men, educated more or less in London, and practising in London, and attending hospitals in London, there was only one candidate who was a graduate of the University of London. All the others were graduates either of Oxford or of Cambridge. I say, therefore, that men go away from London to get their degrees at the present day. They go to Edinburgh, they go to Oxford, they go to Cambridge, they go elsewhere; but the vast majority do not go to the University of Loudon. That, as a degree-giving institution, does not supply the needs of London itself.

The proposed establishment of this new teaching and degree-giving university has been termed an act of hostility, a kind of declaration of war, against the University of London. Now I can say truly, speaking in the name of my fellow-members of the Council here, that there is no such feeling whatever. No such feeling has animated, I believe, any one of the Council or any person connected with this institution. This proposed university will compete, probably, to a certain extent, with the University of London, but it will compete much more with other universities. It will compete much more with the University of Edinburgh; it will compete much more with the University of Cambridge. There is no direct competition intended with regard to the University of London. There is no reason why a new university should not be established. There is no more reason to complain of competition in the establishment of a new university than there is in the establishment of a new school. Every new school competes with every other school in existence. There is no more reason to complain of it than to complain of the introduction of a new member into any of the learned professions. Every man who becomes a lawyer or becomes a doctor may be said to compete with every existing lawyer or doctor. In the same sense the new university, if established, might be said to compete, more or less, with every existing university in the kingdom. In this case there is a competition of friendly rivalry, but nothing else; and beyond that I cannot admit that there is any special competition with regard to any existing university.

If the University of London does not supply the want felt for higher education, how is that want to be supplied in London? There are only one or two methods. You must either take existing institutions, or you must create a *tertium quid*, and what that *tertium quid* may be I know not. But what existing institutions are we do know, and we do know that there are two institutions in this metropolis which for the last half century have been doing the only work in London that approaches to the higher education, or approaches in any way whatever to university education. They have done that work diligently and well under great difficulties and great disadvantages, but with a fair share of success. I mean this College and King's College. Those are the two institutions; and by the combination of those two institutions we may fairly look for the establishment of a new university in London fully capable of discharging the functions of such an institution.

I happen, from circumstances, to be personally acquainted with, I believe, every university in the kingdom; and I can say that so far as the equipment of universities is concerned, in the way of museums, laboratories, libraries, lecture-rooms, and all other appliances—what may be termed the "plant" of a university—these two institutions taken singly are equal to most; this one certainly is, and taken in conjunction they are superior to almost all, except the old Universities of Oxford and of Cambridge. I put them aside; but these institutions, University College and King's College, taken in conjunction, are fully equal in all the requirements of a university to the other universities in Great Britain, the Scottish universities and the two universities of this country—one in the north and the other in the midland counties.

I cannot speak with any precision of detail of King's College, but I can speak with precision of this College; and it may be interesting to you to know what this College really is, and what it can present to the public in the way of supplying the requirements of a great teaching institution of university rank.

This College, in the first place, has complete Faculties of Arts, Laws, Science, and Medicine, and a School of Fine Arts, as well as a Boys' School. This College has fifty-eight professorial chairs in operation. In addition to the fifty-eight professors, there is a large teaching staff both on the general and on the medical side,—teachers, lecturers, demonstrators, and so on, —bringing up the whole members of the teaching staff to something like one hundred. Last session this College had between nineteen hundred and two thousand students. There were five hundred and fifty boys in the school. The buildings of this College, containing, as I have said, museums, libraries, lecture-halls, laboratories, and all the appliances of a university, are spread over seven acres. They cost £300,000 in construction. This College holds on trust no less a sum than £200,000, chiefly devoted to prizes, scholarships, and other objects of that kind; and it holds, besides, in trust a sum of £135,000 for hospital purposes. Its income is between £33,000 and £34,000 a year. Taking, therefore, this College alone, so far as its buildings, the contents of its buildings, and its pecuniary resources are concerned, it stands on an equal footing with several of the universities in Great Britain; and, taken in conjunction with King's College, it stands undoubtedly superior to some.

To this College, therefore, in combination with King's College, we may fairly look to the attainment of our object of establishing a Teaching University in London which will bring the higher education of London to the doors and within the pecuniary resources of the less wealthy classes of the metropolis, so that the disgrace that has hitherto attached to the metropolis of not affording a higher education, and the discredit that university education in England is to a very great extent a privilege of the wealthier and of the well-to do classes, may be wiped away. It should be within the reach of all, even of the student of the most humble means; and it would be well if this country were to take the example of Scotland in that respect, and to follow it. . . .

In this new Teaching University there are two requirements that we insist upon. One is that the candidates for its degrees should have spent a certain specified time in attendance on lectures and instruction within its walls; and the other is that the examinations should be superintended and conducted by the teaching body of the University.

With regard to the first of these two points I wish to say a few words. . . There is something more than mere knowledge that is acquired in academic instruction. There is a culture of mind and a development of the moral and social nature that cannot be acquired by solitary study; and it is for these reasons amongst others that those who are in favour of this movement are desirous that the candidates for the degrees of the new University should prosecute a portion, at all events, of their studies within the walls of the institution, so that they may imbibe something of the spirit, and that they may be in some way, too, impregnated by the *genius loci*. This has been stigmatized as retrograde; but surely there can be nothing retrograde in that which has been found by universal experience to be the better system of education, and which is adopted in every teaching university in the country.

There is another point, and that is in regard to examinations, and it is a very essential point. We feel, and we feel very strongly indeed, that the examinations should not be directed by an outside body on which there are perhaps no examiners and no teachers, but that the examinations should be conducted by the teachers themselves in the institution in which the candidate learns. I do not say by the individual teacher of each class, but by the general body of the teachers, and that is a very different thing. And, as there would be more Colleges than one in the new Teaching University, a candidate need not in any way be examined by his own teacher, although he would be examined under the direction, superintendence, and control of the general body of the teachers. In every university now, I believe, throughout the Kingdom the teachers are assisted in their examination by assessors or by extra-professorial aid, whenever it is needed, and such, of course, would be the case in the new University. We feel that examinations ought not to lead teaching, and that if examinations are allowed to lead teaching, the teaching is fettered by the examination, and you get to a system of "cram"; the higher education and the higher teaching are apt to be neglected. I recollect many years ago a circumstance illustrating this, occurring in this College in connexion with Prof. Sharpey, one of the most distinguished men ever connected with this College, the first Professor of Physiology here and, indeed, in London. There was no course, properly speaking, of Physiology given in London until Prof. Sharpey began his lectures here in the year 1836–37. Prof. Sharpey gave an elaborate course of Physiology. From the commencement he attracted crowds of students. At that time there was connected with this College a most estimable and most amiable and most excellent old surgeon, who had grown grey within the walls, as it were, of the unreformed College of Surgeons, Mr. Samuel Cooper. He was an examiner of the College of Surgeons, and I speak of him with the greatest respect; but he was never able to raise his mind beyond the requirements of the examinations of that institution. When he heard of what Prof. Sharpey was doing, he said, "What is the good of Sharpey teaching them all this kind of stuff? We do not want it at the College of Surgeons. We have never asked for it at the College of Surgeons. Why should he teach it to them?" He had no conception beyond that, and that is the frame of mind that affects every mere examiner. He has a tendency to fetter and tie down the teaching to the level of his own examinations, and it is impossible to bring him or an examining institution above that level. We therefore wish that the instruction should lead the examination, and that the examination should follow in the wake of the teaching, and not the teaching in the wake of the examination.

SCIENCE AND REVELATION.[1]

On the present anniversary, which is the conclusion of my first year of office as President of this Institute, I propose to address a few words to you bearing on the object of the Institute, and on the spirit in which, as I conceive, that object is best carried out.

The highest aim of physical science is, as far as may be possible, to refer observed phenomena to their proximate causes. I by no means say that this is the immediate, or even necessarily the ultimate object of every physical investigation. Sometimes our object is to investigate facts, or to co-ordinate known facts, and endeavour to discover empirical laws. These are useful as far as they go, and *may* ultimately lead to the formation of theories which in the end so stand the test of what I may call cross-examination by Nature, that we become impressed with the conviction of their truth. Sometimes our object is the determination of numerical constants, with a view, it may be, to the practical application of science to the wants of life.

To illustrate what I am saying, allow me to refer to a very familiar example. From the earliest ages men must have observed the heavenly bodies. The great bulk of those brilliant points with which at night the sky is spangled when clouds permit of their being seen, retain the same relative positions night after night and year after year. But a few among them are seen to change their places relatively to the rest and to one another. The fact of this change is embodied in the very name, planet, by which these bodies are designated. I shall say nothing here about the establishment of the Copernican system: I shall assume that as known and admitted. The careful observations of astronomers on the apparent places, from time to time, of these wandering bodies among the fixed stars supplied us, in the first instance, with a wide basis of isolated facts. After a vast amount of labour, Kepler at last succeeded in discovering the three famous laws which go by his name. Here, then, we have the second stage; the vast assemblage of isolated facts are co-ordinated, and embraced in a few simple laws. As yet, however, we cannot say that the idea of causation has entered in. But now Newton arises, and shows that the very same property of matter which causes an apple to fall to the earth, which causes our own bodies to press on the earth on which we stand, suffices to account for those laws which Kepler discovered—nay, more, those laws themselves are only very approximately true; and, when we consider the places of the planets, at times separated by a considerable interval, we are obliged to suppose that the elements of their orbits have slowly undergone slight changes. But the simple law of universal gravitation, combined, of course, with the laws of motion, not only leads to Kepler's laws as a very close approximation to the actual motions, but also accounts for those slight changes which have just been mentioned as

[1] Presidential Address delivered by Prof. Stok P.R.S., at the annual meeting of the Victoria Institute, on Tuesday, July 19.

necessary to make Kepler's laws fit observation exactly. We are inevitably led to regard the attraction of gravitation as the *cause* which keeps the planets in their orbits.

But it may be said, what is the difference in the two cases? Is not the law of gravitation merely a simpler mode of expressing the observed facts of the planetary motions just like the somewhat less simple laws of Kepler? What right have we to introduce the idea of causation in the one case more than in the other?

The answer to this appears to be that in the one case, that of Kepler's laws, supposing them to be true, we have merely a statement of what, on that supposition, would be a fact regarding the motions of the planets, whereas in the other case the observed motions are referred to a property of matter of the operation of which in other and perfectly different phenomena we have independent evidence.

I have purposely omitted to mention the important difference between the two cases, which lies in the circumstance that Kepler's laws require correction to make them applicable to long intervals of time, whereas the law of gravitation shows no sign of failure; because, even if the former had been perfectly exact, however long the interval of time to which they were applied, I doubt if they would have carried with them the idea of causation.

To take another simple illustration, let us think of the propulsion of a bullet in an air-gun. We speak of the motion of the bullet as being *caused* by the elasticity of the compressed air. And the idea of causation comes in because we refer this particular instance of motion to a property of gas, of the existence and operation of which we have evidence in perfectly independent phenomena.

It is thus that in scientific investigation we endeavour to ascend from observed phenomena to their proximate causes; but, when we have arrived at these, the question presents itself, can we in a similar manner regard these causes in turn as themselves the consequences of some cause stretching still further back in the chain of causation? If the motion of the bullet in an air-gun be caused by the elasticity of the compressed air, can we account for the elasticity of a gas? If the retention of the planets in their orbits be due to the attraction of gravitation, can we explain how it is that two material bodies should attract one another across the intervening space?

Till a time well on in the present century, we could only take the elasticity of gases as a fact, and deduce the consequences which flow from it. But the researches of Joule and Clausius and Maxwell and Crookes and others have accumulated so much evidence in favour of the general truth of the kinetic theory of gases, that we are now disposed not to rest in the elasticity of gases as an ultimate property beyond which we cannot go, but to regard it as itself a consequence of the molecular constitution of bodies, and of the motions and mutual collisions of the ultimate molecules of a gas. Respecting the attraction of gravitation we have not at present made a similar advance. Speculations, indeed, have not been wanting on the part of those who have endeavoured to account for it. But none of these so fits into the known phenomena of Nature as to carry with it a conviction of its truth. Yet there is one indication that though we cannot at present explain the cause of gravitation, yet it *may* be explicable by what are called second causes. The mass of a body is measured by its inertia; and, though we commonly think of a body of large mass as being heavy, and though we compare the masses of two bodies most easily and accurately through the intervention of weight, yet the idea of mass may be acquired, and means might easily be suggested by which the ratio of the masses of two bodies might be experimentally determined, without having recourse to gravitation at all. Now, according to the law of gravitation, the force with which a given body attracts another at a given distance is strictly proportional to the mass of the latter. If we suppose the attracting body to be the earth, and the attracted bodies to be in one case a brass weight, and in the other a piece of marble, it follows that if they make equilibrium when placed in the pans of a true balance—I make abstraction of the effect of the buoyancy of the air—their masses are strictly equal, and, accordingly, that weight is a true measure of mass. But there is no reason *a priori*, so far as with our present knowledge we can see, why this should be so. We know that if the bodies in the scale-pans were formed, one of brass and the other of iron, and there were a magnet concealed under the table on which the operator placed his balance, the masses would not be equal when there was equilibrium. But that the law is true, and that, accordingly, weight is a true measure of mass, follows with the highest probability from the third of Kepler's laws, and was proved experimentally by Newton, by experiments with pendulums. Newton's experiment has since been repeated by Bessel, with all the refinements of modern appliances, with the result that so far as the most exact experiments enable us to decide the law is strictly true. This is perhaps the only instance, as Sir William Thomson remarked to me in conversation, in which there is an exact agreement between two quantities, and yet we are unable to give any reason why they should agree. That such is the case, holds out some prospect of scientific men being able some day to explain gravitation itself—that is, to explain it as the result of some still higher law.

Such is the nature of our progress in scientific investigation. We collect facts; we endeavour to co-ordinate them and ascertain the laws which bind them together; we endeavour to refer these laws to their proximate causes, and to proceed step by step upwards in the chain of causation. Presently we arrive at a stage at which, even after long trial, we do not see our way to going further. Yet we are not able to demonstrate that further progress in the same direction—that is, along the chain of secondary causation—is impossible. Science conducts us to a void which she cannot fill.

It is on other grounds that we are led to believe in a Being who is the Author of nature. A conclusion so important to mankind in general is not left to be established as the result of investigations which few have the leisure and ability to carry out. Doubtless, where it is accepted, the study of science enlarges our ideas respecting the greatness of that Being, and tends to keep in check notions of too anthropomorphic a character which we might form concerning Him. Still, the subject-matter of scientific study is not, at least directly, theistic, and there have not been wanting a few instances of eminent scientists who not merely rejected Christianity, but apparently did not even believe in the being of a God.

The religious man, on the other hand, who knows little or nothing of science, is in the habit of contemplating the order of Nature not merely as the work of God, but in very great measure as his *direct* work. Of course, the concerns of everyday life present innumerable instances of the sequence of cause and effect; and few are now so ignorant of the very elements of science as not to allow that the sequence of day and night, of summer and winter, is proximately due to the rotation of the earth about its axis, and the oblique position of that axis with reference to the plane of the earth's orbit. But when we get beyond the region of what is familiarly known, still more when we get outside the limits of well-ascertained scientific conclusions, and enter a region which is still debatable ground, where men of science are attempting to push forwards, and are framing hypotheses with a view to the ultimate establishment of a theory in case those hypotheses should stand the test of thorough examination,—when, I say, we get into this region, a man such as I have supposed may feel as if the scientists who were attempting to explore it were treading on holy ground; he may mentally charge them with irreverence; perhaps he may openly speak of them in a manner which implies that he attributes to them an intention to oppose revealed religion.

To take a particular example. I can imagine that a man such as I have supposed may have always been in the habit of regarding each one of the thousands and tens of thousands of species into which naturalists have divided the animal and vegetable kingdoms as having originated in an independent creative act; that the supposition may have become entwined among his religious beliefs. Such a man would be apprehensive of any attempt to introduce second causes in explanation of the observed fact of the great multiplicity of species.

Akin to the feeling which I have attempted to describe is another, against which we must be on our guard. The religious man is strongly impressed with the truth of certain things which lie outside the discoveries of reason or the investigations of science, and which bear on the whole conduct of his life here, and on his hopes regarding a life hereafter. He believes these truths to be divine, and, accordingly, that no legitimate deduction of human reason is liable to come in conflict with them. But the precise mode in which a conviction of the truth of these things was arrived at depends, to a considerable extent, on each man's idiosyncrasy. His natural bent of mind, his early training, his later associations, have all a good deal to do with it. Divine truth is one thing; our own apprehension of it, and the steps by which in our own minds it has been arrived at, are another. These are liable to human imperfection, and we may not attribute to them the infallibility which belongs to that which is divine. We are not to confound the scaffolding with the building; nor, if we are anxious for the safety of the edifice, need we therefore fear that, if the scaffolding were tampered with, the whole might come tumbling down, nor should we regard as a dynamiter a fellow-workman who would remove a pole or two.

That truth must be self-consistent, come from where it may, is an axiom which nobody would dispute; the only question can be, What is truth? Now, there are truths which we know by intuition, such as the axioms of mathematics; and there are others, again, which, though we do not perceive them by intuition, yet demonstrably follow from what we do so perceive; such, for example, are the propositions of mathematics. Then there are other conclusions which we accept as the result of the application of our reason to a study of Nature. Here the evidence is not demonstrative, and the conclusion may have all degrees of support, from such overwhelming evidence as that on which we accept universal gravitation, to what hardly raises the conclusion above the rank of a conjecture. On the other hand, there are conclusions which we accept on totally different grounds; namely, because we think that they have been revealed. Why we accept a revelation at all, is a very wide question which I cannot here enter into. That we do accept it is implied in the membership of this Institute. But, granting the acceptance of revelation, the question remains, What and how much is involved in revelation? That is a question respecting which there are differences of opinion among those who frankly accept a revelation, and with it the supernatural.

Now, the primary object of the establishment of the Victoria Institute was to examine the questions as to which there was a *primâ facie* appearance of conflict between the conclusions of science and the teachings of revelation. In order that such examination may be usefully carried out, it must be undertaken in a thoroughly impartial spirit, with a readiness honestly to follow truth wherever it may lead. It will not do to assume that the immunity from error which belongs to the divine belongs also to our apprehension of what constitutes the divine, and that therefore, if a conflict there be, the error must be on the side of science. It is true, that many statements which are really little more than scientific conjectures are represented, at least by those who take their science at second or third hand, as if they were the well-established conclusions of science. But it is true also that the progress of science has corrected the assertions of a crude theology. We are disposed nowadays to smile at the idea of any opposition between the Copernican system and the teaching of revelation; but we need not go back to the days of the persecution of Galileo to find an example of a well-supported scientific conclusion having met with a similar opposition, issuing in a similar result.

To gauge thoroughly the amount of evidence on which an asserted scientific conclusion rests, one ought to be well acquainted with the branch of science to which it relates. Still one can get a fair general notion of the evidence by an amount of reading which is by no means prohibitive, or by conversing with those who have made that branch a special study. It may be that the impression thus left on the mind will be that the votaries of science, carried away by an excess of zeal in the attempt to discover the causes of natural phenomena, have really, though honestly, overestimated the evidence. It may be, on the other hand, that the inquirer will perceive the evidence to be weighty and substantial, in which case it behoves him to reconsider the supposition with which he started, that the conclusion was opposed to the teaching of revelation.

One should always bear in mind the great responsibility one incurs, and the mischief one may do, by representing as bound up with revelation that which really forms no part of it. Being by hypothesis no part of it, but only erroneously tacked on to it, it may be false, and being false, it may be in opposition to a conclusion supported by the weightiest evidence, it matters not of what kind, but say scientific. What, then, will be the effect of the error committed by the upholder of revelation? The educated man of science may see through the fallacy; but will it not put a weapon into the hands of the infidel lecturer wherewith to attack revealed religion?

But whether we can agree or cannot agree with the conclusions at which the scientific investigator may have arrived, let us, above all things, beware of imputing evil motives to him; of charging him with adopting his conclusions for the purpose of opposing what is revealed. Scientific investigation is eminently truthful. The investigator may be wrong, but it does not follow that he is other than truth-loving. If on some subjects which we deem of the highest importance he does not agree with us—and yet it may be he agrees with us more than we suppose—let us, remembering our own imperfections, both of understanding and of practice, bear in mind that caution of the Apostle: "Who art thou that judgest another man's servant? To his own master he standeth or falleth."

SCIENTIFIC SERIALS.

Rendiconti del Reale Istituto Lombardo, June 16.—On the importance of the qualitative bacteriological examination of potable waters, by Prof. Leopoldo Maggi. Attention is directed to the mistake made by many chemists, who occupy themselves exclusively with the *quantitative* examination of potable waters, neglecting the much more important question of the specific quality of the germs, owing to the greater difficulty of distinguishing between the various forms of these organisms. Waters largely charged with harmless Bacteria are condemned, although perfectly drinkable, while others apparently pure, but really containing deadly germs in small quantity, are declared to be quite safe, often to the great danger of the public health. It is in fact far more a question of *quality* than of *quantity*, as shown especially by the recent researches of Chantemesse and Vidal on the Bacillus of typhus. On the other hand, Leone has experimentally shown that comparatively pure water is itself a medium of culture, so that a small quantity of innocuous Bacteria may largely increase in it without rendering its use dangerous. Some instructions are added for distinguishing between harmless organisms normally present in water as their natural element, and pathological germs, which render it quite unfit for human consumption.—Meteorological observations made at the Brera Observatory, Milan, during the month of May.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, June 16.—Mr. William Crookes, F.R.S., President, in the chair.—The following papers were read:—A study of the thermal properties of a mixture of ethyl alcohol and ethyl oxide, by Dr. William Ramsay and Dr. Sydney Young.—Derivatives of hydrindonaphthene and tetrahydronaphthalene, by Dr. W. H. Perkin, Jun.—The synthetical formation of closed carbon chains in the aromatic series, by Dr. F. S. Kipping.—The product of the action of ethylene bromide on ethylic acetosodacetate, by Dr. P. C. Freer and Dr. W. H. Perkin, Jun.—The synthesis of hexamethylene-derivatives, by Dr. P. C. Freer and Dr. W. H. Perkin, Jun.—An attempt to synthetize heptamethylene-derivatives, by Dr. P. C. Freer and Dr. W. H. Perkin, Jun.—The composition of shale-spirit, by Dr. A. K. Miller and Mr. T. Baker.—The magnetic rotatory power of the ethyl salts of maleic and citraconic acids and their isomers, by Dr. W. H. Perkin, F.R.S.—The temperatures at which various sulphates undergo decomposition, by Dr. G. H. Bailey.—The reaction between sulphites and nitrites of metals other than potassium, by Dr. Edward Divers, F.R.S., and Mr. Tamemasa Haga.—The action of acetyl chloride on acetoximes, by Mr. Victor Meyer and Mr. A. Warrington.—Sulphinic compounds of carbamide and thiocarbamide, by Mr. George McGowan.—Anarcardic acid, by Dr. S. Ruhemann and Mr. S. Skinner.

EDINBURGH.

Royal Society, July 4.—Mr. J. Murray, Vice-President, in the chair.—Prof. Tait communicated a paper by Mr. A. C. Mitchell on the thermal conductivity of iron, copper, and German silver. Mr. Mitchell made his experiments upon the same bars as were used by Prof. Forbes and Prof. Tait, but the surfaces were nickelized so as to prevent oxidation. The results agree well with those of Prof. Tait, and are probably as correct as the method admits of.—Mr. T. B. Sprague read a paper on the probability that a marriage, entered into by a man of any age, will be fruitful.—Dr. A. B. Griffiths read a paper on the nephridia of *Hirudo medicinalis*, and communicated a paper by Mrs. Griffiths on degenerated specimens of *Tulipa sylvestris*.—Mr. J. T. Cunningham and Mr. Rupert Vallentin described the photosphæria of *Nyctiphanes norvegica*, Sars.—Mr. C. J. Burton read a paper on a Daniell cell for use as a standard of electromotive force.—Prof. Tait read a paper on glories. He showed that the observations made upon glories on Ben Nevis make it certain that Young's explanation of these phenomena (colours of thin plates) is not adequate. He considers that they are produced by diffraction of light reflected from the drops of water.—Mr. J. Murray submitted a report by Prof. Milnes Marshall and Mr. G. H. Fowler on the *Pennatulidæ* dredged by H.M.S. *Porcupine*.

PARIS.

Academy of Sciences, July 25.—M. Janssen in the chair.—Note on M. Gosselin's scientific labours, by M. A. Richet. This memoir on the life and work of the distinguished anatomist and pathologist, who died at the end of last April, is intended to supply the place of the customary obituary notice, M. Gosselin having expressed a desire that no discourse should be pronounced in connexion with his funeral obsequies.—Obituary notice of M. Alfred Terquem, Corresponding Member of the Section for Physics, by M. Mascart. A rapid sketch is given of the brilliant career of this physicist, who was born at Metz on January 31, 1831, and died on July 16, 1887. His numerous scientific publications deal mainly with acoustics, capillary phenomena, and heat. He is the author of an important treatise on "Roman Science in the Age of Augustus," and of a more comprehensive work on the history of physical sciences from the earliest times down to Galileo.—Note on the earthquake of February 23 at Nice, by M. Bouquet de la Grye. The diagram of the curve of the maregraph here figured as taken at the time of the seismic disturbance presents some points of considerable interest. It clearly indicates a rapid upheaval of the ground, followed by a slow subsidence, the sea returning to its normal level in about two hours after the first shock. The maximum of upheaval at Nice was 55 mm., which can scarcely have exceeded the natural elasticity of the earth's crust.—On the meteorite which fell at Jati-Pengilon, Java, on March 19, 1884, by M. Daubrée. The analysis of this meteorite, which weighed 166 kilogrammes, shows bronzite 39, olivine 33·4, iron with nickel and traces of cobalt 21·3, troïlite (sulphur of iron) 5·1, chromite, 0·1; mean density 3·747. The breakage presents some exceptional features, being especially remarkable for the myriads of minute cleavage facets with a sparkling brightness like that of mica. In its general appearance it may be compared to certain very fine-grained feldspar rocks, such as leptynite, and it evidently belongs to the extremely rare category represented by the meteorites of Ensisheim (1492), Erxleben (1812), Cabarras, North Carolina (1849), Morbihan (1869), and one or two others.—Fluorescences of manganese and bismuth: general remarks and conclusions, by M. Lecoq de Boisbaudran. In concluding these protracted studies the author shows in a general way that the observations made with manganese and bismuth are also applicable to other fluorescences. He also concludes that two substances more or less active on a solvent may at times neutralize each other, reducing the two fluorescences to *nil*. A similar result has been obtained by Mr. Crookes with the rare earths.—Solar observations made at Rome during the first quarter of the present year, by M. Tacchini. In supplement to his communication of April 18, the author shows that the faculæ as well as the protuberances were most frequent in the northern solar hemisphere. The maximum of faculæ corresponds to the equivalent zone $\pm 10°$; the solar spots were confined to $\pm 20°$, while the protuberances reached $\pm 80°$.—Solar observations made at Rome during the second quarter of the present year, by M. Tacchini. During this period there was a perceptible increase of all the solar phenomena, and some metallic eruptions were also recorded.—On the determination of the coefficient of elasticity of steel, by M. E. Mercadier. In a recent communication the author proved that in the relation $\frac{\lambda}{\mu}$ of the constants of elasticity λ is very nearly $= \mu$ for glass. Here he shows that for cast steel $\lambda = 2\mu$.—Danger of infection from tuberculous substances, by M. Galtier. The experiments here described fully confirm previous conclusions regarding the great resisting power of the virus of tuberculosis. It retains its activity after being subjected to temperatures ranging from 71° C. to 7° or 8° below freezing-point. It also resists the action of water and the desiccating process, as well as strong pickle, so that the consumption of fresh or corned beef from animals affected by pulmonary diseases is always attended with some danger.—On *Colochirus lacazii*, by M. Edgard Herouard. A full description is given of this new species of the genus Colochirus of the Holothurian family, found by the author in the neighbourhood of Roscoff, and by him named *C. lacazii*, in honour of M. Lacaze-Duthiers.—Contribution to the study of the evolution of the fresh-water Peridiniums, by M. J. Danysz. From his researches on the development of these organisms, as well as of the distantly-allied genera Gymnodinium and Glenodinium, the author concludes that they should be regarded rather as plants than animals. A close study of their successive phases of development, and of the nature of their substance, shows that they are true members of the vegetable kingdom.—Appearance of black rot in the neighbourhood of Agen, by M. Prillieux. An examination of some diseased grapes from this district shows clearly that they have been attacked by black rot which had already made its appearance in the Upper Hérault Valley two years ago, but which it was hoped would die out or spread no farther.—A sealed paper deposited by M. A. Leduc on May 9, 1887, and now opened at his request, describes two experiments showing that the calorific conductibility of bismuth is considerably reduced when this metal is placed in a magnetic field.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Hand-book to Government Situations: B. D. K. (Stanford).—The Conic Sections: G. Heppel (Baillière, Tindall, and Cox).—Formal Logic, Second Edition: J. N. Keynes (Macmillan).—Psychology; The Motive Powers: J. McCosh (Macmillan).—Romantic Love and Personal Beauty: H. T. Finck (Macmillan).—Crown Forests at the Cape of Good Hope: J. C. Brown (Oliver and Boyd).

CONTENTS.

PAGE

THURSDAY, AUGUST 11, 1887.

THE TOPOGRAPHY OF GALLOWAY.

Studies in the Topography of Galloway; being a List of nearly 4000 Names of Places with Remarks on their Origin and Meaning. **By Sir Herbert Eustace Maxwell, Bart., M.P. (Edinburgh: Douglas, 1887.)**

SIR HERBERT MAXWELL will strike a sympathetic chord in the minds of many readers, who have not themselves time to search for the origin of place-names over which they have pondered, and perhaps speculated, without avail. We do not mean that the limited district so thoroughly sifted by Sir Herbert Maxwell affords illustrations for place-names everywhere, but his method of handling the subject serves as a model for the useful imitation of students in other districts where such a convenient hand-list is wanting.

On a clear day, one ascending the backbone of England, say at Cross Fell, beholds, beyond the Vale of Eden, far to the south, Ingleborough and the Shap Fells, on the west the Lake mountains, and towards the north a broad arm of the sea, which he recognizes as the Solway Firth, cutting off the wide-extended plain of the Vale of Eden, which lies spread like a carpet far below him. Beyond the Solway Firth there rises a huge hill capped with cloud and backed by hilly country, cut off by the sinuous coast-line as far as the eye can reach. The hill is Criffel, "a hill of 1850 feet," called on a map in the Bodleian Library, circ. 1330, "Mons Crefel," and by Pont, in Blaeu's Atlas, 1654, "Crafel," a hill whose peculiar granite boulders lie scattered plentifully in the drift over the new red sandstone of the Vale of Eden. It is one of the outposts of Galloway, the origin and meaning of whose place-names form the subject of a most thorough and searching investigation in the present work. These names are conveniently arranged in dictionary form in 322 pages. Many are entered and left unexplained; Sir Herbert Maxwell, with true statesmanship, leaving to others the invidious task of applying the unscientific knot-cutting, or "guessing etymology," which he so scornfully repudiates in the, if anything, somewhat prolix introduction of 44 pages.

We in England, who do not all know our *Bobelloth* and *Bethluisnion* as well as we might, come as learners to Sir Herbert Maxwell's book, which displays much real learning and a fair amount of bibliographical research. Treating, as it does, of a language which is foreign to our ears, a language rejoicing not only in "eclipses," or a vast superfluity of unsounded consonants, but of "triphthongs" or sequences of three vowels, equally unknown to our modern English, Sir Herbert Maxwell's task lies very much in expanding to the full Gaelic form the words from which the vast majority of the names are derived, and at first sight it seems almost as hopeless a task to follow him as to sit down unassisted to master Russian. We can only make one or two observations on the introduction, which digests much from O'Donovan and Joyce. "The Basque word for water is *ur*," hence "the rivers called Oure, Urr, Ure," &c. Like most other words meaning a river, it also means a "bank" of a river: *e.g.* beck; burn (bruinne, a *brink*); river (*ripa*), &c. "*Ur* denotat rivos aquarum impetuose ex alto delabentium" (Junius, "Alph. Run.," 21); cf. Lat. ora, A.S. ore, Eng. ore, the *shore*. In Norway, *ur* is the rough slope of a mountain; Irish, *ur*, a border, *brink*. The author, "dismissing as unattainable" all record of pre-Celtic speech, finds, "from the evidence of these names," that the Pictish of Galloway "belonged to the Goidhelic or Gaelic rather than to the Brythonic or Welsh branch." "No doubt," he adds, "there are names whose forms would bear being assigned to a Brythonic origin, but with these I have not ventured to deal." We will not venture either, but in such glaring cases as the "Rhinns" of Galloway; "maiden craigs" (W. Meiddyn, a cliff, precipice), common in North England; "cors," the fenny district on the coast of Kirkbean (Chalm. "Caled." iii. 234); "carse," Kirkcudbright, "carse gowan," "carse thorn," "carse land," in all of which the physical character answers to the Welsh "cors," "a *marsh*, according to the common acceptation" (Ed. Luid, "Adversaria," p. 268); and in "Corsock," New Abbey, Wel. "corsawg," *fenny* (Chalm. "Caled."), and several others, it is plain that Welsh words do occur, and therefore have to be dealt with. With the principles admirably set forth in the introduction we fully agree. One or two slips occur, as (p. 41) where the author attributes to Pont, as original, a passage copied from Camden's "Britannia." Sir H. Maxwell seems to have followed Murray's error (Note D, Append. to "Hist. of Gall.," 1822). The sentence is "Neirunto this (Vigtoune) Ptolemee placed the city Leucophibia," &c. Now, this sentence appeared in the original Latin edition of Camden, 1586, p. 480, "Galloway . . . Hac regione Leucopibiam urbem statuit Polemeus," &c., published when Pont was about nineteen; and for comparison with the passage from Pont's manuscript we give that from the first English translation o Camden, 1610:—"Neere unto this Ptolomee placed the city Leucopibia, which I know not to say truth where to seeke. Yet the place requireth that it should bee that episcopall seat of Ninian which Bede calleth *Candida Casa*, and the English and Scotish in the very same sense *Whitherne*: What say you then if Ptolomee after his maner ("suo more," 1586) translated that name in Greek Λεὺ οικιδια [*sic*], that is Whitehouses," &c.

Again, the supposed identification of *Rerigonium* with *Bargeny*, attributed by our author (p. 42) to Heylin, 1669, should be attributed to Camden. Thus, under *Carricta*, Camden, 1610, has Rerigonium, "a towne. For which *Berigonium* is read in a very ancient copie of Ptolomee printed at Rome in the year 1480, so that we cannot but verily think it was that which is now called *Bargeney*." Sir Herbert Maxwell rightly points out the anachronism between Loukopibia and Candida Casa. Horsley—with several others who have discussed the Ptolemaic names—avoids the trap, saying, with a side glance at Camden, "others from a fancied etymology place it at Whithern," which error the followers of Camden have perpetuated to our own day. The form "Lucotion" ("Brit. Chorog."), is conclusive. "Brigomono," however, in which form *Rerigonium* appears in "Brit. Chorog." (given in Gale, and Horsley, "Rom. Brit." p. 490), does offer some suggestion of Bargeny.

Of the forty-seven authorities given by the author, sixteen are works exclusively relating to Ireland. We suggest that many of the "Gallowaie" place-names are well illustrated or explained in the following works not consulted by him, viz.:—

(1) Hector Boethius, c. iii. "The Description of Gallowaie (in Holinshed, 1587, p. 9), "Aboue Nidderdale is Gallowaie," &c. Thus "the two other lakes, the *Salset* and the *Neutramen*, of equall length and bredth with the *Loch Mirton*," do not appear in Sir Herbert Maxwell's list. Of the Mull of Galloway, Boethius writes, "which the Scots call a *mule* or *nuke*. . . . The common sort name it the *mules nuke*," an evident reduplication, "newk," which occurs several times in Maxwell's list, being Old Norsk *Hnjúkr*, common in North England for a projecting hill. The forms for "the two great lakes *Rean* and *Lois*" (Ryan and Luce) are also worth entering.

(2) Chr. Irvine, "Historiæ Scoticæ Nomenclatura Latino-vernacula, multis flosculis, &c.," "enriched with many select phrases from the ancient monuments of the Scots and the aboriginal language of the Gael," Edin., 1682. Here (p. 84), "Gallovidia et Wallowithia (for it is so named by the Welshmen)" offers a form not found in Maxwell *s.v.* Galloway, and with many other forms we have before us is well worth entering. In the article (p. 186) on the word Galloway, Sir H. Maxwell, passing by Lloyd's etymology ("Church Government," appended to Stillingfleet, "Orig. Brit.," vol. ii. p. 72), cites Skene for the "stranger-Gael," but has he not observed how Prof. Rhys shakes his head at this ("Celtic Btn.," 1882, p. 153)? The form Galwychya, in "Bulla Innocenti V. *De Holmcolt.*," 1207, is worth recording, but the complete list of forms, which is a very long one, should be given.

(3) Sibbald, "Hist. Animalium in Scotiâ," 1684, Part 2, cap. iv., may be cited under Sir H. Maxwell's "Fumart Liggat" (p. 184), "*Foina* est Boethii." "Nostra arborea est. Sylvas incolat abiegnas. Nidumque super Abietes, sciurorum instar struit." The pine marten inhabits pine-woods and, squirrel-like, builds its nest in the fir-trees.

(4) W. Baxter, "Glossarium Antiq. Brit.," Lond. 1719, deals with the Ptolemaic Galloway names; but much more welcome are the "Adversaria Posthuma" of the learned Ed. Lhuyd, given as an appendix to that work. The title is "D. Edvardi Luidi de Fluvm., Montm., Urbm., &c., in Britanniâ nominibus Ad. Posth." Sir H. Maxwell has "Fínen hill" (p. 182); Luid (p. 268), "Fynnon though generally used for a well [*i.e.* spring], signified also the first or highest lakes of the great mountains.' In Luid's "Adversaria" is a mine of wealth, from which we select two names, (p. 274) "Turch, *porcus*," (p. 267) Türch, a *hog*, in Brecknockshire. O'Reilly (p. 542) gives Irish, Turc, and (p. 528) Torc, Wel. Torch, a *hog*, *swine*. Sir H. Maxwell has "Turkey Hill" unexplained (p. 306), which is translated by "Swinefell, the fell or hill of the swine" (p. 299). Again, "Hespin" (p. 200) in Whithorn is left unexplained. Luid (p. 267) has, Wel. "Hespin, a sheep that yields no milk. There are two or three *brooks* of this name about Ystrad Vehlte, in Brecknockshire, so called because their channels consisting of limestone have great caverns which in summer-time take up all the water the springs afford, so that, the channels being left dry, the brooks are called *Hespin*."

(5) How could Sir H. Maxwell overlook Kirk's list of over 400 Gaelic words in Append. II. p. 99, to Bp. Nicholson's "Scottish Hist. Liby.," Lond., editn. 1776, "A vocabulary of the Irish dialect spoken by the Highlanders of Scotland, collected by Mr. Kirk," with a few words added by Ed. Lhuyd? They are in twelve chapters, several of which relate to Nature and her productions, and help to form place-names.

(6) Horsley, "Brit. Rom.," Lond., 1732, cited.

(7) "Etymology of the Names of Places in Ireland," "by a gentleman well versed in the language and antiquities of that country," contributed by "C. L." to *Ant. Repertory*, vol. iv. 1809, gives a list of eighteen words used in place-names.

(8) Thos. Murray, "Lit. Hist. of Galloway," Edin., 1822, Append., Note D. He has also a fairly full article on the forms of the name "Galloway" in Note A, the oldest of which, in a charter of Earl David, A.D. 1124, is Galwegia [? Galweyia].

(9) W. Mackenzie, "Hist. of Gall.," Kirkcudbright, 1841, has (pp. 12–22) several place-names taken from Chalmers's "Caledonia," without acknowledgment.

(10) M. M. Harper's beautiful "Rambles in Galloway," with illustrations by Faed, &c., Edin. 1876, smooths away the difficulties that lie in the path of the name-hunter, and his terse descriptions of sites are a model, *e.g.* (p. 99) "*Rusco* Castle, beautifully situated on a rising knoll in the Vale of Fleet, near the margin of the river." That is what we want to know. But "Rusco" can only be assumed to be here, as it is known to be in Nidderdale, after a man's name. Throughout Sir H. Maxwell's list, terse physical descriptions are much missed. Again, facing p. 109, Harper gives a view of "Kirkclaugh shore" that goes a long way to convince one that "Kirkstone" is meant. He gives etymological notes on place-names, no doubt from Chalmers, *e.g.* "Minnigaff," p. 133, differently explained by Maxwell (p. 254) and by Symson. Who shall then decide? On p. 151 several names. But his most important hint is in the admirable picture of the "Cow Clout Stone" (p. 187), which in the "Stat. Acct." is described by Crosbie as "a flat stone about 3 feet in diameter," in which are the marks of what might be supposed a *cow's foot*, a horse-shoe and the four nails on each side being very distinct, &c. Were cows formerly shod in Galloway as oxen are near Naples? Sir H. Maxwell has "Cowloot" (p. 128) unexplained. O'Reilly gives—Irish, *luat*, *the foot*, ("Ir. Eng. Dic.," p. 337), which explains the name.

(11) Chalmers's "Caledonia" requires mention apart, for in that work many, if not most, of the names treated in Sir H. Maxwell's painstaking work have been considered, generally with satisfaction to the reader, as will be found in vols. i. and iii., where he will be found to have anticipated some of Sir H. Maxwell's explanations, as "Loch Brack," "Loch Breac, in the Scoto-Irish the *lake of trouts*."

(12) Prof. Rhys, "Celtic Britain," 1882, a good antidote to Skene for those who may require it, has several ancient Galloway names.

(13) Dugdale, "Monasticon," gives a host of charters with the old forms of Galloway place-names. No less than twelve of the eighty-seven charters relating to the Abbey of Holmcoltram, whence the monks had a pleasant row

across Solway to their lands in Galloway, contain forms, too numerous to be enumerated here, of Kirkewinni, Kirkwyni, Kyrkewynwi, Kirkewenny, Kirkgunny, Kirkwynnyn, &c., &c., now Kirkgunzeon. The same St. Winnin probably explains Kirkennan, Kilennan, the ancient name of Buittle (Chalm. iii. c. iii.), and Kirvennie. Here also the River *Urr* is spelt "Hur"—"a portu *Hur* usque ad 'Pouesterbened'" (No. xxiv.), or "Pollesterheved" (No. lxxxii.); perhaps the same "Pow, a *slow-moving rivulet* in flat lands" (Jamieson), recorded by Sir Herbert Maxwell at New Abbey, which last was formerly called "Loch Kendelock" or "Loch Kenderloch" (Chalm. iii. 296, 305), "Loghendello" in a charter of Roland, son of Huctred (No. lxxxi., Holmcoltram, Dugd.), and now "Loch Kinder," after Cendaelaidh, King of the Picts, D. 580, q.v. We find also "Lotchitdale," "Bulla Lucii III." (No. xxi. Holmc., Dugd.), and "Lochent" (No. lxxxi. fol. 103); "Millebronna," "Bulla Alexandri III." (No. xxiv.), which, unless the same as "Millbawn," is wanting in our author's list. So also "Salternes," *i.e.* salthouses (*ib.*); "Polben," probably our author's Polbae (*ib.*); and "Sivchaye," now Southwick, according to Chalmers, iii. 296; "Glenlus," now Glenluce (No. lxxxii.); "Mayby" (*ib.*), now Mabie, in Troqueer, and "Achencork" (*ib.*); "Pollackercin" (No. lxxxi.); "Polleychos," or "Polthos" (No. lxxxii.), some stream, and "Genesik," *i.e. sandy*, "sándy-sike," or gutter (A.S. Sik), for which see "Genoch" and "Gannock" in our author's list, the last form occuring several times in Middlesex and Hertfordshire. Then we find "Mustard Garth," evidence of the Northman, in "Kirkoneville," or the manor of Kirkconnel (*ib.* fol. 112) west of the estuary of the river "Nud," now "Nith," formerly "Nid," the Novios of Ptolemy, a river-name of pre-Celtic origin, ranging from Wales to Trondhjem. "Trenguer" (lxxxiii.) preserves an *n* dropped in the modern Troqueer. "Botil" (*ib.*), A.S. Botl, perhaps "Buittle" (?). The charter of William the Lion also preserves the names Kirche Cormack (Maxwell, "Kilcormack"); S. Andreæ; "Balincros" (not in Maxwell), and "Cheletun," now Kelton.

We will now take at random a few names from Sir Herbert Maxwell's list. We cannot concede that the northern adjectival termination *et* represents A.S. wudu, as claimed by our author (Introd., p. 9), in "Aiket," &c. "Thornit," *thorny*, also occurs in Anglian North England. Are we to understand that the stream which "was rolling along its wild and turbid waters with a *freshet* upon it" was rolling along a fresh *wood?* "Bail Fell," Old Norsk Bali, "monticule," a *grassy bank*, cf. "Bale bank," Nidderdale, so also, "Bailie Hill," but see a learned article, *s.v.* Baillie, Baille, and *s.v.* Bel, Boel, *cour intérieure*, in Duméril, "Patois Normand," 1849, which clears away the fog from "Baillie, meaning doubtful," of Jamieson. "Barean," "Barend," and "Borron," unexplained by our author, a well-known word in North England, a *rocky slope*, or *hill*, where foxes and badgers burrow. It ranges at least as far south as Kettlewell, where it appears as "Borrance," the stony screes below the limestone girdles or cliffs. It is also called "Burran," and, among the Yetholm gipsies, "Burran" means a badger. O'Reilly ("Ir. Eng. Dic.") has:—"Barran, the *tops of mountains;*" "Boireand [Barend], a large rock; a *stony, rocky district.* Is the name of several rocky districts in the north and south of Ireland. It is applied to the face of a desolate mountain in Achil," &c. Similarly used in North England. His last form "Boireand, *Borron*, a large rock," identifies the word completely. Duméril, *s.v.* "Buret," *porcherie*, records low Latin "*Burum* le *Bure* vieil anglais et le vieux français *Buron*," &c. "Bine Hill," O'Reilly has "Binn, a *hill*," which may also explain "Byng Hill," though Old Norsk "Bingr" looks tempting. However, it is not found directly applied to hills as hills. "Caughie Stone" reminds us of numerous "Cockle Hills" in North England, all on moorlands. "Hecla," the *cloak*, and "Cloak Hill" (p. 123), suggests Ir. *Cocal*, a *cloak*, which is intelligible if the cloak be *peat*, as in "Caugh Moss," Girthon. Old Norsk forms are scattered through our author's list, *e.g.* Cawvis Hill, *if* Old Norsk Kálfr, a *calf* (?), as Sir Herbert Maxwell suggests.

"Cockplay, a hill of 950 ft." (p. 124), seems to be a translation of "Cocklakes," or properly "Cocklaiks," *dry ridges on the moors*, which has been explained ("Stud. in Nidderdale") as "the *playing-ground* of the grouse or moorcock." The keepers and others familiar with their habits understand this. "Cokelayk" is often mentioned in the early charters of Holmcoltram Abbey, Cumberland, given in Dugdale. The prefix "Darn" in "Darnarbel," "Darncree," &c., reminds us of "Darnetal," a place-name at Caen. "Darne" in "Patois Normand," and "Darn" in Breton, means a *piece, portion* (Duméril). "Dub," a pool, common in North England, and in our author's list "Dub of Hass," "Duchdubs," is evidently Celtic, as it occurs as "Douve, grand fossé plein d'eau, étang," in "Patois Normand." The necessity for a brief physical description is well illustrated by the name "Knockmullin." Our author observes that the same words serve for eminences and hollows, hills and valleys, and the fact is well known. All his "Knocks" are hills, and he does not say what kind of thing "Knockmullin" is. The second half is clear—"mill," and the word might mean *mill-race*, for we have, "Patois Normand," "*Noc.* Dalle, goutiére en bois, canal qui apporte l'eau sur la roue d'un moulin." "Laicht, on the eastern shore of Loch Ryan" (Skene, "Chron. Picts and Scots." Pref. clxxv. 1867), is omitted in Maxwell's list, though we believe there is a farm of the name there yet. "Rerrick, anciently called *Dundrainan*, the hill of thorns" (Chalm. "Caled." iii. 313) renders the spelling "Rerwick" very questionable. Is there not an initial *d* dropped? "Wigg," certainly the "Wigstones" of the Nidderdale moors, a huge projecting rock, or, rather, pile of rocks, records the Gaelic "Wig," a *rock*. The old forms given by Sir Herbert Maxwell divorce the Whithorn "Wigg" from the A.S. "Wic," to which he would assign it.

We conclude an imperfect review of this important work with a question to which our personal knowledge of Runic inscriptions on British soil suggests that there is no satisfactory answer. Why send the two Runic inscriptions (Introd. p. 18) to Denmark? No Danish scholar has ever deciphered an English Runic inscription correctly from the days of Spelman and Ole Worm to July 21, 1887. We have read our own A.S. Runes from the *Futhorc* Otho B. 10 in Hickes's "Thesaurus," by the true learning of *Kemble;* and, moreover, no known Runic inscription on British soil corresponds to the Scandinavian formula by which, "from analogy," Prof. Stephens would read those found, but unfortunately not given, by Sir Herbert Maxwell. JOSEPH LUCAS.

OUR BOOK SHELF.

The Prevention of Consumption. By C. Candler, Melbourne, Victoria. (London: Kegan Paul and Co., 1887.)

THIS may be considered a book of theories. The author promises by "his theory" to revolutionize the treatment of phthisis, and almost to bring the disease to an end among civilized nations.

The theory is briefly stated thus: "Ordinary phthisis is invariably caused by a local bacillary malaria governed by chemical light." When the author speaks of bacillary malaria, he means that the tubercle Bacillus is like a saprophyte, capable of growing and thriving in the soil, and that from the soil, which is its true birthplace and home, the Bacillus or its spores find entrance into the human system: fortunately for humanity solar light destroys many of these Bacilli.

"It will be observed," says the author, on page 191, "that it is presupposed that the consumptive, and they who are sickening with consumption, are, or have recently been, exposed to a bacillary malaria fostered by an insufficiency of solar radiation, and this is one of the inferences which urgently requires to be verified." Quite so; and this the author ought to have done himself, though he hopes that somebody else will furnish the proof.

The prevention of phthisis the author has no doubt of achieving by plenty of sunlight; and he would force the Governments to supply more sunlight to the inhabitants of big cities, where, as is known, consumption is rife. It is a pity the author does not tell us how this is to be achieved in London or Manchester during a great part of the year. E. KLEIN.

Metal Plate Work: Its Patterns and their Geometry. By C. T. Millis. (London: E. and F. Spon, 1887.)

THIS work is one of the series of Finsbury Technical Manuals, and teaches how all ordinary patterns required by sheet metal-workers can be set out on one geometric principle. It is the first work in which the setting out of such patterns has been systematized. The manufacture of every article in common use is treated as a separate problem, but the principle in all cases is that the parts composing it shall be set out mathematically, so that any worker having become accustomed to cut out his work on this principle could equally apply it to new forms. The first chapters are of the most elementary character, so that the work is not necessarily above the head of ordinary mechanics. That the book is an admirable manual there can be no question, but whether such a book will be widely consulted appears doubtful. In the opinion of two of the chief tinplate workers in Birmingham the knowledge it imparts will save time and prevent waste of material, which results when the rule of thumb and guess-work are in vogue, whilst the workman using it will gain confidence, and his value be increased by the certainty of his pattern working out true. Nevertheless, the great mass of workmen in metals are not yet educated up to the use of such a work, and in all probability in a centre like Birmingham it will only fall into the hands of managers of manufacturing establishments and a limited number of first-class workmen. It is a book, however, that must be required by the artisan more and more to meet the rapid strides of education, and it will, we hope, command a satisfactory sale.

Walks in the Ardennes. Edited by Percy Lindley. (London: W. H. Smith and Son, 1887.)

THIS hand-book, which only costs sixpence, contains all the information the ordinary tourist is likely to want in walking in the Ardennes. The writer is very familiar with the country, and describes clearly and simply the various routes and the chief centres of interest. There are a sketch map of the Ardennes, and a good many illustrations.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Parietal Eye in Fishes.

IN my short paper on this subject which appeared in NATURE of July 14 (p. 246) there are one or two points which need better elucidation than I then gave them.

In the first place, for the sake of brevity, my reference to Ahlborn's valuable paper is too scanty, and I am unwilling to do any injustice to that excellent observer. I did not believe he had ever really been fortunate enough to get sections of the "pineal gland" of a fully adult *Petromyzon planeri*; for, judging from what I had found in every adult examined, I imagined that had he possessed fully adult Petromyzon he must have noticed the black pigment in the parietal eye, and moreover must also have seen and figured the deep fossa in the skull (*vide* my figure in NATURE, p. 247), in which, in the adult, the parietal eye rests. It appeared as though his descriptions and figures of the adult brain had been taken from specimens in which the metamorphosis was not quite complete. I have again studied his figures, and must admit that in other respects some of his drawings represent the brain of adult Petromyzon. The apparent contradiction seemed strange, but it is fortunately not inexplicable.

I must here mention that *Petromyzon planeri* is no longer here in Freiburg so plentiful as when Calberla worked on it, now more than ten years ago. Indeed, I have had great difficulty in obtaining adult and very young specimens. The older Ammocœtes, though not common, are not so rare.

This being the case, I could not examine the number of individuals I should have otherwise wished to do. However, I have now found one adult Petromyzon in which there was no black pigment in the parietal eye and no fossa for the eye in the skull. That the specimen was otherwise adult is certain. This find accounts for the non-discovery by other observers of the black pigment I have described. The parietal eye in Petromyzon, which is a rudimentary organ, like many other rudimentary organs is probably also variable in different individuals, and it is not impossible that the black pigment of the parietal eye is entirely absent in the Petromyzon found in many places.

So far as I can judge at present—and I intend to further examine the point—the parietal eye in the Blindworm (*Anguis fragilis*) is also variable. It certainly varies in size and in distinctness.

The second point relates to the black pigment. Wiedersheim and Ahlborn have stated that the pineal gland in Ammocœtes possesses a gray-white pigment. Owing to scarcity of living Ammocœtes I have not verified this, but I do not for a moment doubt it, and I did not mention it, firstly because I did not think it important, and secondly because I did not wish to lengthen the paper.

I did not describe the pigment in the adult as black, but that such was the case could be inferred from the description, and in not stating its colour I was only following an excellent authority, Prof. Carrière, who, in his book "Die Sehorgane der Thiere," in many cases does not state the colour of eye-pigments. One usually assumes that a retinal pigment is black.

Thus we may say, in very young Ammocœtes the parietal eye possesses black pigment, in older Ammocœtes white pigment, and in adult Petromyzon there is a reversion to black pigment. In what relationship these three pigments stand to each other I am unable to say.

The last point concerns the hypotheses as to the origin of the eye. These were really two in number. The first of them—that which derives the paired eyes and the parietal eye from one common dorsal sense-plate—I hold to be fairly certain, and, indeed, there are many facts to support it.

The second, which derives the parietal eye as a later involution of a portion of this same plate, an involution which was supposed to have taken place after that of the paired eye "Anlage," I only believe to be conceivable. My hope of establishing it lay in the verification of an observation of Goette's; there are no facts to support it, and from more recent investigations of the development I am disposed to attach less value to it. For, from these developmental researches, from studies of the types of eye presented by vertebrates and some invertebrates, and lastly, but not least, from valuable discussion with and criticism by Prof. Wiedersheim, a new track has been found, which gives the explanation of a good deal, but the problem is too long and complicated for treatment here.

The first hypothesis mentioned above is taken as the starting-point, but for the further details there are several other questions which have first to be solved. J. BEARD.

Anatomisches Institut, Freiburg i/Br., July 20.

Physiological Selection.

LIKE so many others who have written on this subject, Mr. Rusden freely criticises my views without having deemed it desirable to read my paper. Had he taken the trouble to do so, he would have found a sufficient recognition of the general fact that instinctive habits not unfrequently serve to mitigate the swamping effects on incipient varieties of intercrossing with their parent forms. Moreover, he would have found that there are others of these habits mentioned by me which are probably much more effectual in this respect than is the one to which he draws attention. Nevertheless, it appears to me evident that all these habits taken together cannot count for much, even where they occur; while it is unquestionable that they occur only in a very small fractional part of organic nature considered as a whole—namely, in some among the more intelligent species of animals. The whole of the vegetable kingdom, an immense majority of the Invertebrata, and a considerable majority of the Vertebrata, cannot possibly have had any of their specific differentiations influenced by any of these forms of what I have already designated as "psychological selection." This sufficiently obvious consideration appears to have entirely escaped Mr. Rusden. He adduces a well-known and a comparatively limited form of psychological selection as a "simple solution" of the difficulty from free intercrossing in all cases!

The other parts of his letter merely indorse the views which are published in my paper. I there say that the theory of natural selection is not, strictly speaking, a theory of the origin of species, but a theory of the development of adaptations. Having read this statement, your correspondent writes:—"To consider the theory of natural selection as a theory of the origin of species is, therefore, clearly an error. . . . The theory of natural selection is one, not of the origin of species at all, but of the preservation of particular varieties," *i.e.* those which present an adaptive character. I do not see how his agreement with my views in this matter could be more clearly expressed, and therefore I cannot understand why he supposes that he is here criticising anything which I have written. If the point of his criticism is that I imagine Mr. Darwin to have fallen into the error of regarding the theory of natural selection as (primarily) a theory of the origin of species, this would merely show again that he has not read my paper. My contention from the first has been that upon this point I am in full agreement with Mr. Darwin, and differ only from those Darwinians who differ from their master in holding that *all* specific changes are likewise adaptive changes, and *vice versâ*. It is only in the presence of this non-Darwinian assumption that specific changes and adaptive changes become synonymous terms, with the consequence that the theory of natural selection is to be regarded as in all cases the only theory of the origin of species.

And this leads me to the last point in my critic's letter. I have argued that the above-mentioned non-Darwinian assumption is opposed to observable fact, seeing that "in a large proportional number of cases" specific characters appear to be wholly useless. Nothing has surprised me so much on the part of my critics as to have found this statement vehemently challenged by so accomplished a naturalist as Mr. Wallace, and therefore I am now engaged in collecting a quantity of evidence upon the subject. But the point here is that Mr. Rusden appears to think there is some ambiguity attaching to the terms "use" and "utility." For he asks whether these words have "any real significance outside human interests and considerations." Now, I can scarcely understand how anyone at this time of day could suppose that when these words are employed in their Darwinian sense they are intended to have any reference to human interests. When an evolutionist speaks of the utility of an organ, it is hardly conceivable that anyone should understand him to mean anything else than the utility of that organ to the species which presents it. Therefore, the term "utility" is equivalent to the term "adaptation," and to say that any organ or structure is of use is one and the same thing as to say that it is adapted to the performance of a function which is of benefit to the organism or to its species. Such, at any rate, is the only sense in which I have myself employed these words; and in doing so I have, of course, followed the terminology of Mr. Darwin, as my critic might have observed without going beyond one of the quotations which he himself makes from the "Origin of Species"—namely, "I have called this principle by which each slight variation, if useful, is preserved by the term 'natural selection.'"

GEORGE J. ROMANES.

Geanies, Ross-shire, N.B., July 29.

The Droseras.

MISS ANNE PRATT in her "Wild Flowers," vol. ii. p. 155, in describing the three British species, after stating the character of the stems and flowers, remarks, "but many persons who know the plant well have never seen the flowers fully open." Two of the species, *D. rotundifolia* and *D. longifolia*, are found in a bog on a common near here, and these have lately flowered in captivity. They were transferred from their habitat and placed in a large saucer with peat and Sphagnum, under a bell glass. The flowers have expanded from 10 a.m. to noon each day, after which the sun left them. A *D. longifolia* in another position was seen to flower at 2 p.m. Moisture and sun seem the conditions to bring out the blossoms. I am not aware whether they have flowered *in situ*, as my plants were gathered in the early morning.

Ramondia pyrenaica, brought from Bagnères de Luchon ten years ago, has flowered each year on an outside rockery in my garden. J. RAND CAPRON.

Guildown, Guildford, July 28.

Comrades.

MY children and their governess, when staying in the north of Ireland lately, witnessed the following curious display of feeling, in animals not usually credited with feelings. A boar pig was in the habit every morning of going to the basket where a blind kitten of about six weeks old was kept, allowing the little thing to creep on to his back, and then taking it about and caring for it during the day. The kitten got its food at the same time as the pig, and at the same trough. In the evening the man who saw to the animals used to carry the kitten back to its basket to pass the night. "Où donc la vertu va-t-elle se nicher?" E. R.

Pollokshields, Glasgow, August 1.

A NEW COSMOGONY.[1]

II.

DR. BRAUN has earned by his excellent series of observations on sunspots (NATURE, vol. xxxv. p. 227) a title to be heard with particular respect on subjects connected with solar physics. In unfolding his views

[1] "Ueber Cosmogonie vom Standpunkt christlicher Wissenschaft. Mit einer Theorie der Sonne." Von Carl Braun, S.J. (Münster: Aschendorff, 1887.) Continued from p. 323.

regarding them in the three concluding sections of his work on Cosmogony, he by no means underrates the difficulties they present. The range of our sensible experience shrinks into absolute insignificance when compared with the exalted conditions reigning in the sun. The temperature at its surface may well reach 40,000° to 100,000° C.; near the centre it mounts probably (our author considers) to ten, possibly to thirty or more million degrees. This unimaginable vehemence of heat is balanced by an unimaginable urgency of pressure. The statement that, in the depths of the sun's interior, it reaches a maximum of 2,000,000,000 atmospheres gives only nominal expression to its value. Figures can at times keep pace with facts only on the condition of being reduced to empty and meaningless symbols.

Gravity and molecular motion—the two universal antagonists—here carry on a conflict intensified far beyond the control of "laws" derived from terrestrial observation. The correlation between elasticity on the one side, and pressure and temperature severally on the other, established by Boyle and Gay-Lussac, holds good only over a strictly limited range of conditions. Calculations founded on the supposition of its continued prevalence in the sun lead at once to manifest incongruities. Solar speculators are thus left, to a great extent, without the guidance of ascertained principles. In the region frequented by them, the scientific imagination has free play. Apposite facts are scarce; misleading analogies too much abound. It cannot then be wondered at if theories of the sun often include extravagances which it is easier for their critics to discern than for their constructors to avoid.

A futile debate has sometimes been raised as to whether the interior constitution of the sun is liquid or gaseous. The truth seems to be that neither word is properly applicable. Without unduly stretching its original meaning, neither describes with even approximate accuracy the state of things prevailing there. The notion of "critical points" has been called in question, and may be inexact. But its introduction has at least had the not unimportant effect of abolishing an artificial distinction. It has shown the separation of the various states of matter to be merely provisional. Their characteristic qualities depend upon circumstances for their development or maintenance. At transcendental temperatures and pressures, the ordinary—probably (as Dr. Braun remarks) even the scientific—criteria of gases and liquids disappear; one state merges into the other; they interchange natures; so that we may indifferently regard the sun's interior as composed of vapours compressed, in despite of their almost boundless calorific energy, to the consistence of fresh putty, or of liquids restrained from boiling by the main force of the strata loaded upon them, while expanded to four or five times their ordinary bulk, and rendered internally mobile by the prodigious elevation of the temperature. An indisputable fact, however, and one fundamental to solar physical theory, is that the sun constitutes a vast reservoir of opposing, tremendously-constrained forces, the delicate equilibrium of which cannot be disturbed, however slightly, without producing effects on a commensurate scale.

Upon such inevitable disturbances Dr. Braun founds his *rationale* of the more obvious solar phenomena. The cooling of a body like the sun does not assuredly proceed quite equably. Local excesses of temperature lead to what we may call local revolts against gravity, signified by swift uprushes from great depths of inconceivably heated substances. These are the so-called "metallic prominences." But where the forces called into play lack energy to produce, or the attendant circumstances are not sufficiently favourable to permit, an actual outbreak, an uplifting of the unbroken photospheric surface takes place, and we see a "facula." "Hydrogen-prominences" mark a medium stage of vehemence. They originate from a commotion which primarily fails to outpass the limits of the chromosphere. The injection, however, into it of a prodigious bulk of metallic vapours rapidly heats the circumjacent hydrogen; it spouts upward in a stream which aërostatic pressure tends to perpetuate, and forms, high up above the sierra-edge of the agitated ocean it springs from, a rosy cloud conspicuous by reason of its incandescence.

But the connexion here indicated, to be significant, should be invariable, which is very far from being the case. Metallic intrusions into the chromosphere are by no means a condition *sine quâ non* to the development of quiescent prominences.

Solar theorists are now for the most part agreed that spots must be ascribed *immediately* to falls of relatively cool matter upon the photosphere; they divide on the question whether the initial disturbance comes from beneath or above it. Dr. Braun ranges himself on the side of those who assimilate outbursts on the sun to volcanic commotions on the earth. Uprushes of vividly glowing substances due to the temporary preponderance of heat over pressure are answered by downrushes of obscure absorbing vapours. Spots would thus be the reactive effects of flames or prominences. Their occurrence would be impossible without preliminary eruptions. But it is at least doubtful whether in this hypothesis the real sequence of events be not inverted. The whole tenor of Mr. Lockyer's observations goes to prove that the yawning of the photosphere leads the way as a symptom of its agitation. After a spot has begun to form, its flame and facular garnishings are added. M. Trouvelot has, indeed, often perceived a nascent spot to be completely masked by towering masses of faculæ; but it is none the less there, waiting to be disclosed. Prof. Young considers the appearance of a spot to be commonly heralded by manifest disturbances of the surface; but since the disturbance is evidenced as well by the presence of "pores" (which may be termed embryo spots) as of faculæ, his authority can scarcely be invoked as decisive of the question of precedence.

This is really the touchstone of the rival theories. Outbursts from the photosphere are either the cause or the consequence of the obscurations of it termed "spots." If the former, they should unfailingly and unmistakably take the initiative. But facts certainly warrant no such rigid conclusion. Admitting then the alternative order of connexion, we can understand that descents of relatively cool matter from coronal regions, perforating the photosphere, must overturn the precarious equilibrium of heat and gravity reigning beneath it, and may thus occasion the tumultuous heavings visible as faculæ, and the amazing escapes of imprisoned vapours challenging attention as flames.

Dr. Braun's papers on the constitution of the sun were published in *Natur und Offenbarung* previous to the appearance of Mr. Lockyer's "Chemistry of the Sun." Hence, perhaps, his complaint that the observed facts regarding the solar rotation had as yet been included in no "plausible" hypothesis. We cannot think him successful in his effort to supply the want.

Adventitious arrivals of nebular supplies from interstellar space play, as our readers are already aware, an indispensable part in the theory of planetary development sketched in the earlier chapters of the work now, in its concluding portion, engaging our attention. By their agency the primitive nebula was set whirling with a motion accelerated outward, its central sluggishness persisting throughout, and modifying the whole of its long history. The inequality is perpetuated within the body of the sun itself, the innermost parts of which may require, our author thinks, as much as forty or fifty days to complete a rotation performed at the equatorial surface in twenty-five. The quickening of angular rate continues with ascent into the solar atmosphere, until, in its higher regions, the period is reduced to ten, if not to five or fewer days. All this, we are asked to believe, is the work of the latest of our nebular annexations, which, forming an equatorial girdle round the sun, partially, and in a degree varying inversely with latitude, communicated its own more rapid movement to the superficial layers of the globe it encompassed.

The process is not even yet concluded. What Dr. Braun holds to be indisputable proof of atmospheric acceleration is derived from Prof. Young's spectroscopic measurement, in 1876, of the sun's rotational velocity. But this is to lay upon the observations in question a burden of inference heavier than they will bear. The rate of equatorial movement, as computed from the observed translation of spots, is 1·25 miles a second; it came out 1·42 miles from the Dartmouth College measures. Considering, however, the extreme minuteness of the entire displacements due to this speed, amounting, for the D lines, to but $\frac{1}{77}$ of the interval between them, the discrepancy is hardly surprising; and it is well known that, in this particular class of determinations, errors lie almost always on the side of excess. Prof. Young himself, it is true, was "inclined to think" that his result betrayed an actual sweeping forward of the absorbing layers over the underlying surface; but even were the fact established, we should expect to find for it a cause less remote than the inrush of a nebula uncounted millions of years ago. Undoubtedly, however (so far we are in agreement with Dr. Braun), that cause would be found to be closely connected with the anomalies of the sun's rotation.

As regards the distribution and periodicity of spots, we are in the present work offered simple and avowed conjectures at which we need only glance in passing. The nebulous swathing not yet completely incorporated with the sun's mass impedes, and has during past ages still more effectually impeded, equatorial radiation. Hence, cooling has, in polar tracts, penetrated further into the interior, with the result of generating an internal spheroidal surface at which temperature-gradients attain a maximum, and from the middle latitudes of which special facilities are afforded for eruptive outbreaks.

But this device is assuredly not a practicable one. The highly artificial arrangement it establishes could not endure one hour. Convection-currents would speedily and without ceremony abolish it. Indeed, augmented radiation from near the poles (which is equivalent to more rapid cooling), besides being contradicted by observation, might be expected to produce just the opposite effect of intensifying the disturbances attendant on cooling. Spots and flames should then, on the hypothesis advanced, be transferred from their "royal" zones to the polar calottes.

Heat-pulsations in a period of $11\frac{1}{9}$ years, occasioned, perhaps, by a slow mechanical oscillation of the sun's volume, the progressive contraction of which may be conducted rhythmically, or by regular alternations of shrinking and swelling, are invoked (certainly under every reserve) to solve the puzzle of the sunspot cycle. The difficulties attending what might be called the "disturbed thermal equilibrium" theory of solar phenomena could not be more forcibly illustrated than by the straits to which it reduces its advocates.

The study of coronal appearances compels our author to take refuge in the unassailable stronghold of electricity. We are far from asserting that he is not fully justified in this measure: the circumstances indeed seem to prescribe it; yet it is always felt to be a desperate one, for the reason that it lands us, almost completely, in the region of the unknown. It is right to add that Dr. Braun is at all times evidently loth to separate from the company of ascertained facts and laws. He advances without them only where their escort cannot be made available.

A. M. CLERKE.

MUSIC IN NATURE.

IN the February number of *Longman's Magazine*, there is a remarkable article "On Melody in Speech," by Mr. F. Weber, Resident Organist of the German Chapel Royal, St. James's Palace. The object of the writer is more comprehensive than his title expresses, for he says in his opening paragraph, "There is an infinite variety of interesting and pleasing sounds in Nature's music around us that may be noted by an attentive ear." This may be readily granted; but Mr. Weber goes on, "These sounds are mostly melodious and harmonious, or *in some harmonious connexion, and form exact intervals and chords.*"

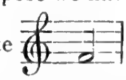
This last sentence is the point of the article. Mr. Weber is not expressing himself figuratively: he writes as a musician, and he distinctly asserts that many of the sounds spontaneously produced in Nature are truly MUSIC in the musician's, and not the poet's, sense of the term. To illustrate this assertion he has taken the trouble to identify and write down, in actual musical notes, the musical passages which he considers he has recognized in a great variety of these natural sounds, and so has challenged the public judgment on the accuracy of his theory.

Now, Mr. Weber is a gentleman of eminence in his profession, and what he says deserves attention. It is easy to say that he has given his imagination too much play in his supposed identifications; but it seems to me the subject ought to be approached from a more comprehensive point of view. The question is, Do such sounds or series of sounds constitute music? or do they not? And if not, why not? If Mr. Weber is wrong, it is probably because he has formed too hasty a view of what music really is; and this is a point that requires serious discussion.

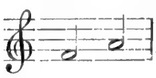
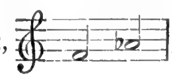
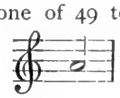
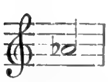
Mr. Weber is not the first who has had this idea. Half a century ago, Gardiner, of Leicester, also a clever musician, published a book called, I think, the "Music of Nature," in which he wrote down musical passages professedly representing a vast number of natural sequences of sounds. There are many other persons, who, while they would not go to the same length as Mr. Weber or Gardiner, still believe that music may be found in the sounds of Nature, and it is worth while to see what grounds there are for such a belief.

Music, in its modern form, is a very complicated structure, combining many elements, such as melody, harmony, counterpoint, tonality, measured time, rhythm, form, expression, tone-colour, and so on. But no one will suppose that the combination of all these is necessary to make what may be strictly called music. We must begin at the other end, and ask what music is if reduced to its simplest possible form? What are the fewest and least conditions absolutely necessary to constitute music, *i.e.* to give the name of music to a combination of sounds?

In the first place, we must have the proper material, namely *musical sounds*, and we must be particular that the sounds are really of a musical character. I am not going into acoustics. I need only say that the most essential quality necessary to give a sound this character is that it must have a *fixed and definite pitch*. A sound that is wavering and indefinite, like the sighing of the wind, or the *portamento* of a voice or violin, though it may be loosely said to be musical, is not strictly a "musical sound." It cannot be defined by the number of its vibrations, it cannot be expressed in any musical notation, and it cannot be used to form musical structure. For this purpose a sound, though it may be short, must be perfectly definite.

Now, suppose we have a sound of this kind, producing say this note . Does the sounding of this note of itself constitute music? We must say No; for the reason that music is an artistic *structure*, which cannot consist of one sound only. We must have other sounds to build up with it. We should hesitate, practically as well as theoretically, to give the name of music to a monotone.

Supposing, then, we add another sound, differing in pitch. Will these two sounds, heard either together or in succession, constitute music? The answer depends on the relation which the pitches of the two sounds bear to each other. For reasons which can be explained, but which it is unnecessary to go into here, it has been settled, by the universal concurrence of all nations who have made music an artistic structure, that the sounds to be used therein may not be chosen at random, but shall only be such as bear certain defined relations of pitch to each other. These relations have varied at different times and among different peoples; in our case they refer to our acknowledged musical *scales*.[1] Hence the answer to our question will be that if the two sounds are related to each other in a way accordant with our scales (but not otherwise) they may be used to form a part of our artistic musical structure, and do constitute elementary music.

Thus, suppose the second sound to vibrate quicker than the first one in the proportion of 5 to 4, or so near to this ratio that it may be mistaken by the ear for it. This will produce the note , having a relation to the former acknowledged in our scales. Then the two notes sounded successively , or together , will constitute music; the former being an element of melody, the latter of harmony. Similarly in the proportion of 6 to 5, , the same thing may be said.

But if we suppose the vibrations of the second sound to have the ratio to those of the first one of 49 to 40, giving a note about half way between  and , we should decline to acknowledge the succession or combination as music according to our understanding of the term, though it might be so in the systems of the Greeks or of other nations.

We arrive, therefore, at the conclusion that the essential feature of music, its minimum component, must be a combination of sounds of different pitches, these pitches being moreover strictly fixed and defined, and their relations to each other corresponding to certain series agreed on and adopted as standard musical scales. Such combination will of itself constitute music; we may add all sorts of other features; but without the above essential foundation we cannot have music, in an artistic point of view.

This definition will enable us now to inquire whether or to what extent "music" in this sense is actually to be met with in Nature, or in the sources mentioned by Mr. Weber.

To begin with, the natural production of the first requisite, *i.e.* notes of fixed and definite pitch, is not frequent. Most sounds naturally produced are uncertain and wavering; precision in pitch of a sound always conveys an impression of artificiality, of its being, in fact, made purposely musical. No doubt natural sounds of definite pitch, even long sustained, do occur. The nightingale is remarkable for a beautiful long steady holding note; a quail gives three notes successively, at the same definite pitch; other birds, and occasionally some animals, give short notes clearly defined; and such notes may even be produced by inanimate causes of steady action, as a waterfall, or any substance naturally set in elastic vibration. All these, however, are exceptional.

But if there is this difficulty in getting one musically-defined note, it will be still harder to find in any natural source the occurrence of two or more such notes that are musically related to each other. The case that will probably first occur to one's mind is the song of our old friend the cuckoo, who has been immortalized by Beethoven as a musical performer. There was some time ago a controversy as to the true notes of the cuckoo, in which some eminent musicians took part; but I fancy the case is the same as the well-known fable of the chameleon: each of the witnesses was right, but the true solution of the question escaped them all. The facts would appear to be as follow.

The cuckoo gives out successively two very distinct strictly musical sounds, and his vocal organs are so proportioned that the interval between them may vary from about two semitones to five. This interval changes with the bird's age. Early in the season it is at its smallest. On May 5 last I heard on the Monte Sacro, at Orta, in Italy, a bird with a splendid voice give notes about a tone apart: late in the season I have heard them fully at a fourth interval. Of course, therefore, as the change goes gradually on they will pass through a minor third and then through a major third (which is Beethoven's interval); but the *exactitude* is all a matter of chance. I have noticed sometimes that the interval lay between a minor and a major third, so that I could not decide to which it inclined. Hence the idea that the cuckoo gives, by predetermined arrangement, any interval recognized in musical scales, is quite a fallacy.

Mr. Weber asserts that "all the animals on land, quadrupeds and bipeds, have their characteristic voices and calls in distinct intervals. The cow gives a perfect fifth and octave or tenth; the dog barks in a fifth or fourth; the donkey brays in a perfect octave; the horse neighs in a descent on the chromatic scale; the cat in a meek mood cries in a fifth, or, when excited, in a major third; proud chanticleer crows in the diminished triad and [in the diminished] seventh chord." All these Mr. Weber writes down; but I fear that more careful observation would never substantiate the idea that the intervals do really correspond with those of our very artificial scales, otherwise than occasionally by pure accident.

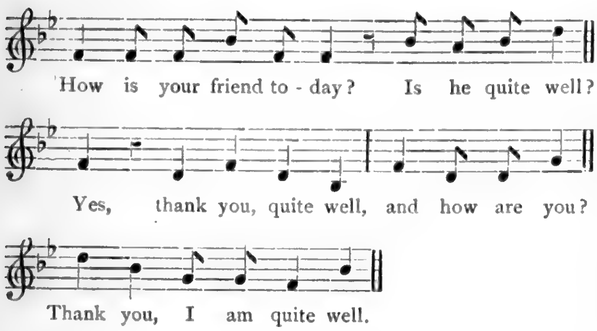
But he makes also inanimate objects conform to our musical system.[1] The wind, he says, sings certain melodies, precisely according to our scales and to our notation, in which he writes them down. This is even more incredible.

Then we come to the main topic of Mr. Weber's essay, "Melody in Speech." He gives a large number of examples, written out fully in musical notation, professing to represent natural language in different varieties: casual expressions, salutations, questions and answers, conversations, and winding up with a speech of an Oxford Professor and a sermon of an English Bishop. These representations are very curious; but it is of course open to any musician to observe for himself the language he hears every hour of the day, and to judge how far it corresponds with this alleged musical character. There is, however, a more conclusive form of test, by what we

[1] Further explanations on this point will be found in my "Philosophy of Music," Second Edition (London: Trübner; and Novello and Co., 1887).

[1] Of course we must exclude from consideration the natural harmonics of vibrating bodies, with which our system is purposely connected. Some really musical effects of the wind may be exceptionally produced in this way.

may call the reverse method. Take the following example given by Mr. Weber:—



Now, let anyone execute this as written; and then ask the bystanders whether it represents the manner in which the sentences would be expressed in talking. I think they would say, "That is not *talking*; it is *singing*;—it is opera recitative." And so on for all the examples. Mr. Weber seems to ignore the essential difference between the two; *i.e.* the absence, in speaking, of the requisites to constitute elementary music. In the first place, in natural speaking there are no musical sounds, properly so called, inasmuch as it is scarcely possible for a hearer to catch notes of fixed and definite pitch; the voice tends constantly to wander about in a vague and indefinite way; the vocal cords, under the natural prompting, have no tendency to remain at a permanent degree of tension; to keep them so there must be an intentional artificial effort; and hence the occurrence, in speaking, of a monotone long sustained is unusual, and has a distinct musical character.

But if we could occasionally trace, in speaking, sounds of definite pitch, we should find the other requisite—namely, definite relations between them—wholly wanting. The idea that a person, when he speaks naturally (be he musician or no musician), has our scales in his mind, and makes his voice conform to them, is altogether untenable. The moment this is done it ceases to be natural speaking, and becomes designed musical performance.

Here, therefore, we find a most positive and unmistakable distinction between natural speech and music. The person using his voice must, for the latter purpose, do two things which require predetermination and effort; and which are therefore essentially artificial and not naturally prompted: he must execute tones of well-defined pitch, and he must give them certain definite pitch relations with each other.

Mr. Weber has, in this matter, unintentionally approached very nearly a matter much debated among philosophers, namely, the Origin of Music. Mr. Herbert Spencer, some years ago, propounded a theory that music had taken its rise from the inflections of the voice in ordinary language. This has been strongly controverted; but Mr. Weber goes further, and asserts that ordinary language is actually *music ready made!*

He has given, as a part of his illustrations, some interesting examples of street cries. These have no doubt a really musical character; but it is odd that he did not see the distinction between them and ordinary talking—namely, that such cries and calls are *purposely sung*, and not spoken in the natural way. Of course, conforming to this condition, they can be correctly written down, and reasoned upon as specimens of true musical melody.

There is a useful moral to be drawn from all this; namely, a regret at the discouragement which is given to the study of the theory of music in a scientific point of view. Grove's Dictionary declares this to be useless to practical musicians, and so it is as long as they confine themselves to practice; but when they meddle with theory the want of it must instantly make itself known. It is no disrespect to Mr. Weber to say that his article shows the loose way in which such matters are too often regarded. No one who has taken the very first steps in the philosophical study of the structure of music could entertain the idea that the sounds naturally emitted by birds, cows, or dogs, formed by the howling of the wind, or used in conversation, were entitled to be called either "music" or "melody." WILLIAM POLE.

THE BRITISH MUSEUM (NATURAL HISTORY BRANCH).

AMONGST the many new and interesting features connected with the British Museum (Natural History), Cromwell Road, has been the opening of a new gallery to the public, containing the Historical and Type Collections in the Department of Geology and Palæontology, under the care of Dr. Henry Woodward, F.R.S., who has favoured us with the following account of the same.

Taking the exhibition cases in *chronological order*, the earliest is the "Sloane Collection." This is the most ancient portion of the Geological Collection, having formed a part of the Museum of Sir Hans Sloane, Bart., F.R.S., acquired by purchase for the nation in 1753.

The geological specimens are stated to have consisted "in what by way of distinction are called *extraneous fossils*, comprehending petrified bodies, as trees or parts of them; herbaceous plants; animal substances," &c., and reported to be "the most extensive and most curious that ever was seen of its kind." Until 1857 the fossils and minerals formed one collection, so that a large part of the Sloane Collection consisted probably of mineral bodies and *not organic*, but in any case only about 100 specimens of invertebrate fossils can now be identified with certainty as forming part of the original Sloane Museum. Each specimen in the Sloane Collection had originally a number attached to it, corresponding to a carefully prepared Manuscript Catalogue, still preserved, which contains many curious entries concerning the various objects in the Museum. In the course of more than 130 years, many of these numbers have been detached from the objects or obliterated in cleaning. But as all fossils at this early date were looked upon *merely as curiosities*, but little attention was paid to the formation or locality whence they were derived. Historically, the collection has immense interest to us, marking the rapid strides which the science of geology has made of late years, especially as regards its more careful and systematic methods of study.

The next collection in chronological order is the "Brander Collection," and is the earliest one in which types of named and described species have been preserved.

This collection was formed by Gustavus Brander, F.R.S., in the earlier half of the last century, and an account of the same, with eight quarto plates, was published in 1766, entitled, "Fossilia Hantoniensia Collecta, et in Musæo Britannico deposita." The descriptions of the species given in the work were written by Dr. Solander, one of the Officers of the British Museum. They were "collected in the County of Hampshire, out of the cliffs by the sea-coast between Christchurch and Lymington, but more especially about the cliffs by the village of Hordwell, nearly midway betwixt the two former places" (*op. cit.* p. 111).

Only a small number out of the original 120 figured specimens are now capable of being identified, the rest having become, in the course of 120 years, commingled with the far more numerous and later Eocene Tertiary acquisitions, and so have lost their connexion with this admirable memoir. The engravings of the shells are equal to any modern published work descriptive of the fossils of the Eocene formation; but the names given by Dr. Solander are in many instances incorrect, according to our present knowledge of the genera of Mollusca.

The next series to which attention may be directed is the collection of Dr. William Smith. This collection, which was commenced about the year 1787, was purchased by the Trustees in 1816, a supplemental collection being added by Dr. Smith in 1818.

It is remarkable as the first attempt made to identify the various strata forming the solid crust of England and Wales by means of their fossil remains. There had been other and earlier collections of fossils, but to William Smith is due the credit of being the first to show that each bed of chalk or sandstone, limestone or clay, is marked by its own special organisms, and that these can be relied upon as characteristic of such stratum, wherever it is met with, over very wide areas of country.

The fossils contained in this cabinet were gathered together by William Smith in his journeys over all parts of England during thirty years, whilst occupied in his business as a land surveyor and engineer, and were used to illustrate his works, "Strata Identified by Organized Fossils," with coloured plates, quarto (1816; four parts only published): and his "Stratigraphical System of Organized Fossils" (quarto, 1817).

A coloured copy of his large geological map, the first geological map of England and Wales, with a part of Scotland, commenced in 1812 and published in 1815—size 8 feet 9 inches by 6 feet 2 inches wide, engraved by John Cary—is exhibited in the last wall-case on the right-hand side of this gallery, at the north end. It is well worthy of careful inspection.

William Smith was born at Churchill, a village of Oxfordshire, in 1769; he was the son of a small farmer and mechanic of the same name, but his father died when he was only eight years old, leaving him to the care of his uncle, who acted as his guardian. William's uncle did not approve of the boy's habit of collecting stones ("pundibs" = *Terebratulæ*, and "quoit-stones" = *Clypeus sinuatus*); but seeing that his nephew was studious, he gave him a little money to buy books. By means of these he taught himself the rudiments of geometry and land-surveying, and at the age of eighteen he obtained employment as a land surveyor in Oxfordshire, Gloucestershire, and other parts, and had already begun carefully and systematically to collect fossils and to observe the structure of the rocks. In 1793 he was appointed to survey the course of the intended Somersetshire Coal-Canal, near Bath. For six years he was the resident engineer of the canal, and, applying his previously-acquired knowledge, he was enabled to prove that the strata from the new red marl (Trias) upwards followed each other in a regular and orderly succession, each bed being marked by its own characteristic fossils, and having a general tendency or "dip" to the south-east.

To verify his theory he travelled in subsequent years over the greater part of England and Wales, and made careful observations of the geological succession of the rocks, proving also, by the fossils obtained, the identity of the strata over very wide areas along their outcrops.

His knowledge of fossils advanced even further, for he discovered that those *in situ* retained their sharpness, whereas the same specimens derived from the drifts or gravel-deposits were usually rounded and water-worn, and had reached their present site by subsequent erosion of the parent-rock.

In 1799, William Smith circulated in MS. the order of succession of the strata and embedded organic remains found in the vicinity of Bath. His large geological map of England and Wales is dated 1815. On June 1, 1816, he published his "Strata Identified by Organized Fossils," with illustrations of the most characteristic specimens in each stratum (4to). In 1817 he printed "A Stratigraphical System of Organized Fossils," compiled from the original geological collection deposited in the British Museum (4to). In 1819, he published a reduction of his great geological map, together with several sections across England. These have just been presented to the Museum by Mr. Wm. Topley.

Mr. Smith received the award of the *first* Wollaston Medal and fund in 1831, from the hands of Prof. Sedgwick, the President of the Geological Society, "as a great original discoverer in English geology, and especially for his having been the first, in this country, to discover and teach the identification of strata, and to determine their succession by means of their embedded fossils."

In June 1832, the Government of H.M. King William the Fourth awarded Mr. Smith a pension of £100 a year, but he only enjoyed it for seven years, as he died August 28, 1839. In 1835, the degree of LL.D. was conferred upon Mr. Smith by the Provost and Fellows of Trinity College, Dublin. The highest compliment paid him was that by Sedgwick, who rightly named him "the Father of English Geology."

The bust above the case which contains William Smith's collection is a copy of that by Chantry surmounting the tablet to his memory in the beautiful antique church of All Saints at Northampton, where his remains lie buried.

We come next to a collection, the very name of which betrays the antiquity of its origin. It is known as "Sowerby's Mineral Conchology."

This collection was begun by Mr. James Sowerby, prior to 1812, and continued by his son, Mr. James de Carle Sowerby, during the preparation of their great work entitled, "The Mineral Conchology of Great Britain," which appeared in parts, between June 1812 and December 1845, and forms six volumes octavo, illustrated with 648 plates.

The value of the work consists in the fidelity and accuracy of the figures given, and also in the fact that most of the specimens drawn are here named and described for the first time. They comprise fossils from all parts of England and from every geological formation. The small green labels mark the specimens actually figured in the work. The collection was purchased by the Trustees from Mr. J. de Carle Sowerby, January 1861. It may be interesting to record that many of the later parts were illustrated by plates drawn by the late Mr. J. W. Salter, for so many years palæontologist to the Geological Survey. When a youth, Salter was apprenticed to Mr. J. de Carle Sowerby, who was at that time both a naturalist and an engraver. The youthful apprentice afterwards married his master's daughter, and became, as is well known, one of the most brilliant palæontologists in this country.

Another curious but small series represents the "types" or "figured specimens" of "König's *Icones Fossilium Sectiles*."

This illustrated work, on miscellaneous fossils in the British Museum, was prepared by Mr. Charles König, the first Keeper of the Mineralogical and Geological Department, after its separation from the general Natural History Collections in 1825.

The engravings are rough, but characteristic, and the first "century" (or 100 figures of fossils), is accompanied by descriptions; the plates of the second "century" have names only, but no descriptions are published with them.

A far more important collection is that known as "The Gilbertson Collection."

In 1836, Prof. John Phillips published Vol. II. of his "Illustrations of the Geology of Yorkshire," which is devoted to the "Mountain Limestone District." In the introduction he writes as follows:—"My greatest obligation is to Mr. William Gilbertson, of Preston, a naturalist of high acquirements, who has for many years explored with exceeding diligence a region of mountain limestone, remarkably rich in organic remains. The collection which he has amassed from the small district of Bolland is at this moment unrivalled, and he has done for me, without solicitation, what is seldom granted to the most urgent entreaty; he has sent me for deliberate examination, at convenient intervals, THE WHOLE OF HIS MAGNIFICENT COLLECTION, accompanied by remarks dictated by long experience and a sound judgment." He (Gilbertson) had proposed to publish on the *Crinoidea* himself, but his sketches, as well as his specimens, were all placed at Prof. Phillips's disposal. Phillips adds: "An attentive examination of this rich collection rendered it unnecessary to study minutely the less extensive series preserved in other cabinets; . . . . *most of the figures of fossils are taken from specimens* in Mr. Gilbertson's collection, because these were generally the best that could be found."

The Gilbertson Collection was purchased for the British Museum in 1841.

The collections which follow mark a distinct era in the annals of geological science. Some forty-seven years ago a little Society was founded by a few London geologists—namely, Dr. J. Scott Bowerbank, F.R.S., Searles V. Wood, Prof. John Morris, Alfred White, Nathaniel T. Wetherell, James de Carle Sowerby, and Frederick E. Edwards—for the purpose of illustrating the Eocene Mollusca, and entitled the "London-Clay Club." They met at stated periods at each other's houses, and after a time they said, "Why should we not illustrate all the fossils of the British Islands, and from every formation?" No sooner proposed than a Society was founded, called the "Palæontographical Society," in the year 1847 (just forty years ago). The first volume, issued in that year, was "The Crag Mollusca, Part I., Univalves," by Mr. Searles V. Wood (with twenty-one plates).

The "Searles Wood Crag Collection" was commenced in 1826, and occupied about thirty years in its formation. It represents the Molluscan fauna of the Red and Coralline Crags of the neighbourhood of Woodbridge, and from Aldborough, Chillesford, Sudbourne, Oxford, Butley, Sutton, Ramsholt, Felixstowe, and many other localities in Suffolk, also from Walton-on-the-Naze in Essex. The specimens so collected were employed by Mr. Searles Wood in the preparation of his Monograph on the Crag Mollusca, published by the Palæontographical Society (1848–61); with supplements in 1871, 1873, and 1879, illustrated by seventy-one quarto plates. Each figured specimen is indicated by having a small green label affixed to it.

A geological description of the Crag formation by Mr. S. V. Wood, Jun., and Mr. F. W. Harmer, was issued by the Palæontographical Society in 1871 and 1873.

The collection was presented by Mr. S. V. Wood to the British Museum, January 1856, and a supplementary collection was given by Mrs. Searles V. Wood in 1885.

The next "Palæontographical Collection" is of nearly equal antiquity and fully of equal merit. It is the Eocene Molluscan Collection formed by the late Mr. Frederick E. Edwards, about the year 1835, and was continually being added to, until a few years before his death, which happened in 1875. It was acquired for the nation by purchase in 1873.

Originally intending to illustrate the fossils of the London Clay, Mr. Edwards extended his researches over the Eocene strata of Sussex, Hampshire, and the Isle of Wight, where, assisted by Mr. Henry Keeping, he made the most complete collection ever attempted by any geologist, and it still remains unrivalled.

Mr. Edwards contributed six memoirs to the Palæontographical Society, 1848–56, also separate papers to the *London Geological Magazine*, 1846, the *Geologist*, 1860, and the *Geological Magazine*, 1865, descriptive of the Eocene Mollusca, in his collection.

Mr. S. V. Wood continued the work for Mr. Edwards, describing and figuring the "Eocene Bivalves" in the annual volumes of the Palæontographical Society for 1859, 1862, 1870, and 1877. Each specimen which has been figured is specially marked.

About 500 species have been described and figured, but the collection is very rich in new and undescribed forms.

The last collection is that of a naturalist who devoted his entire life to the study and illustration of a single class of organisms, namely the Brachiopoda. It was formed by the late Dr. Thomas Davidson, F.R.S. (of 9 Salisbury Road, West Brighton, and Muir House, Midlothian), between the years 1837 and 1886, with the object of illustrating his great work on the "British Fossil Brachiopoda," published by the Palæontographical Society, in six large quarto volumes between the years 1850 and 1886, comprising 2290 pages of text, and 234 plates, with 9329 figures, and descriptions of 969 species.

Dr. Davidson was also the author of the Report on the recent Brachiopoda collected by H.M.S. *Challenger* (vol. i. 1880); of the article "Brachiopoda," in the "Encyclopædia Britannica," ninth edition, 1875; of a Monograph of "Recent Brachiopoda" (Trans. Linnean Society, 1886 and 1887), and of more than fifty other separate memoirs mostly bearing upon Brachiopoda both recent and fossil, printed in the Transactions and Journals of the various learned Societies, &c.

His collection, both of Recent and Fossil Brachiopoda, together with all Dr. Davidson's original drawings, his numerous books and pamphlets, were presented by him to the British Museum through his son Mr. William Davidson, February 1886. By his direction the entire collection of recent and fossil species are to be kept together in one series, for the convenience of reference of all men of science who may wish to consult the same.

NOTES.

ON Tuesday the Technical Instruction Bill was read a second time. The second reading having been moved by Sir W. Hart Dyke, Mr. S. Leighton proposed as an amendment that the measure should be rejected, on the ground that a new charge ought not at present to be imposed on ratepayers. The amendment was negatived; but in dealing with it Mr. Goschen and Mr. W. H. Smith found it necessary, as Mr. Mundella complained, to adopt a very "apologetic" and "persuasive" tone. The fact seems to indicate that a good many members of the House of Commons do not even yet realize that an adequate system of technical instruction is absolutely necessary to enable this country to hold its own in the industrial and commercial warfare of the present age.

IN a memorandum on the Scotch Technical Schools Bill, which has been introduced by the Lord Advocate, it is stated that, "as there is a School Board in every parish and burgh in Scotland, it is unnecessary to extend the powers of the Bill to any other local authority." The Bill is not to take effect in any parish or burgh until after the triennial election of a new School Board next year; and the resolution of a Board to establish a technical school requires confirmation at a second meeting of the Board, and also by the Scotch Education Department. While the subjects to be taught in the technical schools will be determined by the Department of Science and Art, the schools will in other respects be under the Scotch Education Department. No scholar will be admitted into a technical school until he has passed the fifth standard, which in Scotland frees from the obligation to attend an elementary school. The proposal that adults above the age of twenty-one shall not be eligible for admission to technical schools maintained out of the rates ought to be rejected. If adults desire to attend these institutions, there seems to be no good reason why their wish should not be gratified.

ON Wednesday, the 3rd inst., the Local General Committee appointed to make arrangements for the Manchester meeting of the British Association assembled in the Town Hall, Manchester, to receive the report of the Executive Committee. Mr. Alderman Goldschmidt presided, and there was a numerous attendance. In the report, which was adopted, the Executive Committee gave a most satisfactory account of the efforts which had been made to secure the success of the meeting. Attention was especially called to the number of eminent foreign visitors who are expected to be present, and it was definitely announced that among these visitors will be the Emperor of Brazil, who has been a member since 1871. Of the members of the Association more than a thousand have already expressed their intention of attending the meeting, and many ladies and gentlemen have sent in their names as new members or associates. The Executive Committee pointed out that at previous meetings of the Association a large proportion of the visitors had received offers of hospitality, and they expressed a confident hope that Manchester would not fail in this respect. Numerous excursions have been arranged for Saturday, September 3, and Thursday, September 8; and offers of hospitable entertainment have been received and accepted in connexion with several of these. The Duke of Devonshire has invited a party to Bolton Abbey; and invitations have been received from the Duke of Westminster to visit Eaton Hall; from W. H. Foster, Esq., and Sir Thomas Storey, to visit Hornby Castle and Lancaster; from W. Morrison, Esq., M.P., to visit Malham and Gordale; from the Rev. M. Farrer, to visit Ingleborough; and from the Directors of the London and North-Western and Lancashire and Yorkshire Railway Companies to visit their Locomotive Works at Crewe and Horwich. Hospitable offers have also been received in connexion with excursions to Northwich, Buxton, Stonyhurst, Tatton, Macclesfield, Gawsworth, Clitheroe, the Liverpool Docks, the Longdendale Reservoirs, and Worsley. The Liverpool Marine Biological Committee have arranged for a day's dredging expedition; an invitation has been received for a visit to the Isle of Man at the close of the meeting; and several other excursions have been organized. Many of the principal works and mills will be open for inspection during the meeting.

THE following is the programme of the Manchester meeting of the British Association:—Wednesday, August 31, President's Address, in the Free Trade Hall, at 8 p.m. Thursday, September 1, Sectional Meetings, 11 a.m. to 3 p.m.; *conversazione* at the Royal Jubilee Exhibition, by invitation of the Executive Committee of the Exhibition, 7.30 to 11 p.m. Friday, September 2, Sectional Meetings, 11 a.m. to 3 p.m.; lecture by Prof. H. B. Dixon, F.R.S., on "The Rate of Explosions in Gases," in the Free Trade Hall, at 8.30 p.m. Saturday, September 3, Sectional Meetings, 11 a.m. to 1 p.m.; excursions; lecture to working men by Prof. George Forbes, F.R.S., on "Electric Lighting," in the Free Trade Hall, at 8 p.m. Monday, September 5, Sectional Meetings, 11 a.m. to 3 p.m.; lecture by Colonel Sir Francis de Winton, K.C.M.G., R.A., on "Explorations in Central Africa," in the Free Trade Hall, at 8.30 p.m. Tuesday, September 6, Sectional Meetings, 11 a.m. to 3 p.m.; *conversazione* at the Town Hall, by invitation of the Right Worshipful the Mayor of Manchester. Wednesday, September 7, General Meeting in the Chemistry Lecture Theatre of Owens College, at 2.30 p.m. Thursday, September 8, excursions.

THE Royal Archæological Institute has had a very successful series of meetings in Salisbury and the neighbourhood. On Tuesday, the last day of the Congress, the members were entertained at luncheon at Rushmore Park by General Pitt-Rivers, the President. He conducted the party to Woodcutts, where he has lately discovered the remains of a Romano-British village. The skeletons dug up show that the people, whoever they were, that inhabited this village were very inferior in stature, the males being on an average only 5 feet 2 inches in height, and the women only 4 feet 10 inches. General Pitt-Rivers has in his museum a very large collection of articles that must have been in daily use among them, including coins, both British and Roman, brazen, silver, and gilt fibulæ, knife-handles, chains, tweezers, bracelets, locks, padlocks, flint arrow-heads, fish-hooks, and horse-shoes, to say nothing of a bowl of Samian ware and the bricks of a hypocaust. The members, having seen all that General Pitt-Rivers had to show them, agreed that he "had kept the most interesting of all the days and the best of his treasures for the last."

Science announces the death of Dr. Charles Rau, Curator of the Archæological Department of the National Museum at Washington. America owes to him the excellent arrangement of the large prehistoric collections at Washington. His writings on American archæology, contained in the annual reports of the Smithsonian Institution and in various journals, and his recent work, "Prehistoric Fishing in Europe and North America," secured for him a high place among American archæologists.

THE subscription made by some friends of the late Dr. Walter Flight, F.R.S., has resulted in a sum of £317. This amount has been handed over to Mr. Basil Martineau, of Hampstead, and Mr. Henry Basset, F.C.S., of Barnsbury, who have kindly consented to act as trustees on behalf of Mrs. Flight. The Committee of the Fund, anxious to avoid expense, trust that the subscribers will excuse the printing and circulating of individual notices to the above effect.

WE have more than once referred to the scheme promoted from Kew for the establishment of minor Botanical Gardens in the several West Indian Islands. The gardens of the Windward Islands are to be in correspondence, as far as relates to the supply of useful plants, and information concerning them, with the chief Botanical Department in Jamaica. The Island of Grenada has been the first to take advantage of the new scheme. Its newly established Botanic Garden was opened to the public on July 18. Barbadoes has recently recorded its adhesion. We learn with much regret that the members of the group of Leeward Islands decline at present to take any part in the scheme.

THE *Annalen der Hydrographie und maritimen Meteorologie* of the German Hydrographic Office for July contains the first part of the discussion of the daily synoptic weather charts of the North Atlantic Ocean and adjacent parts of the continents for the autumn of 1883, viz. for the months September to November (see NATURE, June 16, p. 159), thus commencing from the period undertaken by the Meteorological Council. The discussion is accompanied by nine charts, showing clearly (1) the paths of the barometric minima, (2) the position and changes of the barometric maxima, and (3) the mean position of the isobar of 765 mm. (30·119 inches) for fifteen periods of three to nine days each. The same number also contains a comprehensive discussion of the rainfall of Mauritius and neighbouring portions of the Indian Ocean, compiled from the observations published by Dr. Meldrum and all other available sources, by Dr. W. Köppen.

THE volume of "Hourly Readings" of the self-recording instruments at the Observatories in connexion with the Meteorological Office for the year 1884, the last part of which is just published, contains two elaborate appendixes:—(1) The harmoni analysis of the diurnal range of air-temperature at seven Observatories for each month of the twelve years 1871-82. The numerical results have been obtained directly from the continuous photographic records by means of Sir William Thomson's harmonic analyzer. A comparison of the monthly means obtained from the machine with those got by calculation shows that the results obtained by the former are as accurate as those obtained very much greater labour by the latter method. (2) Tables and formulæ, by Lieut.-General R. Strachey, R.E., C.S.I., to facilitate the computation of harmonic coefficients in the form $p_1 \cos n\ 15^\circ$, &c., and in the form $P \sin (n\ 15^\circ + \tau^\circ)$. Tables are calculated for the coefficients of p and q from hourly values, harmonic coefficients from five-day means, non-periodic corrections, and multiples of the usual sines. Tables of multiples of the natural and logarithmic sines, &c., have been previously published, but we know of nothing which at all compares in detail and usefulness with the tables now in question, for the calculations which they are intended to simplify.

DR. VON LENDENFELD'S account of his investigations in the Australian Alps, published in Ergänzungsheft No. 87 of *Petermann's Mitteilungen* (see NATURE, July 21, p. 283), contains some interesting observations on the meteorology of that district, especially with respect to rainfall. He found that the mountainous part of the continent has much more rain than any other part of temperate Australia south of the zone of tropical rains. At Hiandra, to the north of the Kosciusko group, it amounts to 61 inches; while between the mountains and the coast the amount is small, being only 18 inches at Cooma. Some places to the west of the mountains, and still in sight of them, suffer much from want of water. Generally speaking there appears to be no connexion between the weather on the Alps and that on the coast. On the latter most rain falls in autumn, while on the mountains the spring is especially wet. In the middle of summer (January and February) rainfall is least both on the mountains and on the coast. In the lowlands precipitation always falls as rain, while on the mountains snow falls at all times of the year, and it never rains in winter. The amount of dew is exceedingly great, but as this is only taken into account in the total amount of the rainfall, the climate of the mountains appears drier than it really is. There are not sufficient observations to determine the temperature and wind conditions accurately, and these can only be estimated from the behaviour of the snow. At heights exceeding 1000 metres (3280 feet) the snow lies for a month or two, and above 2000 metres it is met with in places, even in the height of summer. Snow-drifts are found exclusively on the eastern slopes, which clearly proves the prevalence of westerly winds in winter.

IT has been arranged at the Hong Kong Observatory that Mr. Knipping will, as heretofore, investigate typhoons within the area of the Japanese weather-maps, as well as north and east of that area. Father Faura will investigate the typhoons in their passage across the Philippine Archipelago, and those that approach very near the coast of Luzon. Dr. Doberck will investigate typhoons at sea south and west of Mr. Knipping's district, from information collected from men-of-war and merchant vessels, and typhoons in China from the facts recorded in the returns of the Imperial Chinese Maritime Customs.

A NOVEL series of voltaic combinations, in which solutions of alterable compounds are substituted for the attackable metals, has lately been investigated by Dr. Alder Wright and Mr. C. Thompson (Journ. Chem. Soc., August 1887). The chief feature of the new cells consists in the replacement of the zinc or its equivalent by a plate of carbon, platinum, or other conducting but unchanged substance, immersed in a solution of some easily oxidized or chlorinated compound, and opposed to a similar plate in contact with the solution of a substance capable of being readily deoxidized or dechlorinated. The plate in contact with the oxidizable fluid acquires the lower potential or becomes the negative pole, while the other plate takes the higher potential and forms the positive pole with regard to the outer circuit. An almost endless variety of new combinations may thus be employed, some of which may be expected to develop considerable energy. A convenient cell, of electromotive force 1·5 volt, consists of a U-tube, into one limb of which is poured a solution of sodium sulphite, while a solution of "chromic liquor," that is, a mixture of sulphuric acid and potassium bichromate solution, is run into the other, a little moderately concentrated sulphuric acid being previously placed in the bend to prevent too rapid diffusion of the two. On placing the two platinum plates in their respective solutions and completing the external circuit by a wire, a constant current is maintained, owing to oxidation of the sulphite to sulphate, and reduction of the chromic acid to chromium sulphate. In all the cases examined the currents were remarkably steady, and capable of performing measurable amounts of electrolytical work.

SIR PHILIP MAGNUS has presented his Report on the technological examination held in 1887, under the direction of the City and Guilds of London Institute for the Advancement of Technical Education. A special feature of this year's examination is that forty-eight candidates were examined in nine subjects, under the direction of the Institute, in New South Wales. Examinations were held in Sydney, Bathurst, and Newcastle. The question papers were sent out to Sydney, and the answers of these colonial condidates were forwarded to London, where they were examined together with those of other candidates, the date of the examination having been so timed as to render this arrangement possible. Sir Philip Magnus considers that the increase in the total number of candidates examined and of those who have passed is satisfactory. In 1886, 4764 candidates were examined, of whom 2627 passed; in 1887, 5508 were examined, of whom 3090 passed. The increase in the number of candidates in 1887 is 744 as against 796 in 1886. During the past session 365 classes were held in 121 different towns. Manchester heads the list of provincial centres from which the largest number of candidates have passed, the number being 183 as against 169 in the previous year. Next in order comes Glasgow with 169 as against 163, Leeds with 114 as against 81, Blackburn with 73 as against 10, Huddersfield with 69 as against 70, Belfast with 66 as against 74, Bradford with 63 as against 80.

THE results of the survey and last census of India are that the area of the peninsula of Hindostan is 1,382,624 square miles, and the population 253,891,821. Although immense tracts of country are annually cultivated, according to the most recent survey ten million acres of land suitable for cultivation have not as yet been ploughed. At the same time, 120 millions of acres are returned as waste lands.

ON July 21 the people of Nancy were astonished by the sudden appearance of an immense number of common ants, which were brought by a very strong wind. Most of the insects were wingless.

ACCORDING to the *Free Press* of Singapore, a work which has occupied much of the recent attention of those Government officials connected with the Land and Survey Departments of the Straits Settlements has been completed, and has been sent to England. This is a map of the Malay Peninsula, based upon one produced in 1879 under the direction of the Straits Branch of the Royal Asiatic Society, but altered and improved by subsequent exploration.

IN a recent number of the *Revue Scientifique*, M. Arnaudeau develops the idea of a double postal tube between Dover and Calais, to be suspended in air. Each tube should be

1 metre in diameter, and of thin metal, allowing the supports to be far apart, say 800 metres. In the tube, a train of ten to fifteen small waggons should run on rails on a floor, the motive power compressed and rarefied air actuating a piston. The lower part of one tube should hold telegraphic, and that of the other telephonic wires. The metallic foundation-piers, some of which should be as much as 70 metres high, should be of truncated-pyramid shape, and capable of floating at first, but gradually filled with masonry and water, and sunk to the bottom. These should support tall pillars having suspension-cables at the top. By the pumping out of the water, these piers could be raised and shifted if necessary.

IN the Exhibition recently opened at Havre there is an interesting collection of specimens of poisonous fishes. Some are poisonous when eaten; others are merely venomous. Among the first are many Sparoids, a Tetrodon, and many *Clupea*, which are abundant near the Cape of Good Hope. In the Japan Sea is found a very peculiar Tetrodon, which is sometimes used as a means of suicide. It brings on sensations like those produced by morphia, and then death. Another interesting collection in the Exhibition is that of a number of Bacteria, and pathogenetic microbes. This collection was formed by Prof. Cornil, of Paris.

AT the annual meeting of the Seismological Society of Japan, on May 27, Prof. S. Sekiya exhibited an interesting model of his own design, showing the motion of the ground at the time of an earthquake. The actual motion was magnified fifty times. At the same meeting, Prof. Milne read a paper on the effects produced by earthquakes upon the lower animals. Animals often show signs of alarm not only while an earthquake is going on, but before the shock is felt. Prof. Milne's friend, Mr. James Bissett, of Yokohama, testifies that thirty seconds before the first shock on the 15th of last January one of his ponies suddenly got up on its feet and pranced about in the stall, evidently terrified at the coming shake. A pony at Tokio was observed to act in a similar manner. Prof. Milne has had many opportunities, just before earthquakes, of confirming the fact that pheasants scream; and several observers have assured him that in like circumstances frogs suddenly cease croaking. Of all animals, geese, swine, and dogs are said to give the clearest indications of an approaching earthquake. It is said, too, that many birds show uneasiness, hiding their heads beneath their wings, and behaving in an unusual manner. Prof. Milne suggests that some of the lower animals may be sensitive to small motions which we do not notice. The terror manifested by intelligent animals like dogs and horses may be, he thinks, the result of their own experience, which has taught them that slight tremors are premonitory of movements more alarming. In the case of pheasants, frogs, and geese, alarm may be due solely to the tremors. Strange behaviour on the part of animals several hours or days before an earthquake Prof. Milne attributes for the most part to accidental causes. In volcanic districts, however, as he shows, it has sometimes happened that before an earthquake certain gases have emanated from the earth; and where this has occurred the smaller animals have not only been alarmed, but sometimes killed.

A VIOLENT shock of earthquake was felt on August 26 at Laghouat. It caused much consternation, these phenomena being very rare in the vicinity.

EARLY in December, an Exhibition of winter flowers, plants, and fruit will be opened at Mayence.

THE locust plague has done much damage this year in Algiers. All the efforts of the authorities to cope with it have proved fruitless, and it is feared that the evil will be not less formidable next year, the eggs deposited being numberless.

THE annual meeting of the North of England Institute of Mining and Mechanical Engineers was held on Saturday last in the building of the Newcastle Exhibition. Sir Lowthian Bell delivered his presidential address. Dealing with the progress of railroads and navigation, he pointed out that fifty years ago the tonnage sailing under the British flag might be taken at 750,000, of which a little above 50,000 consisted of steamers. By the end of 1885 this country possessed 3,456,562 tons of sailing ships, and 3,973,483 tons of steam vessels, making a total of 7,430,045 tons. Referring to compound engines, he said they all knew how the dangers attending the use of steam at a high pre sure had been met by the introduction of the compound system, in which, by the use of three cylinders, a great addition to the expansive force of the steam was now extensively employed. To such an extent had this been carried that 350 tons of coal were now doing the work which formerly required 750. Mr. T. W. Bunning read a paper in which he advocated the federation of the different mining and mechanical associations in the kingdom on the lines of the Society of Chemical Industry. Sir Lowthian Bell was re-elected President.

THE additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from West Africa, presented by Mr. T. Sutton Flack; a Blue and Yellow Macaw (*Ara ararauna*) and a Red and Yellow Macaw (*Ara chloroptera*) from South America, presented by Mr. W. Reid Revell; a Green Turtle (*Chelone viridis*) from the West Indies, presented by Mr. James McGregor; two Griffon Vultures (*Gyps fulvus*), European, a Dark-green Snake (*Zamenis atrovirens*), European, deposited; a One-streaked Hawk (*Melierax monogrammicus*) from West Africa, an Elegant-grass Parakeet (*Eupherra elegans* ♀) from South Australia, purchased.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 AUGUST 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 August* 14**

Sun rises, 4h. 45m.; souths, 12h. 4m. 30·3s.; sets, 19h. 24m.; decl. on meridian, 14° 24′ N.: Sidereal Time at Sunset, 16h. 56m.

Moon (New, August 19, 6h.) rises, 23h. 52m.*; souths, 7h. 40m.; sets, 15h. 35m.; decl. on meridian, 18° 18′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridia ° ′
Mercury	3 9	10 49	18 29	17 58 N.
Venus	8 45	14 28	20 11	3 57 S.
Mars	1 55	10 5	18 15	22 32 N.
Jupiter	11 11	16 21	21 31	10 24 S.
Saturn	2 40	10 36	18 32	20 27 N.

* Indicates that the rising is that of the preceding evening.

August.	h.	
16	—	Venus at her point of greatest evening brilliancy.
16	20	Mercury at greatest elongation from the Sun, 19° west.
1	21	Mercury in conjunction with and 0° 33′ nort of the Moon.
19	—	Total eclipse of the Sun. The central line of totality passes from south of the Baltic across Europe, Asia, and Japan. At Greenwich and in England and Scotland generally the sun will rise partially eclipsed a few minutes only before termination of the eclipse. In Ireland the eclipse ends before sunrise.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Aug. 16,	20 28	*m*
R Arietis	2 9·7	24 32 N.	,, 20,		*M*
Algol	3 0·8	40 31 N.	,, 19,	3 48	*m*
R Comæ Berenices	11 58·5	19 25 N.	,, 15,		*M*
T Ursæ Majoris	12 31·3	60 7 N.	,, 16,		*M*
δ Libræ	14 54·9	8 4 S.	,, 19,	21 32	*m*
U Coronæ	15 13·6	32 4 N.	,, 16,	0 53	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 16,	2 28	*m*
			,,	22 36	*m*
X Sagittarii	17 40·5	27 47 S.	,, 17,	21 0	*m*
U Sagittarii	18 25·2	19 12 S.	,, 18,	1 0	*m*
β Lyræ	18 45·9	33 14 N.	,, 14,	4 0	*M*
R Lyræ	18 51·9	43 48 N.	,, 16,		*m*
η Aquilæ	19 46·7	0 43 N.	,, 14,	2 0	*m*
δ Cephei	22 25·0	57 50 N.	,, 20,	0 0	*m*

M signifies maximum ; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near γ Andromedæ	25	42 N.	Swift ; streaks.
,, μ Persei	61	49 N.	Very swift ; streaks.
,, δ Draconis	291	70 N.	Swift ; short.

NEW GUINEA EXPLORATION.

On March 15 last a private exploring Expedition, commanded by Mr. Theodore Bevan, left Thursday Island for New Guinea in the steamer *Victory*, which had been placed for six weeks at Mr. Bevan's disposal by Mr. Robert Philp, the owner. Mr. Bevan's object in undertaking this expedition was to ascertain whether it was possible to reach the mountains in the interior of New Guinea by means of the Aird or other large rivers flowing into the Gulf of Papua, and to establish, if possible, friendly relations with the natives in the neighbourhood of the gulf, with the view of paving the way for future explorations.

We reprint from the *Sydney Morning Herald* of May 23 the following account of the expedition :—

The expedition has proved the existence of spacious waterways leading far into the interior of the island, the two most important—and magnificent rivers they seem to be—having been named the Douglas and the Jubilee. These discoveries may be destined to be of considerable importance to Australia, for a flourishing industrial European community may in the not very remote future settle on the banks of these waterways. Northern Queensland, from its situation, may naturally be expected to reap the greatest advantages from the opening up of New Guinea, but, directly or indirectly, the habitation of its fertile plains and valleys with pioneer settlers must prove beneficial to the metropolis of New South Wales. A comprehensive account of the expedition will be published in due course, illustrated by a chart showing the new discoveries, and by photographic views of new mountain ranges and previously unknown tribes of natives, but a brief description of some of the principal discoveries made will probably be read with interest.

Of the country in the vicinity of the Aird, very little up to the present is known, and at Thursday Island old experienced hands looked upon it as little short of madness, having regard to the supposed treacherous channels existing, and the hostility of the natives, to attempt to enter the rivers which discharge their waters into the gulf. Mr. Bevan, however, paid little regard to the grim forebodings, and the Expedition was fortunate in reaching Cape Blackwood in the month of April, at a time when the waters are invariably smooth, and when there is little reason to fear tempestuous weather. The exploring party soon set to work. Several minor streams were discovered, particulars concerning which will be given in due course, but, as already indicated, two new fresh-water rivers of magnitude were found, disemboguing their waters through various mouths into the Gulf of Papua. Both these pursue a devious course amidst ranges of hills, washing the base at times of lofty mountains. The rivers are longitudinally about 60 miles distant from each other. The first one—the Douglas—is reached by the Aird, up which the *Victory* steamed, and it became manifest that the Aird was only one of several mouths of the main stream, which was navigated for a distance of 130 miles, but which, however, in reality took the party inland only about 80 miles, by latitude, northward of Cape Blackwood. The explorers left this river through a channel marked upon the Admiralty chart as dry land, and this brought them into Deception Bay. The existence of this passage, in which there is from four to eight fathoms of water, proves Cape Blackwood to be an island. It may here be stated that for the first 30 miles up the Aird the country was found to be of deltaic formation, with alluvial islands scattered here and there ; but beyond that the main stream of the Douglas becomes a compact watercourse, flowing between rising ground on either side. The country about the delta is flat, covered with scrub, and the banks are well defined. On the higher waters of the Douglas there is a practically uninhabited forest country, which in parts could be easily cleared. Two important fresh-water tributaries to the Douglas were discovered, one of which has been named the Burns and the other the Philp. A new range of mountains observed in this vicinity was named by the leader after his uncle, Mr. Thomas Bevan, an ex-Sheriff of London.

The Gulf of Papua has been explored up to Orokolo, and to the westward of that village are what appear to be fine rivers, but these were reported by the natives to be separate mouths of one river, and the natives' report has been confirmed by Mr. Bevan, who, proceeding up a sixth large channel to the west of Bald Head, came upon the main river, which fed the delta and cut inland at right angles into the five other rivers. There was a heavy break on each bar of the first five openings, probably due to south-east weather on the Queensland coast, but a smooth-water passage was found into the sixth opening. The time at the disposal of the party was too limited to enable them to survey each opening of the river, so a westerly course was pursued, and the *Victory* steamed up a large channel running in a northerly direction from Bald Head to the point of its confluence with other waters. A week was spent in examining the rivers coming in from the north-west, but although high land was seen it could not be reached by any branch in that direction. The easterly passages were next tried, and a channel was found running easterly and north-easterly, almost at right angles, into two other streams. Taking the branch running inland, they proceeded a few miles further, and found it led into two other streams, one going inland and the other with a current towards the sea as before. Yet again did they meet two other streams, and still steaming up the one leading inland, they, on going 5 miles further, came upon another, and this time the last arm leading seaward. Here they found themselves on a fresh-water river nearly half a mile wide, with a steady current flowing towards the sea. A magnificent panorama of rising country was now opened up. Range over range of hills stretched into the distance, capped by some towering blue mountain peaks, and so clear was the atmosphere that even the high mountains, which must have been leagues away, seemed close at hand. They were all clad with trees, and upon the face of them could easily be distinguished the water-gullies, brightly illuminated by the glistening rays of the sun. The river was navigated 110 miles from Bald Head, or about 50 miles in latitude from Orokolo, its chief trend being in an easterly and north-easterly direction, although the course was unusually serpentine. In honour of Her Majesty's having completed the fiftieth year of her reign, this river, probably the finest in British New Guinea, has been named the Jubilee. The ranges into which the waters carried the little steamer, drawing 9 feet of water, were named the Albert Victor.

Very little trouble was experienced with the natives during the expedition. Only once was the party attacked, and that was when going up the Aird—about 20 miles from its mouth—probably by the same tribe that attacked Capt. Blackwood forty-two years ago. The hostile blacks fired several flights of arrows, some of which fell harmlessly by the vessel's side, but they dispersed at the sound of the steamer's whistle, and after a few shots had been fired wide ; neither the attacking nor attacked sustaining the slightest hurt. Through this untoward circumstance Mr. Bevan was unable to obtain the name of the tribe. Another tribe, who evinced their peaceful intentions by carrying green bows in their canoes, were found inhabiting the country behind Aird's Hill. A third tribe was met with 48 miles inland, as the crow flies, from Cape Blackwood, and these called themselves the Tumu. At the confluence of the Douglas River with Deception Bay, a fourth, the Moko tribe, was found. The Kiwa Pori tribe, the fifth met with, were ascertained to be the inhabitants of the country close to Bald Head, in the Papuan Gulf. The Birumu tribe were seen about 16 miles north-west of Bald Head, and the Evorra, the seventh and last tribe, were found about the same distance north-east from Bald Head. With all, except of course the first, friendly relations were established. Mr. Bevan's previous experience of New Guinea natives and knowledge of some of their habits and dialects were exceedingly serviceable to him; and with the exercise of a little patience he was enabled to inspire them with the fullest confidence. Several natives were induced to go on board the steamer, and were photographed. Only three of the tribes could be spoken of as large, the one possessing the greatest numerical strength being the Kiwa Pori, which numbered from 400 to 500 men. The result of Mr. Bevan's observations is that the country is practically uninhabited except along the coast. No natives were seen on the Jubilee River beyond 25 miles from the coast-line.

The best of the land—and fine rich soil it is—appeared to lie between the head of the deltas of both rivers and the foot of the hills, where it looked exceedingly fertile, and covered in places with a palm scrub which could be readily cleared. Sago, tobacco, bananas, bread-fruit, and sugar-cane were found to be indigenous. As already stated, the country about the deltas is alluvial and flat, and then in turn come sandstone, limestone, and ironstone, as well as the stratified rocks which mark the earlier geological periods. Mr. Bevan hopes, at no distant date, to be able to complete the work of which this preliminary expedition he has now made is but the precursor. In the animal, vegetable, and mineral kingdoms, there is a splendid field for men of science. About eighty ornithological specimens have been obtained by the party, and a few snakes, lizards, and fishes, which will be examined at the Australian Museum. A large and varied ethnological collection has also been obtained by Mr. Bevan in exchange for trade from the tribes with whom he established friendly intercourse. Some of the prominent features in the landscape have been named after Mr. Richard Wynne, Mr. F. E. Joseph, Dr. Ramsay, Messrs. Harrie Wood, C. S. Wilkinson, E. Fosbery, and other well-known Sydney citizens.

A word is necessary with regard to the climate, which is described as by no means unhealthy. The temperature varied from 72° F. at daybreak to about 86° in the shade at noon. The party returned to Thursday Island within the time stipulated by the owners of the *Victory*, in excellent health, and with unimpaired physique. The cost of the expedition was from £500 to £600, but from this a considerable sum, represented by the value of the collections, must be deducted. In response to Mr. Bevan's application, the Government have placed at his disposal a competent draftsman to aid him in making up his plottings.

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE Institution of Mechanical Engineers held their summer meeting last week, at Edinburgh, under the presidency of Mr. E. H. Carbutt. The meetings were held in the Library Hall of the University, the members being received by the Marquis of Tweeddale, the chairman, Sir William Muir, Principal of the University, and other members of the Reception Committee. The two papers first read on Tuesday related to the Forth Bridge and the machinery employed in its construction. Both papers we reprint to-day. The discussion on the first of them referred mainly to the subjects of expansion and contraction under variations of temperature and to wind-pressure, and in reply the author of the paper, Mr. E. M. Wood, explained that 1½ inches per 100 feet was allowed for expansion, or double the amount usually thought sufficient; whilst, as regards the wind-pressure, the highest registered had been 35¼ lbs. per square foot, whilst 56 lbs. was allowed for. All the speakers who discussed the paper of Mr. Arrol, the contractor for the bridge, referred in high terms to the skill and ingenuity exhibited throughout. Later on in the day the members made an excursion to the Forth Bridge, Mr. Arrol and the heads of the various departments at the works acting as guides. A striking feature was the comparative noiselessness with which the work was carried on, owing to the successful use of hydraulic power in riveting.

We regret to learn that on the day of the visit to the bridge two men had lost their lives owing, it is believed, to the staging on which they were employed giving way; this raises the number that have been killed at the Forth Bridge works to six during the last two months, the number of men employed averaging between 3000 and 4000.

The third paper read was by Mr. F. J. Rowan, on electro-magnetic machine tools, which were invented by him to overcome the difficulties of riveting by hand; they perform their work in a very complete way. The conditions of the work itself involve the separation of the riveting portion of the apparatus from the bolster or holder-up, whilst the riveting process requires that the two portions of the machine should be rigidly held together. This is effected by magnets so arranged on opposite sides of the plating with their poles of unlike denomination facing each other, that they are drawn towards each other, thus pressing the plates together, and insuring the proper condition for riveting. The riveting itself is effected by an electric motor, which by means of gearing and a cam, lifts the hammer against a spring, the amount of compression imparted to the spring in lifting being regulated by hand.

The first paper read on Wednesday was descriptive of the electric light on the Isle of May, by Mr. D. A. Stevenson. The machinery, boilers, and engines, are placed near the base of the island, and close to the water-supply, as it was found that the saving which would be effected by not having to convey fuel to the top of the island, or to pump up water, would compensate for the loss of energy due to the resistance of extra length of the electric conductor. The electric generators are two De Meritens alternate-current machines, each weighing 4½ tons. The induction arrangement of each machine consists of five sets of twelve permanent magnets, sixty in all, each magnet being made up of eight steel plates. The armature, 2 feet 6 inches in diameter, is composed of five rings with twenty-four bobbins on each, arranged in groups of four in tension and six in quantity, and makes 600 revolutions per minute. With the circuit open, each machine develops an electromotive force of 80 volts, with the circuit closed through an arc 40 volts. An average current of 220 amperes is developed, thus yielding 8800 watts of electrical energy, or 11·7 horse-power in the external circuit. In the dioptric arrangement constructed by Messrs. Chance to the author's design, the condensing principle has been carried further than in any apparatus previously constructed. The principle consists in darkening certain sectors by diverting the light from them and throwing it into the adjoining sectors so as to reinforce their light. The author agreed with the conclusion arrived at by the Trinity House that taking first cost and annual maintenance into account, electricity should only be used for important landfall-lights; where, however, the most powerful light was desired, independently of cost, the electric arc had no rival. Some interesting observations have been carried on for the last five months which prove the electric light to be the most penetrating of all lights in all states of weather. Every night at twelve o'clock the light-keepers at St. Abbs Head, twenty-two miles distant, where there is a first-order flashing light, and one of the most powerful oil-lights in the service, observe the Isle of May light; whilst the keepers there observe the St. Abbs Light. The result of these observations has been that the Isle of May light has been seen one-third more frequently than the other. The paper was discussed by Sir James Douglas and several other speakers. A paper was read on the construction of the Tay Viaduct, by Mr. F. S. Kelsey, the resident engineer. This bridge is two miles long, and has taken five years to construct, having been opened for traffic on June 20 last. A paper on the dredging of the lower estuary of the Clyde, by Mr. C. A. Stevenson, was read. Both these papers, which were fully discussed, are of technical rather than scientific interest.

In the evening a *conversazione* was given by the Lord Provost, magistrates, and Council of the city in the Museum of Science and Art. Sir William Thomson gave a very exhaustive lecture on waves, concluding with an important suggestion. It seemed to him that inasmuch as wave resistance depends almost entirely on surface action, it might be diminished relatively very much by giving a great deal of body below the water-line. High speeds of 18 or 20 knots might thus be obtained. By making ships like the old French ships, swelling out below the water-line, there would be a large additional displacement and carrying power, and little addition to wave disturbance.

THE STRUCTURE AND PROGRESS OF THE FORTH BRIDGE.[1]

As a visit to the works of the Forth Bridge is included in the programme of the present meeting of this Institution, the author trusts that a short sketch of the preliminary proceedings, with a description of the structure and progress, from one engaged on the work from its outset, will prove of interest in explaining the reasons and means adopted for connecting the railways on opposite shores of the Firth of Forth, at the site of the historic ferry and still existing Hawes Inn, whose time-table for the departures of the ferry-boat is so quaintly alluded to in "The Antiquary."

Previous Proposal.—For many years, suggestions for establishing direct communication between the southern railways running into Edinburgh and the Fifeshire lines, with the object of more direct access to Perth and the north, had been frequently considered by the companies interested in that route; but until an Act of Parliament was obtained in 1873 for the construction of a suspension bridge, designed by the late Sir Thomas Bouch, for crossing the Forth at the site of the present works, no proposal gave prospect of successful issue. Although the type of bridge then proposed was not one generally considered applicable for the passage of railway trains, yet no positive objection seems to have been taken to it, inasmuch as a contract was entered into for its construction, workshops were erected at the site, and foundations were started. But after the severe gale at the close of the year 1879, so destructive to a viaduct in an equally exposed position, it was deemed prudent to suspend operations; and the directors of the North-Eastern, Midland, and Great Northern Railway Companies, which each have an interest in obtaining direct access to the eastern and northern districts of Scotland, requested their respective consulting engineers, Mr. T. E. Harrison, Mr. W. H. Barlow, and Mr. (now Sir John) Fowler, to confer together and report upon the possibility of some other plan for making through communication between the existing lines at the point already selected. Tunnelling was out of the question on account of the depth of the water; the proposals therefore took the form of bridges.

Present Plan.—On May 4, 1881, the engineers submitted their joint report, unanimously agreeing that the steel bridge on the cantilever and central-girder system, designed by Sir John Fowler and Mr. Benjamin Baker, was not only the least expensive, but the best suited for the situation. The soundness of this decision has since received confirmation in the fact that seven long-span bridges have been or are now under construction in different parts of the world, and many more are proposed on the principle adopted for the Forth Bridge. For the substitution of this design in place of the suspension bridge contemplated in 1873, the Forth Bridge Railway Company appointed Sir John Fowler and Mr. Baker their engineers, and obtained an Act of Parliament in July 1882. The financial obligations for the construction of the bridge having been undertaken by the railway companies interested in the through route—namely, the North-Eastern, Midland, Great Northern, and North British—tenders were invited for the work, and from the applications received two offers were selected; and with the combined firm of Messrs. Tancred Arrol and Co. a contract was made in December 1882 for the entire execution of the work.

General Dimensions.—The total length of the bridge will be 8300 feet, or 380 feet over one mile and a half. There are two main spans of 1700 feet each, two side spans of 675 feet each, with the ends counterbalanced and anchored to the masonry, and three intervening piers; these together make up about a mile of the total length, and the remainder is composed of fifteen approach spans of 168 feet each, and of masonry arches and abutments. For a length of 500 feet in the centre of each of the two 1700-feet spans there is a clear headway for navigation of 150 feet above high water; the rails being placed at a level 6 feet higher. From the base of the deepest pier to the top of the cantilevers the total height is 450 feet, or only 10 feet less than the Great Pyramid of Egypt.

The cross sections of the main spans are of trapezoidal form, 330 feet in height from centre to centre of the members over the piers, and 33 and 120 feet in width across top and bottom respectively, and tapering towards the ends of the cantilevers, thus giving a form which is eminently suitable for withstanding lateral pressure. The girders carrying the railway are supported at intervals inside the cantilevers, &c., by trestles or cross frames, and a continuous lattice-work parapet 4½ feet above the rails extends the whole length of the bridge.

Load, and Wind Pressure.—In addition to its own weight the bridge is being constructed to support, without exceeding in any member the unit stresses permitted by the Board of Trade, a load equivalent to trains of unlimited length equal to 1 ton per foot run on each line of railway, or passing trains consisting each of two engines and tenders at the head of sixty coal trucks weighing 15 tons each; and also to withstand a lateral wind pressure of 56 lbs. per square foot of exposed surface of train and structure. The magnitude of the lateral pressure may be judged from the fact that over the mile length of main spans the estimated surface exposed to a point blank wind at right angles to the bridge amounts to a little more than 7½ acres; the pressure of 56 lbs. per square foot on this surface would therefore be equivalent to a total of more than 8000 tons. In addition to lateral winds, the direction from any point of the compass has been provided for, even including the imaginary condition of each group of main piers becoming the centre of a whirlwind. Effects of temperature will be provided for in the rails, and at the junctions of the central girders with the cantilevers; and the bearings on both the main piers and under the weighted ends of the cantilevers have provision made for allowing movements due to changes of temperature and to the elasticity of the cantilevers under lateral pressure. The lateral play allowed is limited, so that the whole of the piers may act in concert to resist combined actions of all forces tending to disturb the normal state of rest of the 50,000 tons of permanent load. As a further provision 48 steel bolts of 2½ inches diameter, secured 24 feet down in the masonry by anchor plates, hold down the bed plates with an initial tension of 2000 tons; the nuts and saddle-plates are so arranged as to allow freedom of lateral movement to the skew backs; but any lifting would at once be prevented by the anchorage coming into action, which however could only happen under the assumed circumstances of a wind pressure more than double that already mentioned, acting over the whole estimated surfaces. The maximum pressure on the base of the piers will be a little over 6 tons per square foot.

Forms of Parts.—The enormous forces to be resisted have been met by adopting the most suitable forms of parts for withstanding the stresses. Tubular members are used for compression, and open-braced box-forms for tension. These parts vary in size as required. Though the tubular form has scarcely been used in this country for bridge members since its employment by the late Mr. Brunel, no difficulty has arisen in connexion with its use; even the junctions are dealt with as readily as the generality of the work.

Masonry.—The masonry for the main piers, above the whinstone concrete filling of the caissons, consists of a casing of Aberdeen granite, inclosing and bonded into a hearting of Arbroath stone set in cement, and strengthened by three massive wrought-iron belts built into the stone-work. The deepest pier weighs about 20,000 tons. The remainder of the masonry of the piers and abutments is of a similar class, whinstone being largely used in the interiors.

Steel.—For the principal members of the superstructure subject exclusively to compression, the steel used has a tensile strength of from 34 to 37 tons per square inch, with at least 17 per cent. of elongation in a length of 8 inches; for the other parts 20 per cent. of elongation, with 30 to 33 tons tensile strength. The rivet steel has 25 per cent. elongation, and 26 to 28 tons tensile strength per square inch. The whole of the steel is manufactured by the Siemens process. No sheared edges or punched holes are permitted.

Work started.—No time was lost by the contractors in starting the work. The land was at once entered on; the old workshops were put in order, and the extensive range of offices, stores, workshops, and yards was commenced, which now cover fifty acres. Meanwhile the centre line of the bridge was fixed, and the position of the piers determined. The foundations of those on land were begun simultaneously with the building of temporary jetties for gaining access to the piers that had to be sunk below water-level. These jetties, which are still used for conveying the material, are in themselves no small work; the southern or Queensferry jetty extends 2200 feet from the shore, and is connected with the workshops by an incline worked by a rope driven by a stationary engine. In order that the operations might be carried on continuously day and night when needful, electric light installations, supplemented by lucigens, were laid

[1] Paper read by Mr. E. Malcolm Wood before the Institution of Mechanical Engineers, on Tuesday, August 2.

over the works and piers; and telephonic communication was established between the offices and all the centres of operations. The workshops and yards were rapidly completed, and furnished with tools, of which many are of a special and novel description. Ever since the commencement the work has progressed without interruption, and has gradually assumed the gigantic proportions of the present time. Over 3000 hands have been employed continuously for the last year; and during the present summer months the number has been increased to 3600. The majority find lodgings in the neighbourhood of the bridge; and the remainder make use of a special train service to and from Edinburgh, and a steamer to and from Leith, put on for their use night and morning.

Materials.—The materials for the permanent work have been obtained throughout from producers of repute: Aberdeen granite from Messrs. Fyfe; Portland cement from Messrs. Hilton and Anderson and Bazley White; Siemens steel from the Steel Company of Scotland and the Landore Steel Works. All the steel has been subjected to rigid examination, and has passed the ordeal of specified tests before leaving the makers' works; a few specimens showing its high quality are exhibited. The materials delivered up to the present time have included—

Granite	550,000 cubic feet.
Portland cement	21,000 tons.

The amount thus far erected has been—

Masonry in piers and abutments ...	129,500 cubic yards.
Steel in approaches and main spans	19,000 tons.
Steel for main spans, prepared ready for erection, about	20,000 tons.

By the time the first consignment of steel arrived, the shops were ready for the preparatory operations, and the whole establishment was rapidly organized to deal in the most complete manner with the work to be executed. Hydraulic power is freely used, from the extremely neat form of shop crane to the 2000-ton press for curving the tube-plates. With the exception of the main-pier caissons, made by Messrs. Arrol Brothers of Glasgow, and the superstructure of the approach spans by Messrs. P. and W. Maclellan of Glasgow, the whole of the work has been turned out of the shops at the bridge, their present capacity being an output of 1300 tons of finished work per month.

Shop Practice.—The procedure in the shops may be described as follows. The flat plates and bars are first straightened. The plates to be curved are heated to a uniform red heat in a gas furnace, and while red-hot are moulded in dies under hydraulic pressure to the required form, stacked and coated with ashes, and allowed to cool slowly and equally; any subsequent warping is taken out by placing them again in the press when cold, and giving them a final squeeze into the correct shape. The butts of the bars are cold sawn, and the edges and butts of the flat plates are planed in the usual manner. The ends of the curved plates are planed in a novel form of machine, in which the tool travels in a circular path readily adjusted to the radius of the curved plate. On completion of the planing, the plates are taken to the tube yard, and are built up round the longitudinal ribs and internal stiffening frames, which have previously been fitted together in moulds to the exact diameter required: so that the plating of the framing at once gives the tube its proper form. The plates are in 16-feet lengths, and break joint alternately over the stiffeners at 8-feet intervals. Means are adopted to keep the tubes in line while the rivet holes are pierced by a travelling annular drilling frame, which is mounted on wheels and carries a boiler and engine driving ten drills by cotton ropes. A pair of drills are attached to each bed; and as the beds can traverse the circumference of the tubes, while the drills can traverse the length of the beds, the whole outside of the tube is commanded, and the holes are completed with accuracy to insure their precise coincidence when the parts are rebuilt at the site. As fast as each section of 8 feet length is finished, the machines are propelled along the rails to take up a new position; they thus travel gradually in successive stages over the whole length laid down. The tee and trough-shaped parts are built together in the shops, and the holes are drilled by adjustable vertical and horizontal drills, fitted to a travelling carriage; the power is transmitted to the machines by ropes from the shop shafting. Numerous radial machines are also in use for the secondary parts. For dealing with special parts, many ingenious and somewhat novel workshop appliances have from time to time been brought into use, beyond those here mentioned. All the parts of the junctions are carefully fitted together in the yard in the exact positions they will relatively occupy in the bridge. After each member has been prepared, the pieces are painted, marked, and stored until required for erection.

Founding Piers.—With the founding of the piers below water commenced the more difficult part of the undertaking; but without any sensible delay the whole of the piers have been successfully sunk and completed. The foundations for piers in shallow water were put in either by tidal work or by open cofferdams, and the excavation was carried down to boulder clay or rock. Though these were of individual interest themselves, from the size and difficulties met with, they are dwarfed by the magnitude of the operations connected with the deep-water piers, of which those in the south group are embedded in the boulder clay in one case at 90 feet below mean water-level, while at Inchgarvie they rest on a level bench cut out of the sloping whinstone rock at a depth of 72 feet.

Caissons.—The caissons for all the deep piers are 70 feet in diameter at base; the cutting edges and shoe are of steel, and the upper parts of wrought-iron. They were first built on ways on the south shore, and were launched with sufficient ballast on board in the form of concrete to insure their stability while towed out to their berths at the end of the jetties, where guide piles and dolphins were used to place them in correct position. Temporary wrought-iron cofferdams were built upon the top of the caissons, timber working decks constructed, cranes and concrete mixers fixed, air-pressure connexions made good, and sinking operations commenced with a pressure in the excavating chamber sufficient to drive out the water; the air-compressing machinery was placed on the jetty alongside. The Inchgarvie caissons had to be equipped with all these fittings while moored to the south jetty, so as to be ready for work on arrival over their rocky bed.

The working chamber was illuminated by electric lights; and communication was effected with it through three shafts with air locks on the level of the upper deck. The two shafts for the skips bringing up the excavated material were constructed with horizontal sliding shutters, worked by hydraulic rams, in place of the usual swing doors. The winding drum for bringing up the skip from the working chamber was not in the lock itself, but driven by an engine outside. On arrival of the skip in the lock, the lower slide was shut to, and the blow-off cock opened for releasing the pressure, the top slide drawn back, and the hook of the discharging crane was coupled to the skip by hand. This direct and rapid method of transit for the excavated materials greatly facilitated the sinking; the whole operation from arrival of the skip in the lock to its removal lasted only about three-quarters of a minute in ordinary working, the sequence of the movements being automatically controlled by interlocking gear. The air locks in the third shaft for the men were constructed with a view of rapidly changing shifts, and had double chambers, each capable of holding seven men.

The silt overlying the harder deposit was expeditiously expelled from the working chamber by means of ejection pipes passing into water outside, the air-pressure being sufficient to blow out charges of silt and water mixed in a box which communicated by a valve with the ejection pipes. On reaching the boulder clay, portable steel diggers, actuated by hydraulic cylinders placed between the roof and the implements, were brought into use to break up this hard material.

At Inchgarvie a modification of this system was required for sinking the deep piers into the hard whinstone rock, which had a natural slope of 1 in $4\frac{1}{2}$. Bags of sand and concrete were deposited in two piles on the deeper side of the site to be occupied by the caisson, which had been launched with massive timber blocks in the chamber, to rest upon this artificial bed; these blocks and the edge of the caisson touching the rock on the shallower side were the first bearings it took when lowered at the site. The whole of these primary operations required extreme care to provide for differences of weight on the base, due to the depth of water at different states of the tide. Then by means of rock drills and ordinary quarrying operations inside the air-chamber the rock was excavated until the caisson was sunk to a level bench cut out of the sloping rock. In these caissons the full pressure of air due to the head of water was maintained during the sinking, and it was found advisable to change the gangs every four hours; the maximum pressure reached at high tide was 33 pounds per square inch above the atmosphere. The last of these caissons was got down to its final depth in October 1885.

In sinking the southern group of caissons, the air-pressure hardly ever exceeded 22 pounds per square inch, the silt and clay acting as a lute; and the working shifts were of six hours' duration, about twenty-seven men being down at a time.

Recovery of Canted Caisson.—With the exception of the north-west pier in the southern group, the whole of the piers were completed with regularity. But the caisson for that particular pier, weighing with concrete some 3000 tons, while ready at the site for placing in its final position, by some means became waterlogged on New Year's Day, 1885, and on the tide falling slid forwards on the mud about 15 feet, and canted over through 25°. After an ineffectual attempt to right it by pumping, regular siege was laid to it; but not until the autumn following, after nine months of incessant work, was a timber jacket or cofferdam completed, which enabled the pumps at last to obtain command over the leaks. The caisson then floated again, and after repair was sunk in position in the ordinary manner, arriving at its final depth in 1886. After the excavation had been completed, the chambers were rammed with concrete and grouted up, the concrete and anchorage and masonry were completed, and the temporary cofferdam was ready for removal.

Men Employed.—No difficulty arose in obtaining a sufficient number of men inured to work under air-pressure, as M. Coisseau, of the firm of MM. Couvreux and Hersent, of Paris, brought his staff of trained excavators from the Antwerp harbour improvement works, and contracted for the work to be executed under air-pressure.

Raising Viaduct Girders.—After the masonry of the approach viaduct piers had been carried up to a convenient height, a temporary stage was built, upon which the girders were erected and riveted up. Steel cross-beams with pairs of hydraulic jacks were placed under the ends of the girders over the piers; and a stage surrounding the piers was suspended from the main girders. From this platform the men in charge of the rams conducted the operations of lifting and blocking up the girders; and the masons afterwards completed the stonework in the vacant spaces. By this plan the girders were raised to their final height in July of the present year. The whole of the ten spans on the south side were lifted simultaneously as soon as they were all riveted up. The materials for the piers were first raised in trucks, by a steam hoist on the jetty, to a tramway laid on beams between the bottom members of the girders, and afterwards lowered into position by winches over each pier, these winches being driven by running ropes from engines on the girders at alternate piers. These approach spans now require only the parapet and a few other details for completing them in all respects, ready for the permanent way.

Erecting Steel Work over Main Piers.—On the completion of the masonry, the operation of erecting the steel work was commenced on the northern piers early in 1885 by riveting up the bed plates, and lowering them into position over the heads of the foundation bolts. Their surfaces were afterwards smoothed by emery wheels, and coated with crude petroleum, to prepare them for receiving the bearing plates of the cantilever bases or skewbacks. These, as already mentioned, have freedom for a limited amount of sliding, and the gauges at present attached show that the sliding movements follow the changes of temperature as anticipated.

The skewbacks, forming the junction of five tubular and five rectangular members over the piers, were then erected, and were connected with the horizontal members at the same level, which had been built together on a stage. After the connexions had been riveted up, a commencement was made upon the upper parts over the piers; these parts have since been erected without any form of fixed scaffolding, and the operation is still in progress over the Inchgarvie piers.

The lifting gear for raising the erecting platforms consists of a pair of plate frames, one below the other, fixed inside each 12-feet pier-column, by pins passing through the wings of the frames and the ribs of the column. The lower frame supports a hydraulic lifting press; and upon the ram rests a through box-girder cross-beam, at right angles to the length of the bridge, passing through voids in the columns, where plates are temporarily left out for this purpose. These cross-beams support lattice-girders in pairs, one on each side of the column, which extend a little more than the full length of the side of the quadrangle formed by the piers. Upon the top of all comes the main deck, furnished with gantries, cranes, oil-heated rivet-furnaces, &c., complete in all respects for carrying on the chief operations of erection. On the bottom level of the girders is a lower deck, with the ends housed in to form temporary shelters for the men. The box and other girders are built up of parts which will eventually be used in the permanent structure. Communication between the level of the jetty and the platforms is made by hoists, drawn up between wire-rope guides by the winding engine on the level of the jetty, which lifts the material by wire ropes to the platform; safety clutches are attached to each cage, for seizing the guide ropes in case the hauling gear were to give way. During lifting operations, access to the platforms is gained by ladders laid up the cross-bracing between the main columns over the piers.

The process of raising the platforms is as follows. Water-pressure at about 30 cwts. per square inch is conveyed from pumps on the jetty to the lifting presses by wrought-iron piping taken up the inside of the columns, and is turned into a cyinder, lifting the load off the series of pins in the top frame. The pins are then withdrawn, and the ram lifts the box-girder, carrying with it the loose frame, until opposite the next series of holes in the ribs of the columns, into which the pins are then inserted; the pressure is released, and the box-girder again rests upon the upper frame. In the return stroke the ram, hanging by its shoulders from the upper frame, by means of its piston form now hauls up the lower frame, from which the pins have been withdrawn; and when this has been repinned, it is ready to support the press for another upward stroke. By this means the platforms have been gradually raised, generally through lifts of 16 feet at a time, until arrived at the summit. On their way up they have been utilized for building the tubular cross-braces and other work; and at the present time those at the southern and northern piers form the stage for erecting the top members between the heads of the main columns. The platforms at Inchgarvie are now only 40 feet below the height to which they will have ultimately to be raised.

In building the pieces together, they are connected by service bolts, until the hydraulic riveters are brought into action. For the open work the riveters are of the gap type; but for the closed tubular work, a special adaptation was devised by Mr. Arrol, by which the rivets are closed in any part of the built tubes. When these machines arrive at the top of the columns, after having completed the riveting on the way up, they are taken apart ready for application elsewhere.

Erecting Cantilevers.—The building out of the first projecting bays of the cantilevers is being conducted on the system just described, with such modification as to suit the altered circumstances. The bottom members are first erected, and have been built by means of overhanging frames in panels, resting upon the completed portions of the tube, and so constructed that, as fast as the work is riveted up by the annular riveting machines, and the forward portions of the cage-like framing are brought into bearing, the back frames can be unshipped and taken forwards to the working face. Upon the top of this framework a movable hydraulic crane is placed for lifting the pieces into position, which are brought alongside from the pier by carriers suspended from a single rail of angle bar. As soon as the limit is reached at which these members can support the projecting work, inclined supporting stays are introduced, which connect the bottom member at this part with a temporary horizontal tie stretching between the main columns at about the level of the cross-bracing; thence the inclined stays slope down again, and are attached to the bottom member on the other side of the pier. After this has been done, platform girders with decks are built at a convenient level to rest on cross-beams carried by rising frames, which are introduced between the corners of the first vertical member of the bridge: this member having been pierced beforehand with a series of pin-holes, in readiness for a lifting action similar to that used in the main columns. The ends of the platforms nearest the piers are raised by suspension bars, by the action of hydraulic rams attached to the main columns at a higher level. From these platforms, as in the previous cases, the erection of all parts commanded by them is carried on as they rise.

The erection of the secondary parts proceeds simultaneously with that of the main members, the railway girders being built by corbelling out from the supports, and the other parts by light stages when the parts themselves cannot serve as a means of support to extend the work. As will readily be understood, the erection of these sections calls for greater nerve and judgment on the part of the men employed than does that of the portions previously described.

In conclusion, the author desires to express his indebtedness to Sir John Fowler and Mr. Benjamin Baker, through whose kindness he has been enabled to place before the Institution the foregoing particulars respecting an undertaking which, as shown by the magnitude of the works now being carried on, constitutes one of the greatest engineering feats ever attempted.

THE MACHINERY EMPLOYED AT THE FORTH BRIDGE WORKS.[1]

THE greater part of the machinery at the Forth Bridge works is original in design and novel in construction, chiefly because of the unusual nature of the work to be carried out. It may be roughly classed under the following heads: hydraulic bending and setting, planing, drilling, erecting, and riveting. In designing the machinery and tools to accomplish these different kinds of work, there had ever to be kept in view rapidity of production, with a very high quality of work in the finished structure. An idea of the quantity of machinery provided to deal with the material passing through the shops may be partly formed from the fact that it is capable of finishing 1500 tons in a single month.

Hydraulic Bending and Setting Machinery.—To bend and twist the large steel plates required in the construction of the tubes and their connexions, a great variety of hydraulic presses had to be provided. The largest of these is capable of exerting a pressure of 1600 tons between the dies. It consists of four 24-inch cylinders, resting on two longitudinal girders bedded in concrete. From each cylinder rise two iron colums, which carry a fixed table overhead. On the top of the rams another table is placed, which can be raised or lowered at will. Between these two tables are placed the blocks which stamp the plates to the desired shape. In most cases this shape is the arc of a circle, but in others the form is very varying, while in some instances the plates are flanged as well as bent or twisted. In nearly every case, after a plate has been set while heated, it requires to be finally adjusted when cooled. To dispense with the heating of the plates gives unsatisfactory work, and is in many cases impossible. In no instance is this plan of bending adopted to any extent without annealing the plates both before and after the work has been put upon them. Much of the final adjusting of the plates is done by presses consisting of a simple ram fixed to the upper of two girders, which are bound together at the ends, the lower girder serving as the seat for the block on which the plate is placed. Numerous other forms of presses are employed for lighter work.

Planing Machinery.—A special class of machinery is employed to plane the edges of the plates. In the case of most of the plates this requires to be done very carefully, because in the structure of the bridge a certain percentage of the stress in compression is taken up by the plates butting, instead of wholly by the rivets as in the tension joints. This statement applies to all plates in the tubes.

The sides are first of all planed on what may be looked upon as an ordinary planing machine. It is provided however with special double side-cheeks, between which are two fixed swivelling tool-boxes, one on each side of the machine. These tool-boxes can when desired be transferred to a special cross-slide, as it is sometimes more convenient to work with one box in the cross-slide rather than with both between the side-cheeks. Both tools act together and cut continuously—that is, during the backward as well as the forward travel of the table. The plate to be cut is fixed upon a curved block, which in turn is securely bolted to the table.

For planing the ends of the curved plates a special machine had to be designed and built, in which the plates are secured to a fixed table, while the tool is made to travel backwards and forwards in a swinging pendulum that receives its motion through a connecting-rod from a travelling saddle. The tool cuts both ways in this instance also, and is fed to its work by hand.

The planing machines employed to finish the rectangular plates for girder work are of the usual pattern for plate-edge planing, but with the addition of an end slide provided with a separate tool for planing one end of the plate at the same time that one of its sides is being similarly treated. This machine finishes a plate at two settings, with the certainty that the ends are at right angles to the sides.

In some machines two saddles are upon the main slide, and in others two tools are in one saddle; both devices have their advantages. The facing of the tees, angles, and other sections is done as a rule by cold steel saws, in order to secure good butting.

Drilling Machinery.—As will be inferred from the varying character of the work, the drilling is performed by various classes of machines. The principle kept in view is that, wherever possible, girders, tubes, &c., should be drilled only while their various parts are temporarily built and held together by bolts in the position they will finally occupy in the finished structure; in this way the highest class of work is obtained.

For drilling the tubes, the machines, each complete in itself, are made large enough to embrace the entire circumference of the tube. They consist of a wrought-iron under-frame or carriage, on which are placed the engine and boiler. On it are also fixed two large cast-iron annular rings or headstocks, embracing the tube, round which ten drilling slides and heads travel circumferentially. The slides are moved around the rings and consequently around the tubes by a worm at each end, gearing into a worm-wheel that forms part of the rings. The motion of the drill-heads on the slides is longitudinal, or parallel to the tubes. These two motions easily permit of the ten drills working at any part of the circumference of the tube comprised between the two annular rings, which embrace a length of 8 feet. When this length is finished, the whole machine is travelled forwards, and is again ready to drill a new length of 8 feet. The tube rests on timber blocks, which are removed from the front and placed behind as the machine travels forwards. In the case of the lighter tubes, the rate of drilling is as high as 12 lineal feet of tube per shift of ten hours; this represents about 800 holes drilled

The booms of all girders are drilled separately on blocks, thus leaving the bracings to be drilled to template, which is done by radial drills at another time. The machines employed to drill the booms are of a wholly different kind from those used for the tubes. They are moved along rails, running on each side of the blocks upon which the booms are built, and parallel with them. They consist of a double carriage with upright columns, connected together by means of a cross-beam and sundry other framing for carrying the shafts, pulleys, &c. To the columns and cross-beam are secured slides, to which the fixed drill-heads are bolted on the front of the machine; while to the back are attached radiating arms, each carrying a single drill. In this way there are both fixed and swinging drills on the two sides of the machine, capable of drilling holes in either a horizontal or a vertical plane. The fixed drills serve for all holes in the regular pitch, while the movable drills take what may be called odd holes, such as those where the struts and ties are to be secured to the booms. All the fixed drills are self-feeding, but the movable ones are fed by hand. The number of drills simultaneously at work varies greatly; at times as many as thirteen have been employed together on a single boom.

Other machines having radials with only single drills are used for a special class of drilling, and are found to work to great advantage. With the exception of a few special tools, all the remaining drilling is done by radials capable of making a complete circle round the column on which they are supported. Tables are placed on each side of these machines, and the work is fixed on one of the tables; and as the drills are placed at a convenient distance from one another, all the drilling required is easily accomplished without a second shifting of the work.

Erecting and Riveting Machinery.—To erect and rivet such large quantities of material at the immense height at which much of it requires to be done demands a large quantity of special plant for riveting and other purposes. The ordinary class of riveting is accomplished by means of small portable riveters, consisting of two arms held apart by links and stays; one arm acts as the holder-on, while the other carries the hydraulic cylinder for supplying the power, the cylinder and arm together forming one casting. For some of the more difficult work, where neither could this form of riveter be employed nor could the work be done by hand, small direct-acting hydraulic cylinders were used; the die for forming the rivet-head was here fixed into the piston. Two 4-inch cylinders were usually employed, held

[1] Paper read by Mr. William Arrol before the Institution of Mechanical Engineers on Tuesday, August 2.

to their work either by hard wood packing placed against the permanent structure, or by temporary girders brought into proper position. In these machines the pressure employed was 3 tons per square inch. A large amount of excellent work was performed by these machines in positions where it was practically impossible to do it otherwise.

The riveting of the vertical columns of the piers is done by riveting machines attached to the under sides of the lifting platforms. They are lifted with the platforms, and do their wo k while the platform is at rest. They consist of two longitudinal girders or uprights, one on the outside and the other on the inside of the column. Along the face of each girder a riveting cylinder is raised or lowered by hydraulic power. The inside girder has a trunnion at top and bottom, fitting into a step in two temporary diaphragms for supporting the thrust of the rams in riveting. It is turned round on the trunnions at will, so as to rivet up an entire length of 16 feet of the tube both circumferentially and longitudinally. The outside girder and riveting cylinder when at work always face the inside. The outside girder is attached at top and bottom to two wrought-iron rings, which encircle the column, and not only furnish the necessary support but also permit of the machine being moved round the column by hydraulic power as required. Over 800 rivets have been closed in a day by one of these machines.

In the erection of the large piers of the bridge, hydraulic power is utilized to a great extent. The principle adopted is to build the piers from off a platform raised by hydraulic pressure as the work of erection proceeds, utilizing the piers themselves in process of building as the support of the rising platform.

THE CHEMISTRY OF THE RARE EARTHS.

IT is now nearly twelve months since the chemical world was agitated by the memorable departure made by Mr. Crookes, in his address to the Chemical Section of the British Association, in attempting to translate into language thoughts which had been irresistibly forced home to the minds of many men of science as to the insufficiency of the theories of our modern chemical philosophy to account for the presence in our midst of those objects of ever-increasing interest, the chemical elements. It will be remembered that, both in the address referred to and in his lecture at the Royal Institution on the "Genesis of the Elements," Mr. Crookes based a large portion of his arguments upon the remarkable experiences which he himself had met with in endeavouring to separate the constituents of the rare earths contained in several sparsely distributed minerals. It may be of interest just to recall the main conclusions drawn by the lecturer from his experiments upon the substances yielded by the laborious but fruitful process of fractionation. Yttrium, which only two years ago was supposed to be a simple substance, fell under that *nil desperandum* sorting influence into five components, each of which presented a distinct phosphorescent spectrum; samarium, one of the constituents of old didymium, was found to consist of two and possibly of three ingredients; and finally, the two components of didymium itself, into which it had been separated by Dr. Auer von Welsbach, were shown by Mr. Crookes, M. de Boisbaudran, and M. Demarçay to consist themselves of several.

Contemporaneously with the work which has been carried on by these and other experimenters, Drs. Krüss and Nilson, who have at their disposal tolerably large quantities of Scandinavian minerals containing rare earths, have been engaged upon work of a similar nature, and have lately published in the *Berichte* of the German Chemical Society results of the highest interest, not only confirming the conclusions above referred to, but announcing that, "in place of the rare metals erbium, holmium, thulium, didymium, and samarium, we must now accept the existence of more than twenty elements."

Considering the interest which Mr. Crookes's addresses have called forth, and the important bearing of this contemporaneous work upon a subject of such paramount importance to the first principles of chemistry, it may be of advantage to give a short description of the experiments which have led to results of such magnitude.

The minerals examined were specimens of thorite from Brevig and Arendal, in the province of Christiansand, of wöhlerite from Brevig, cerite from Bastnäs, fergusonite from Arendal and Ytterby, and of euxenite from Hitterö and Arendal. The nitrates of the earths contained in these minerals gave very beautiful absorption-spectra, and a precise measurement of the positions of the lines and bands in these spectra resulted in the surprising observation that in certain minerals only a particular few of the absorption-bands of the nitrates of some of the rare earths were visible; thus, only one line out of all the lines considered to belong to the nitrate of holmium, the metal which Soret called X, was visible in any intensity in the spectrum of the nitrates from thorite of Brevig; moreover, this particular line is but insignificant, among many much more intense, in the usual spectrum of the nitrate of holmium. The more intense lines were either not at all or only faintly visible in the spectrum of thorite of Brevig; hence it is concluded that Soret's X must consist of at least two ingredients, of which one is found free in thorite of Brevig, and gives this one line of wave-length 428·7.

In these observations a single 60° prism of dispersion $A - H_2 = 4° 18'$ was preferred, inasmuch as weak lines or bands cannot be distinctly seen with more dispersion, and the position of maximum darkness becomes more difficult to fix; the spectroscope was fitted with the most refined micrometer arrangement for the accurate determination of the wave-lengths, so that the whole of the work may be checked by future observers. Before passing to the discussion of the main results of the experiments, a brief description of the procedure in case of one of the minerals examined may not be without interest. Thorite of Brevig is a typical specimen of the Scandinavian rare earth minerals, and its treatment was as follows. After removal of the thoria, which was required for the purpose of determining the atomic weight of thorium, the solution in ice-cold water of the sulphates of the mixed earths was precipitated by oxalic acid, leaving the iron, manganese, and uranium in solution. The oxalates were then ignited and the residual earths again converted to sulphates; the sulphates were converted to hydrates by precipitation with ammonia, and the hydrates dissolved in nitric acid, by which a lovely pink solution of the nitrates was obtained. As small quantitities of thorium and cerium were still contained in the mixture, the nitrate solution was evaporated to dryness, and the residue ignited, whereby the thorium and cerium nitrates were converted into insoluble basic salts. The filtered solution of the residue then contained the nitrates of the didymium and yttrium metals, and gave the following absorption-spectrum:—

Thorite of Brevig.

Observed position of max. darkness.		Previously observed wave-length.	For	Intensity of the absorption-bands.
Reading of micrometer.	Observed wave-length.			
2354	728·3	728·3	Di	Tolerably strong.
2381	708·2	708·2	Di	Very faint.
2411	686·0	684·0	Tm	Extremely faint.
2420	679·3	679·4	Di	Faint.
2457	654·1	654·7	Er	Faint.
2480	640·6	640·4	X	Very faint.
2505	626·1	626·1	Di	Faint.
2568	591·5	591·5	Di	Faint.
2596	579·2	579·2	Di	Tolerably faint.
2605	575·4	575·4	Di	Faint.
2689	539·6	...	?	Very faint.
2713	531·3	531·3	Di	Faint.
2721	529·2	530·0	Di	Extremely faint.
2740	523·6	523·1	Er	Strong.
2747	521·6	521·5	Di	Very faint.
2781	512·2	512·2	Di	Faint, broad.
2872	485·9	485·5	X	Very faint.
2888	482·3	482·0	Di	Very faint, but sharp.
2913	476·5	477·7	Sm	Faint.
2944	469·2	469·0	Di	Strong.
2974	462·3	463·2	Sm	Faint.
3068	445·6	445·1	Di	Very strong.
3076	444·2	444·7	Di	Very strong.
3164	428·7	428·5	X	Strong.
3240	417·3	416·7	Sm	Strong.

Similar observations were made upon the nitrates derived from the other minerals above mentioned, the actual wave-lengths being in every case determined so that the position of the lines can be open to no doubt whatever.

Ten years ago the erbium earths were considered as the oxide of a single element, but we now know that they consist of the oxides of scandium, ytterbium, thulium, erbium, terbium, holmium, and yttrium. Out of the rich data furnished by the present observations the observers believe they can prove that all those erbium earths whose nitrates give absorption-spectra are not oxides of simple bodies, but mixtures of the oxides of various new elements. Yttrium, as before mentioned, has already been shown by Mr. Crookes to consist of five constituents, and it will be interesting to see what light the workers in Stockholm have thrown upon the nature of some of the remainder.

M. Lecoq de Boisbaudran showed that by fractionation of Soret's X two new substances were arrived at, which he named holmium and dysprosium, but these are now shown to be themselves compound, for one part of dysprosium is not present in thorite of Brevig or cerite of Bastnäs, although present in the mixture called X; in fact, de Boisbaudran's dysprosium lines Dyα, Dyβ, and Dyδ are found to belong to three different elements; and the other constituent, the holmium of de Boisbaudran, is probably made up of no less than four distinct components.

As the introduction of fresh names is rapidly increasing the difficulty of work in this direction, Krüss and Nilson prefer to simply label the components by affixing the letters of the Greek alphabet to already accepted symbols. The metal called by Soret X is therefore constituted as follows:—

	Wave-length of characteristic line in absorption-spectrum of nitrate.
Xα	640·4
Xβ	542·6
Xγ	536·3
Xδ	485·5
Xϵ	474·5
Xζ	451·5
Xη	428·5

of which

Thorite of Brevig	contains...	Xα, Xδ, Xη.
,, Arendal	,, ...	Xβ, Xγ, Xϵ, Xζ, Xη.
Wöhlerite of Brevig	,, ...	Xγ, Xζ, Xη.
Cerite of Bastnäs	,, ...	Xα, Xη.
Fergusonite of Arendal	,, ...	Xβ, Xγ, Xδ, Xϵ, Xζ, Xη.
Fergusonite of Ytterby and euxenite of Arendal and Hitterö contain	... ...	Xα, Xβ, Xγ, Xδ, Xϵ, Xζ, Xη.

We are now accustomed to distinguish as erbium that body whose nitrate solution exhibits, in addition to a large number of lines in the violet and ultra-violet, two principal lines of wave-lengths 523·1 and 654·7 respectively, of which the former is the most intense. But in euxenite of Hitterö much greater difference is shown, one being extremely strong, while the other is barely visible; therefore here again the observers consider themselves in face of at least two elements, Erα and Erβ, one giving 523·1 and the other 654·7. Moreover, they have succeeded in separating the two almost completely by a method of fractionation similar to that employed by Mr. Crookes.

Cleve, in 1879, gave the name of thulium to the metal whose oxide formed the strongest base present in the mixture of erbium earths; and its salts, according to Thalèn, exhibit two absorption-bands, 684·0 and 465·0, of which the former is the most intense. Again, the variations are found to be too great for the supposition of a single earth to be tenable, one line being entirely absent in fergusonite and thorite of Arendal, while the other is strong; hence thulium must also consist of two ingredients, Tmα and Tmβ.

The observations with regard to didymium are all the more interesting as entirely confirming Mr. Crookes's statements, and Drs. Krüss and Nilson even go further in proving either that our interpretations of the indications of spectrum analysis are grossly wrong or that didymium is composed of not less than nine distinct elements. Dr. Auer von Welsbach's symbols for praseodymium and neodymium, the two constituents of didymium which he actually separated, are discarded, and the same nomenclature adopted as in the case of holmium.

	Characteristic line in absorption-spectrum of nitrate solution.
Diα	728·3
Diβ	679·6
Diγ	579·2 and 575·4
Diδ	521·5
Diϵ	512·2
Diζ	482·0
Diη	469·0
Diθ	445·1
Diι	444·7

The name samarium was given by M. de Boisbaudran to an element identical with Marignac's Yβ, an ingredient of the old didymium. The nitrate of this metal gives seven absorption-bands according to Thalèn, but it is surprising that in thorite and euxenite of Arendal the line 416·7 is tolerably strong, and even very strong, without another samarium line to be seen in the spectrum; the conclusion is inevitable that there must be in this substance a constituent whose nitrate gives the line 416·7, and to this the name Smα is given, all other samarium lines being provisionally supposed to belong to Smβ.

The main result of this splendid work, therefore, appears to be that, instead of holmium, erbium, thulium, didymium, and samarium, we must, if we follow Krüss and Nilson, recognize the existence of at least twenty-two new elements, the fate of some of which may be, in the near future, to be subjected to still further subdivision. If we add to these the results previously obtained by Mr. Crookes with respect to yttrium, the astounding revelation is presented to us that instead of six we find ourselves in face of twenty-seven, or a clear gain of at least twenty-one new elements.

But now comes the vital question—Are these really new elements, or are they only different molecular aggregations of the atoms of a few, as suggested by Mr. Crookes? It certainly seems very remarkable that so large a number of elements should be crowded together about the central series of the periodic system, and we appear to have a repetition of the same phenomenon, in a much intensified degree, as obtains in the cases of nickel and cobalt, rhodium, ruthenium, and palladium, and of iridium, osmium, and platinum. It may, however, be interestin this connexion to remember that this precise state of things was predicted by Mendelejeff himself (*Ann. Chem. Pharm.* Suppl. 8, p. 158), and in no way militates against the new element theory. Krüss and Nilson, rather than be obliged to have recourse to the introduction of new or auxiliary theories of spectrum analysis, prefer to rest upon the simpler and apparently more straightforward assumption that these substances, whose nitrate spectra show such marked differences, are indeed *bonâ fide* new elements. The accuracy of this view will doubtless be strongly contested, but in any case the result appears likely to be equally striking; for, if future work shows its want of accordance with facts, then an entirely new field of research has been opened, and the generally accepted ideas of the structure of matter must of necessity undergo a complete metamorphosis.

A. E. TUTTON.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 16.—"On Figures of Equilibrium of Rotating Masses of Fluid." By G. H. Darwin, M.A., LL.D., F.R.S., Fellow of Trinity College and Plumian Professor in the University of Cambridge.

The intention of this paper is, first, to investigate the forms which two masses of fluid assume when they revolve in close proximity about one another, without relative motion of their parts; and secondly, to obtain a representation of the single form of equilibrium which must exist when the two masses approach so near to one another as just to coalesce into a single mass.

When the two masses are far apart the solution of the problem is simply that of the equilibrium theory of the tides. Each mass may, as far as the action on the other is concerned, be treated as spherical. When they are brought nearer to one another this approximation ceases to be sufficient, and the departure from sphericity of each mass begins to exercise a sensible deforming influence on the other.

The actual figure assumed by either mass may be regarded as a deformation due to the influence of the other considered as a sphere, on which is superposed the sum of an infinite series of deformations of each due to the deformation of the other and of itself.

But each mass is deformed, not only by the tidal action of the other, but also by its own rotation about an axis perpendicular to its orbit. The departure from sphericity of either body due to rotation also exercises an influence on the other and on itself, and thus there arises another infinite series of deformations.

It is shown in the paper how the summations of these two kinds of reflected influences are to be made, by means of the solution of certain linear equations for finding three sets of coefficients.

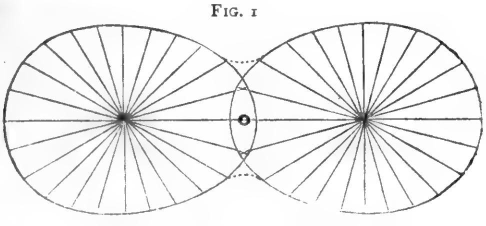
The first set of coefficients are augmenting factors, by which the tide of each order of harmonics is to be raised above the value which it would have if the perturbing mass were spherical. The second set correspond to one part of the rotational effect, and belong to terms of exactly the same form as the tidal terms, with which they ultimately fuse. The third set correspond to the rest of the rotational effect, and appertain to a different class of deformation, which are in fact sectorial harmonics of different orders. The term of the second order represents the ellipticity of the mass due to rotation, augmented, however, by mutual influence. All the terms of this class, except the second, are very small; their existence is, however, interesting.

From the consideration that the repulsion due to centrifugal force shall exactly balance the attraction between the two masses, the angular velocity of the system is found. It is greater than would be the case if the masses were spherical.

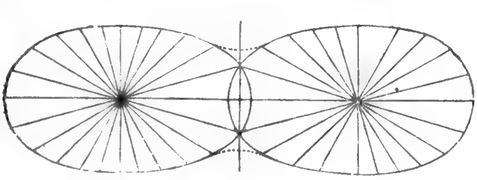
The theory here sketched is applied in the paper numerically, and illustrated graphically in several cases.

When the masses are equal to one another they are found to be shaped like flattened eggs, and the two small ends face one another. Two figures are given, in one of which the small ends nearly touch, and in the other where they actually cross. In the latter case, as two portions of matter cannot occupy the same space, the reality must consist of a single mass of fluid consisting of two bulbs joined by a neck, somewhat like a dumb-bell. In the figure conjectural lines are inserted to show how the overlapping of the masses must be replaced by the neck of fluid.

FIG. 1



Section perpendicular to axis of rotation.



Section through axis of rotation.

A comparison is also made between the Jacobian ellipsoid of equilibrium with three unequal axes and the dumb-bell. It appears that with the same moment of momentum the angular velocity is nearly the same in the two figures, but the kinetic energy is a little less in the dumb-bell. The intrinsic energy of the dumb-bell is, however, greater than that of the ellipsoid, so that the total energy of the dumb-bell is slightly greater than that of the ellipsoid.

Sir William Thomson has remarked on the "gap between the unstable Jacobian ellipsoid . . . . . and the case of the smallest moment of momentum consistent with stability in two equal detached portions." "The consideration," he says, "of how to fill up this gap with intermediate figures is a most attractive question, towards answering which we at present offer no contribution."[1] This paper is intended to be such a contribution, although an imperfect one.

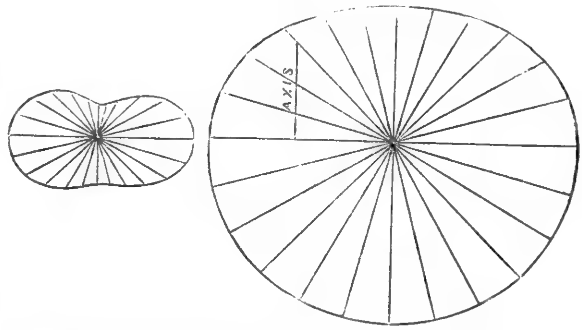
M. Poincaré has made an admirable investigation of the forms of equilibrium of a single rotating mass of fluid, and has specially considered the stability of Jacobi's ellipsoid.[2] He has shown by a difficult analytical process, that when the ellipsoid is moderately elongated, instability sets in by a furrowing of the ellipsoid along a line which lies in a plane perpendicular to the longest axis. It is, however, extremely remarkable that the furrow is not symmetrical with respect to the two ends, and there thus appears to be a tendency to form a dumb-bell with unequal bulbs.

M. Poincaré's work seemed so important that, although the figures above referred to were already drawn a year ago, this paper was kept back in order that an endeavour might be made to apply the principles enounced by him, concerning the stability of such systems. The attempt, which proved abortive on account of the imperfection of approximation of spherical harmonic analysis, is given in the appendix to the paper, because, notwithstanding its failure, it presents features of interest.

The calculations in this paper being made by means of spherical harmonic analysis, it is necessary to consider whether this approximate method has not been pushed too far in the computation of figures of equilibrium which depart considerably from spheres. A rough criterion of the applicability of the analysis is derived from a comparison between the two values of the ellipticity of an isolated revolutional ellipsoid of equilibrium as derived from the rigorous formula and from spherical harmonic analysis. As judged by this criterion, which is necessarily in some respects too severe, the figures drawn appear to present a fair approximation to accuracy.

Since, as above stated, the rigorous method of discussing the stability of the system fails, certain considerations are adduced which bear on the conditions under which there is a form of equilibrium consisting of two fluid masses in close proximity, and it appears that there cannot be such a form, unless the smaller of the two masses exceeds about one-thirtieth of the larger. It seemed therefore worth while to find to what results the analysis would lead when two masses, one of which is twenty-seven times as great as the other, are brought close together. As judged by this criterion the computed result must be very far from the truth, but as the criterion is too severe, it seemed worth while to give the figure. The smaller mass is found to be deeply furrowed in a plane parallel to the axis of

FIG. 2.



Ratio of masses 1 : 27. Upper half of figures section through axis of rotation. Lower half section perpendicular to axis.

rotation, so as to be shaped like a dumb-bell, and although this result can only be taken to represent the truth very roughly, yet it cannot be entirely explained by the imperfection of the analytical method employed. It appears then as if the smaller body were on the point of separating into two masses, in the same sort of way that the Jacobian ellipsoid may be traced through the dumb-bell shape until it becomes two masses.

M. Poincaré has commented in his paper on the possibility of the application of his results, so as to throw light on the genesis of a satellite according to the nebular hypothesis, and this

[1] Thomson and Tait, "Natural Philosophy" (1883), § 778 (i). He also remarks elsewhere that by thinning a Jacobian ellipsoid in the middle, we shall get a figure of the same moment of momentum and less kinetic energy.

[2] *Acta Math.* vii. 3 and 4, 1885.

nvestigation was undertaken with such an expectation. He remarks, however, that the conditions for a separation from a mass which is strongly concentrated at its centre, are necessarily very different from those which he has treated mathematically.

However, both his investigation and the considerations adduced here seem to show that, when a portion of the central body becomes detached through increasing angular velocity, the portion should bear a far larger ratio to the remainder than is observed in our satellites, as compared with their planets; and it is hardly probable that the heterogeneity of the central body can make so great a difference in the results as would be necessary if we are to make an application of these ideas.

It seems then at present necessary to suppose that after the birth of a satellite, if it takes place at all in this way, a series of changes occur which are still quite unknown.

Paris.

Academy of Sciences, August 1.—M. Janssen in the chair.—On the silicates of thorine, by MM. L. Troost and L. Ouvrard. It was lately shown by the authors that the study of the double phosphates formed by thorine and zircon with phosphoric acid and potassa or soda furnished no argument for associating thorine with zircon. Their further researches on the combinations of thorine and silica have yielded a compound substance, in which this base seems to be still further removed from zircon. The silicates of thorine were prepared by heating a mixture of silica and thorine with the chloride of calcium used as a solvent, and by varying the conditions two silicates were obtained, differing entirely in their composition and crystalline form. The crystals belong to the orthorhombic system, with density 6·82 at 16° C., analysis yielding 18·01 silica and 81·80 thorine. This compound corresponds to the formula $2ThO \cdot SiO_2$ ($Th = 58·1$), or $Th'O_2 \cdot SiO_2$ ($Th' = 116·2$). There is no isomorphism between this silicate of thorine and zircon $ZrO_2 \cdot SiO_2$; but here thorine may be regarded as playing the part of a bioxide. This conclusion has been confirmed by the recent experiments of MM. Krüss and Nilson, who, when determining the vapour-density of thorium, obtained numbers approaching, but always inferior to, that corresponding to the formula $Th'Cl_2$.—New fluorescences with well-marked spectral bands, by M. Lecoq de Boisbaudran. The new fluorescences here described are specially remarkable both for the number and the position of their distinct rays. They are often very bright, and are obtained by taking as agents the oxides of Sm, $Z\alpha$, $Z\beta$, and as solid solvents alumina or gallina. Alumina with 1/50 of samarine shows a red, an orange, and a green band, whose positions differ little from those occupied by bands obtained from the inversion of the induction-spark on a solution of chloride of samarium. The red is extremely weak, the orange more visible, the green easily distinguished, although less luminous than the orange.—Fluorescence of spinel, by M. Lecoq de Boisbaudran. The natural spinels give both a red fluorescence, whose spectrum has been carefully described by M. Edm. Becquerel, and also occasionally a greenish fluorescence. It is here shown that the former is due to the presence of chromium, the latter to that of manganese. By introducing 1/1000 of MnO into the composition of artificial spinel, the beautiful green fluorescence gives the same green band, but considerably more intense. By replacing the manganese with 1/100 of oxide of chromium, there is developed a magnificent red fluorescence presenting all the characters of that of the ordinary natural spinels.—Heat of formation of some crystallized tellurides, by M. Ch. Fabre. It is shown that several metallic tellurides may be obtained by heating in nitrogen a mixture of powdered tellurium and filings of the metal. The tellurides of iron, nickel, cobalt, and thallium not hitherto obtained, are crystallized, resisting hydrochloric acid and sulphuric acid at a low temperature, but slowly changing in a moist atmosphere. Reduced to a fine powder they are easily dissolved in bromine and the water of bromine yielding the corresponding bromide, hydrobromic acid, and tellurous acid. A comparison of the heats of formation of the crystallized tellurides and selenides seems to show that in the same group, according as the equivalent weight of the metalloid combined with the metal increases, the quantity of heat liberated by the combination diminishes. But in order to verify this hypothesis, it would be necessary to determine the heat of formation of the corresponding crystallized sulphides.—On the succinimidoacetic and camphorimidoacetic ethers, by MM. Alb. Haller and G. Arth. In order to obtain these ethers, the authors have employed the sodified derivatives of succinimide and camphorimide, the latter behaving like its analogues in the presenc the alkaline metals.—On a new isomere of benzine, by M. Griner. Besides the dipropargyle belonging to the fatty se discovered by M. L. Henry, the author has obtained anot isomere of benzine, which does not combine with ammonia cuprous chloride, and consequently is not acetylenic. simplest formula would seem to be $CH_2—C\equiv C—C\equiv C—C$ —Remarks in connexion with the observations of M. Grav on the preparation of the chromates of aniline and their appl tions, by MM. Ch. Girard and L. L'Hote. The authors rep that they were the first to isolate and study the bichromate aniline, a crystalline salt, of which they gave the formula chemical properties, and from which they have succeeded preparing certain colours such as mauvéine, pheno-safrani violaniline, &c.—On the effects of salting on pig's flesh affec by charbon, by M. F. Peuch. The experiments here descril show that even in thoroughly salted bacon the charbon is killed, but its virulence is destroyed.—On a new microbe det mining indigotic fermentation and the production of blue indi by M. E. Alvarez. The author's experiments show that ind is a product of fermentation determined by a special micr greatly resembling those of pneumonia and rhinosclerome, wh also have the power of setting up indigotic fermentation. T microbe of indigo also possesses pathologic properties determ ing either a passing local inflammation, or even rapid de with congestions of the visceræ and fibrine exudations.

BOOKS, PAMPHLETS, and SERIALS RECEIVE

A New Mode of Geometrical Demonstration, with Examples; D. Ma (Brown, Aberdeen).—Terra: A. A. Anderson (Reeves and Turner) Annales de l'Observatoire de Nice, tome ii. (Paris).—Food Adulte tion and its Detection: J. P. Battershall (Spon).—Electricity: W. Lard (Longmans).—British Dogs, Nos. 9 and 10: H. Dalziel (Gill).—Bees a Bee-keeping, vol. ii. parts 9, 10, 11: F. R. Cheshire (Gill).—McGill U versity Annual Calendar, Faculty of Medicine (Montreal).—On the Edu tion of Engineers: H. Dyer (Munro, Glasgow).—Hints to Meteorologi Observers, 2nd edition (W. Marriott (Stanford).—Archives Néerlandai des Sciences Exactes et Naturelles, xxi. (Harlem).—Brain, part xxxv (Macmillan).—Quarterly Journal of the Royal Meteorological Society, Ap (Stanford).—Meteorological Record, vol. vi. No. 24, vol. vii. No. 25 (Sta ford).—Annalen der Physik und Chemie, 1887, No. 8*b* (Leipzig).—Beiblät u den Annalen der Physik und Chemie, 1887, No. 7 (Leipzig).

CONTENTS. PAG

THURSDAY, AUGUST 18, 1887.

THE PHYSIOLOGY OF PLANTS.

Lectures on the Physiology of Plants. By Julius von Sachs. Translated by H. Marshall Ward, M.A., F.L.S. (Oxford: Clarendon Press, 1887.)

IT is significant of the progress which the science of botany has made in the last twelve years that the accepted text-book of Sachs, which was published in 1873 as a single volume of 850 pages, is now represented by three volumes with an aggregate of about 1900 pages. The anatomical treatment in the original text-book was condensed, and wanting in detail; it is now replaced by the comparative anatomy of De Bary. Book II. of the text-book, which dealt with special morphology, has been re-edited by Goebel as a separate work, a translation of which has recently appeared (see NATURE, vol. xxxv. p. 577). The physiological portion of the original text-book, entirely remodelled and re-written by its author, appeared in Germany in the form of the "Vorlesungen über Pflanzenphysiologie," a translation of which is the book now under review. In producing this as the last of the series of three volumes above mentioned, the Clarendon Press has completed an undertaking which must earn the heartiest thanks of English students of botany.

It is not improbable that the publication of this volume will mark a stage in the history of the science in this country. The period of dependence mainly on translated text-books has now been of some duration, and it is not to be expected or desired that translation should altogether cease. Nevertheless it is unsatisfactory to receive the bulk of our supply at second hand, and subject to those delays which necessarily attend translation. It is to be confidently hoped that a period of home production, which has already begun, will now ensue, and thus demonstrate at once the healthy growth of the science amongst us, and the fact that there is still alive that skill of exposition in which this country has not been deficient in the past.

Written in the lecture form, and in an easily-flowing style, which the translator has successfully reproduced, the book is aimed at, and should surely attract as well as satisfy, "students and cultivated readers." The first five lectures are devoted to organography, and writing as a physiologist, with the express purpose of preparing the way for a physiological treatment of the subject, the author has adopted a method of "physiological organography," protesting against that "purely formal morphology," which has been prevalent during the last thirty or forty years, and which he complains of as having left the physiological relations of organs entirely out of account.

In laying down his system of "physiological organography," the author ranges all organs in five categories: (1) root, (2) shoot, (3) sporangia and spores, (4) archegonia, (5) antheridia. It will be noted at once that the term shoot is used in a comprehensive sense including leaf and stem where these are distinguishable. The shoot as a whole is thus co-ordinated with the root, a method which commends itself physiologically as more suited to the time than the old distinction of stem, leaf, and root as co-ordinate categories. Secondly, it will be observed that the time-honoured attempt to recognize in the sporangium the result of metamorphosis of some part of the vegetative system, in fact, to define it as representing a metamorphosed leaf, pinna, &c., is abandoned, and Goebel's generalization that the sporangium is an organ of independent nature is accepted. The author then proceeds to apply his method of treatment to the vegetative organs. Referring, by way of illustration, to numerous plants, he distinguishes as *typical forms* of root or shoot those which "present the essential peculiarities in great perfection," he recognizes as *rudimentary* those parts of plants low in the scale, in which "the organic differentiation generally is not so far advanced as in the typical ones," and as *reduced forms* those in which it may be assumed "that, in consequence of special modes of life, more simply organized forms have again arisen from those more highly organized." Lastly, he designates as *metamorphosed forms* those "which have, it is true, been derived later from the typical ones, but which contribute to the greater perfection of the entire organism," such as flowers, tendrils, &c.

In the application of this system the rhizoids of mosses, of fern prothalli, and liverworts, the organs of attachment of various Algæ, and even the mycelium of Fungi, are designated "roots," while the term "shoot," including the distinctive parts of stem and leaf, where these are distinguishable, is applied indiscriminately to the aërial parts of vascular plants, mosses, Algæ, and even to the fructification of the Fungi. While accepting this method as throwing a certain light on the various forms of plants, when regarded from the physiological point of view, it cannot be too strongly impressed upon us that it is in no sense a substitute for the purely formal morphology. This is clearly stated by the author himself, when he says (p. 72) that his method "is by no means intended to exclude the purely formal comparison as it has hitherto been conducted under the name of morphology; its effect on the latter is only to be that of explaining and enlightening." While reading these admirably-written lectures, some whose bent is strongly physiological may think that pure morphology has had its day, and is effete, while the true and only point of view is the physiological; but it is not the author's object to teach this doctrine, and it is to be regretted that, in order to avoid any uncertainty of interpretation, a more distinct terminology was not introduced. The author, who draws a clear distinction between the morphological "member" and the physiological "organ," might well have devised a system of terms applicable exclusively in this physiological sense, and so have both cleared the way for his own views and have saved from risk of error those whose morphological sense is dull.

A concise exposition of the internal structure of the plant follows, the cellular character of most plants being contrasted with the structure of the Cœloblastæ, which Sachs has designated "non-cellular plants." This part, though illustrated by many of the familiar figures from the old text-book, has been entirely re-written in accordance with more recent researches. It is not merely a descriptive and comparative treatment; the physiological end is constantly kept in view, so that the first twelve lectures may be regarded as preparatory to the more purely physiological part which follows. After a short explanation of the external conditions of vegetable life, and of the physical properties of plant-tissues, the remainder of the book is assigned to physiology properly so called, and it is divided into four parts dealing respectively with nutrition, growth, irritability, and reproduction. It is impossible, within the limits of a short review, to give an adequate idea of this comprehensive work; but it may be stated at once that it is as a whole decidedly preferable to the physiological part of the old text-book, which it has replaced. Its superiority is based not solely on its more modern view and larger sphere of observation, but also on its more clear construction. The information it contains is more easily accessible to the student, and to this end the addition by Prof. Marshall Ward of a thorough working index will materially conduce.

It remains to mention certain points in the book which for various reasons will be of special interest to English readers. Sachs's views on the transfer of water in plants are well known from his other writings. Here he puts forward in a concise form his opinion that the transfer is effected through the substance of the lignified walls. Much has been written since the first publication of these lectures to shake confidence in Sachs's view, and a defence of his position against recent attacks would now be of greater interest than the plain statement of his own case which is here given. In the succeeding chapters, on the regulation of the stream of transpiration, and the consequent supply of salts in solution, and on the general nutrition of plants, there is little to demand detailed notice. The writing is clear, and works up the results of recent investigation in a very readable form.

In the next part, which treats of growth, there is much fresh material to interest English readers, the most notable being that in Lecture XXVII. Here Sachs gives a really masterly epitome of his researches on the arrangement of cells in embryonic tissues, reducing to a system what was before 1878 a chaos of isolated observations, and leading up to the important conclusion that "the mode of cell-division depends only upon the increase in volume, and the configuration of the growing organ," and further, that apical cells, where they occur, are merely to be regarded as gaps in the system of construction. After a series of seven lectures on irritability, the volume closes with a discussion of reproduction, both from the comparative and physiological points of view.

Regarded as a whole the book is certainly a remarkable one. Prof. Sachs is a man who does not undervalue his own work, and who has no fear of stating his own convictions; and this volume may fairly be taken as expressing his opinion on vegetable physiology in 1882. In this respect it will always be a valuable work, and will maintain an historic interest long after the actual views expressed in it are either superseded, or have passed out of the range of controversy.

F. O. B.

A DICTIONARY OF PHILOSOPHY.

A Dictionary of Philosophy in the Words of Philosophers. Edited, with an Introduction, by J. Radford Thomson, M.A., Professor of Philosophy in New College, London. (London: R. D. Dickinson, 1887.)

TO those who like to pick up information in a scrappy way, this volume will no doubt prove useful. Chancing, for example, on the word *realism*, and feeling somewhat hazy as to its exact meaning, the inquirin reader turns to his "Dictionary of Philosophy," ar under the head "Realism or Dualism" finds a stateme from Fleming of the theory "as generally held," ar short paragraphs descriptive of (1) Sir W. Hamilton natural realism, (2) Herbert Spencer's transfigured realisr (3) the reasoned realism of George H. Lewes, and (intuitive realism, McCosh. Still unsatisfied, he turns the "Theories of the Concept," and learns of the do trine of realism from Monck, Whately, and Mill; of i varieties (extreme realism and moderate realism) fro Ueberweg; of its origin from Ferrier, Maurice, ar Ueberweg; of its truth and error from Noah Porter ar Whately; and he is perhaps rather shocked, in conclusio to learn from Mill that it is "an abandoned doctrine."

An introduction (of 35 pages) has been written by t editor, "for the sake of beginners in philosophical studie with the view of affording to such readers a general su vey of the field of thought before them." We think t editor might have added, "and as an incentive to tu for explanations to the body of the work." We dou whether the beginner would gain much from a "Sket of the History of Philosophy" so short as that given the fourth part of the introduction. We quote, by wa of example, the description of post-Kantian Germa philosophy, with one sentence of which we are in co plete accord:—

"The course of philosophy in Germany since the tin of Kant has been very remarkable, but is very diffic thoroughly to trace. The following are, however, t chief developments:—(1) German idealism advanced wi very rapid strides. It is common to say that Fichte subjective idealism was followed by the objective idealis of Schelling, and that by the absolute idealism of Heg But such a description can convey no meaning to t ordinary reader. (2) In reaction from this tendency w the modern German materialism, expounded by Mol schott, Vogt, and Büchner—a modification of the ancie atomism, according to conceptions of modern scienc (3) A development of one side of Kant's philosophy w the pessimism of Schopenhauer and Von Hartman According to the former of these, the absolute existen which Kant held to be unknown is will, whilst the latt lays the greatest stress on the unconscious. The thinkers are, however, better known for their theory human life, of which both take a gloomy and desponde view. (4) Herbart by no means accompanied the pr gress of post-Kantian idealists; he is characterized l Schwegler as 'extending the monadology of Leibnit (5) Ulrici and Lotze may be taken as examples of Germa philosophers who hold by the spiritual interpretation human nature."

The arrangement of the body of the work is as fo lows:—Two preliminary sections are devoted respective to (1) "Designations, Definitions, and Divisions," ar (2) "The Mind." In the latter are subdivisions ((1) mind, (2) the intellect, (3) faculties of the intelle (4) personality and the ego, (5) the nature of man, ar (6) consciousness. Then follow four main division (A) the psychology and philosophy of cognition, inclu ing three sections on ancient, mediæval, and mode schools; (B) the psychology and philosophy of feelir with paragraphs on æsthetics; (C) the psychology a philosophy of the will, with a section on free-will a determinism; and (D) moral philosophy of ethics, w a concluding section on the immortality of man. T sections on cognition do not seem to be very happily arranged; but a double index—an index of names and an index of subjects—renders it easy to make use of the volume as a dictionary. It would have been well, however, if a synoptical table of contents had also been added.

Turning now to one or two points of more special interest to the man of science, we think that the promise in the preface that "a fair representation has been secured of the teaching of the physiological and evolutional psychologists of our own time," is by no means fully redeemed. Barely a page and a half is devoted to "The Brain and Nervous System." The page on "Sensibility and Muscularity" is not very satisfactory; while the information conveyed in the three pages or so devoted to "The Five Senses" is sufficiently meagre. Such observations as Goldscheider's on "pressure-spots" and "temperature-spots" are not alluded to. We have come across no mention of Lotze's theory of local signs. But it would be easier to enumerate the few elementary points that are mentioned than the many important generalizations that are ignored.

Looking up *evolution* under "Modern Philosophical Schools," we find Mr. Herbert Spencer's well-worn definition preceded by that given by Mr. Sully in his article in the "Encyclopædia Britannica," an extract happily chosen. Two or three paragraphs on mental evolution from "The Principles of Psychology" are then cited. Mr. Sully's criticism of the Spencerian position is succeeded by Mr. Stirling's sweeping and not very acute criticism of the evolution theory in general. A paragraph from Mr. Fiske, on evolutional religion, concludes the two pages and a quarter devoted to this subject. There are indeed other incidental quotations, but we cannot say that the doctrine of evolution is adequately represented.

Nothing, however, is easier than to find fault with the execution of a work of this kind. We trust the labours of the editor and of the "collator of experience" have not been expended in vain. There are in this "Dictionary" a great number of well-selected passages from philosophers of all shades of opinion; and there must be many men with but little leisure for philosophic study who will be glad to make or to renew acquaintance with the thoughts and the speculations here presented.

C. LL. M.

OUR BOOK SHELF.

Hay Fever and Paroxysmal Sneezing. By Morell Mackenzie, M.D. Fourth Edition. Pp. 96. (London: J. and A. Churchill, 1887.)

PERHAPS none of the minor ills to which humanity is prone has given rise to so much discussion as the subject under review. We have the views of those who regard it as a complaint due to "pollen"; of those, again, who look upon it as a neurosis, in which the much maligned and little understood "sympathetic system" is considered to play the chief part; and of others who attribute this and kindred disorders to the hurtful consequences of the presence of swellings, exostoses, bony ridges, &c., in the nasal cavities. The latter school relies on a mode of treatment which in its endeavours to clear the nose of all so-called obstructions, by the free use of the saw, the drill, the gouge, the dental engine and electric motor, &c., reminds one more of the efforts of a mechanic, anxious to bring the nasal cavities into comparison with a polished eburnated cylinder, than of the intelligent practitioner. This kind of thing is being carried to excess, and an earnest protest must be made against the officious and meddlesome surgery of the nasal passages which is advocated amongst a certain class of modern specialists.

It is an old idea that hay-fever is produced in persons having a certain nervous erethism, or predisposition, by the contact of the pollen of certain flowering grasses with some portion of the upper respiratory tract, or the conjunctiva. Dr. Mackenzie is an advocate of this view, and he regards the action of this pollen as more dependent upon its "vital, than chemical or physical characteristics." Those grains with the longest pollen-tubes (Liliaceæ) are less irritating than the pollen of Graminaceæ, the pollen-tubes of which are quite rudimentary. Pollen rubbed into the noses of hay-fever patients is exceedingly irritating, and is more active than alum or tannin. Dr. Mackenzie thinks that the absence of vibrissæ, or want of mobility of the alæ nasi, or dryness of the mucous membrane, leads to the entry of pollen into the nasal cavities. Many interesting facts are referred to in this book which substantiate the author's views; and it is difficult to come to any other conclusion, in the face of such an able exposition, than that, whatever may be the condition of the sympathetic or central nervous system, which in a word constitutes the necessary "predisposition," the introduction of pollen into the eyes, nose, or throat, is necessary for the production of "hay-fever." Some interesting experiments are related by Dr. Mackenzie on dredging the atmosphere during the hay-fever season, with the object of counting the pollen-grains floating in the air. While these were enormously increased during the month of June when hay-making was general, and diminished during July in the country, even the air of the streets of London was only on one or two days during this season found to be free from pollen-granules. Thus persons, even in the heart of a large town, are not free from this external irritant.

The section on paroxysmal sneezing is very good. The author regards the affection as one of the respiratory centre, the afferent impulse of which is conveyed by the trigeminal nerve-fibres. Dr. Mackenzie rightly condemns much of the unscientific jargon written about the power of isolated ganglia, such as Meckel's ganglion, to be directly concerned in these conditions, and justly refers the nervous mechanism to the cerebro-spinal centres, quoting at length Gaskell's recent researches on the sympathetic nervous system, on which, indeed, he founds his views. The author's ideas are set forth with great ability and moderation, and this book forms a valuable contribution to the discussion of this much vexed subject. The treatment of these complaints is fully dealt with in the book.

The Owens College Course of Practical Organic Chemistry. By Julius B. Cohen, Ph.D., F.C.S. (London: Macmillan and Co., 1887.)

WHATEVER may be the failings of this little book, there is no doubt it is a step in the right direction—that of making what is called organic chemistry really a practical study, as is the case with inorganic. The introducers of the author, in a short preface, seem to imply that the practical study of organic chemistry should of necessity be connected with, and indeed lead up to, research. Now, however desirable it may be that a considerable number of people should do organic research, there are a great many cases where the student of chemistry will gain as much as will be useful to him by simply making some careful preparations, just as is done with ordinary quantitative analysis, with no intention of making analysis a profession.

It has no doubt been a standing disgrace in this country that, up to within the last few years, organic chemistry has been a "*black-board*" subject in most, if not all, schools; and for this neglect there is little excuse, for a great number of most important experiments may be made without more expenditure than in the case of ordinary quantitative analysis.

A somewhat similar plan of work to the one in this little book has been followed for the last three or four years at the summer course of the Normal School of Science, and no doubt other Colleges where chemistry is a leading subject will have adopted some plan of practical organic instruction. The publication of this book will save some trouble to teachers in directing the preparations. The book is divided into two parts, and, curiously enough, what is generally considered the easier, viz. marsh-gas derivatives, are put in the second part. The author gives as his reason for this, that the selected examples offer fewer difficulties. That is a matter of opinion to some extent, and may depend on the course of lectures the student is hearing at the time.

In Part I., after the purification of alcohol, ether, benzene, and short descriptions of boiling-point determination and fractional distillation, we pass on to formation of benzene derivatives, commencing with bromobenzene, ethyl benzene, &c., to typical members of different families, ending with ethyl benzoate. The descriptions of process to be followed are short, but generally to the point, and are preceded in each case by references to the literature on the subject, which is a very valuable addition, and should be useful to beginners. The appendix, consisting of notes on the preparations, is very good, but would have been better placed, probably, in the text, or in connexion with the most typical substance of a group or family. As to the physical constants, melting and boiling points and specific gravity only are mentioned. Surely a great number of substances, the preparation of which is described, allow of their vapour-densities being taken by Victor Meyer's method? Beyond that there is little to complain of. The book is fairly well adapted for its ostensible purpose.

My Microscope. By a Quekett Club Man. (London: Roper and Drowley, 1887.)

IT is impossible to give in a small volume of some sixty pages a clear description of the microscope and the wonders it reveals. Still the author has managed to make his little essays interesting; and if there is not much depth in his work, he has perhaps written enough to induce some of those who are not already the possessors of a microscope to get one. It is surprising that he has not laid more stress on the advantages of a binocular over a monocular instrument.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Sun and Fire Symbolism.

THERE is a phase of sun and fire symbolism in our very midst which seems hitherto to have received but little attention, viz. the presence of such symbols as crests or in the coats-of-arms of many of the oldest noble families and landed gentry of the British Isles. We find them in the greatest numbers in the armorial bearings of our Scottish families, and those belonging to the most northern counties of England; probably for the same reasons that they are most numerous on objects which have been found in the northern portions of Scandinavia, *i.e.* that the light and warmth of the sun were naturally prized in such districts, and also because they have there survived longer, owing to the isolated position of the inhabitants depriving them of free intercourse with the outer world.

In a letter in NATURE (vol. xxxv. p. 558) headed "The Svastika both as Sun and Fire Symbol," I gave illustrations of some of the emblems of the sun and of the svastika as a fire symbol, and also alluded to the wheel as being in use in some countries to this day as a preservative against fire. A type of fire symbol exists in some parts of England at our very doors. In Gloucestershire and Herefordshire—possibly also in some of the other south-western counties of England—it is not an uncommon circumstance to see on the external walls of some of the older houses one or two pieces of iron in this form  and sometimes thus . It seems evident that they could not have added much support to the building, since they were bolted on to it at one point only—the centre.

A most interesting explanation of them was given a few years ago by an old servant of our family who died about five years ago; his age went with the century. He was a Gloucestershire man, and on being asked the reason of the  form of these irons, he replied "that they were made thus in order to protect the house from fire, as well as from falling down."

In the little village of Kingstone, in Herefordshire, it is still the custom for the people on the eve of May-day to take two short pieces of wood and nail them in this form  over the door of a house or a stable, removing the one of the previous year. On inquiry why this was done, the reply was, "To scare the witches or the evil spirits away."

In the crests and armorial bearings of many of our families we find at least three distinct forms of sun and fire symbolism.

(1) The *sun* in splendour.

(2) *Fire*, represented sometimes by a mountain in flames.

(3) The *sun* as a ring, or as a simple circle, the heraldic terms for this latter type being amulets (Collins's "Peerage of England," London, 1779) and annulets (Sir Bernard Burke's "Peerage, Baronetage, and Knightage," London, 1880).

I propose now to give examples of a few of the most typical of each kind.

Blount, Bt.—This family is of French extraction, and formerly Lords of Guisnes, in France; their crest is an armed foot in the sun. Motto, *Lux tua via mea*.

Blunt, Bt.—Probably originally the same family. These latter have for a crest the sun in glory, charged in the centre with an eye, issuing tears.

In the Earl of Clancarty's arms—the Trenches came from Poitou in 1575—on the first and third quarters is the sun in splendour, and in the centre an escutcheon with the coronet of a Marquis of the Netherlands, charged with a wheel with six spokes. (The wheel is still used as a preservative against fire, both in Holland and in Denmark.)

Musgrave, Bt., of Hayton, has, for his crest, two arms in armour embossed, sustaining the sun; so has also Musgrave, Bt., of Tourin, co. Waterford; and their arms are the same.

The rising sun and the sun in his splendour is also borne—

By the Marquis of Lothian, by the Earls of Stamford and Warrington, by Lords Polwarth and Hammond—Lord Polwarth's crest is a lady richly attired, holding a sun in her right hand and a half moon in her left; and it also forms the crest of Tyrwhitt, Bt., Fairbairn, Bt., the Earls of Antrim, Nicholson, Bt., where it is placed between two stars of eight points, and of many more families.

We find fire symbols in connexion with the sun in the armorial bearings of Macleod of Lewis. Their crest is the sun in splendour, and in their arms they have a mountain in flames on the first quarter and the three-legged Manx man in the second quarter; the motto belonging to this latter is *Quocunque jeceris stabit.*[1] The Duchess of Sutherland (Countess of Cromartie) bears this symbol in her arms for Macleod, in the first and third quarters.

The crest of the Earls of Seafield is a salamander in flames; the Marquis of Hertford has a phœnix in flames out of a ducal coronet; Mackenzie of Scatwell, co. Ross, has on the second quarter of his arms a rock in flames, and on the third quarter three legs of a man armed, for Macleod of Lewis; Lord Ongley, a phœnix in flames holding in its beak a fire-ball; Verney, Bt., a demi-phœnix in flames looking at the rays of the sun; and Carmichael, Bt., has in the second and third quarters of his arms, a cup with flames of fire issuing therefrom.

We will now turn to the third portion of our subject—the sun as a ring, or a simple circle—bearing in mind that the former type is in heraldry called *amulets* or *annulets*, and that the simple circle is styled a *bezant*.

The Earl of Lonsdale has in his coat-of-arms six annulets forming a triangle; the Earl of Bantry one annulet in the first, and ten bezants in the fourth quarter of his coat-of-arms; Barron, Bt., on a Saltier gu., five annulets. Amory Bagge, and Bailey, Bts., bear also annulets.

In the possession of a member of the writer's family is a seal of rock crystal, on which is the motto *Luceo non uro*;[2] beneath this is a baron's coronet (for the Barony of Strange, which came to the Dukes of Athole through the female line), and below this, again, the sun in glory. It is believed (but none are now living who know this for a fact) that this seal formerly belonged to Marjory, second wife of the fourth Duke of Athole, eldest daughter of James, sixteenth Lord Forbes, and widow of John Lord Macleod, who died s.p. in 1789; she married the Duke in 1794, and died in 1842, having had by him a son and a daughter who predeceased her.

The Isle of Man belonged at one time to the Macleods—when, is apparently not known—but in 1405-6 it came into the possession of the Stanleys (afterwards Earls of Derby), through Sir John Stanley, Kt., who in conjunction with Roger Leke received a commission to seize the city of York and its liberties, and also the Isle of Man upon its forfeiture by Henry Percy, Earl of Northumberland. The Stanleys held the Isle of Man until the death of Ferdinando, the fifth Earl, without male heirs, when the Barony of Strange—which title had been borne by the second Earl, who died in 1522—fell into abeyance between his daughters, and the earldom went to his brother William, sixth Earl, who bought from his nieces their claims on the Isle of Man. His son, again, the seventh Earl, was summoned to Parliament in 1627 as Baron Strange, under the impression that his father had enjoyed it; this was, however, not the case, and the summons was virtually a new peerage, which eventually devolved upon the ducal house of Athole, through the marriage of Amelia Anna Sophia, youngest daughter of the seventh Earl of Derby, by his wife Charlotte de la Trémoille, daughter of the Duc de Thours in France, with John, second Earl and first Marquis of Athole. Failing male heirs to her father, Charlotte, daughter of the second Duke of Athole, became Baroness Strange on his death in 1764, and also succeeded to his rights in the Isle of Man. She married her cousin, John Murray, who became the third Duke.

Another seal, also in the writer's family, has an impression which differs essentially from the armed legs of the Isle of Man. It is known to have been the private seal of the fourth Duke. The Manx emblem correctly described is "the three legs of a man armed ppr. conjoined in the centre at the upper part of the thighs, placed in triangle garnished and spurred *or*": but on this seal the three legs are bare, and conjoined in the centre by a sun with rays. In fact, it is the trinacria of Sicily.

HARRIET G. M. MURRAY-AYNSLEY.

[1] Signifying, "However you throw me I stand." This is true of the svastika, a fire symbol likewise.

[2] "I give light, but I do not burn."

Bishop's Ring.—The Sky-coloured Clouds.

DURING a recent visit to the Alps I carefully looked for Bishop's ring, and found that it was generally visible at high altitudes in the middle of the day when the sky was clear. On the whole, the higher one ascended, the plainer it was, up to a height of 9000 feet, beyond which I did not go. On one occasion it was visible nearly or quite as low down as Chamounix, altitude 3400 feet; but this was the lowest point I saw it from. It was always extremely faint, so much so that if I had not seen it previously in its greater brightness I should not have noticed it at all,—indeed, it could usually only be detected by a careful comparison of the colour of different parts of the sky. Its dimension seems the same as heretofore.

About sunrise and sunset this circle continues occasionally conspicuously visible here, but it is long since I saw it in the middle of the day, when the sky has been really clear; sometimes, however, I have seen a similar circle, but with much duller colours and extremely feeble, giving one the impression that it was lower in the atmosphere than Bishop's ring as caused by the volcanic dust, and that it might be caused by smoke.

Last evening, and still more this morning, there was a bright display of the "sky-coloured clouds" (if I may so call them). I seldom or never saw them more brilliant than they were this morning, when I observed a circumstance as regards their colour that I have not noticed before: the greater part of them was coloured as usual, the lower part a dull yellowish green, and the upper part a bright, though light, blue; but there was a border of yellow above the blue, very faintly lit up it is true, but unmistakable. The display was almost confined to that part of the horizon between north-north-east and north-north-west, the cloud-mass evidently not extending further east or west to any extent, and the upper border after 2 a.m., at least, was evidently the actual southern edge of the cloud sheet.

I had the opportunity of watching these clouds gradually fade away in the increasing daylight, showing that in all probability they did not evaporate, but simply became invisible owing to the increasing light of the sky, and perhaps also to their losing light themselves. It is again evident, therefore, that they are of an exceedingly filmy and transparent nature, and indeed can hardly be considered real clouds at all.

Their motion was very slow, but appeared on the whole much as usual—viz. from a north-easterly direction.

Sunderland, July 30. T. W. BACKHOUSE.

The Electricity of the Contact of Gases with Liquids.

SINCE the delivery of Helmholtz's famous Faraday Lecture, "the charge on the atom" has been assumed by physicists, notwithstanding the very serious objections urged by Maxwell against such a theory. A re-perusal of the latter, some eighteen months ago, excited me to make some experiments on the subject. It occurred to me that by allowing such solutions as potassic iodide and chlorine water to react in an insulated vessel some information might be obtained as to the equality or inequality of the atomic charges.

My object at present is not to give an account of the many experiments of this kind which I made, but briefly to call attention to one result to which they led, and I shall describe only such experiments as are necessary for this purpose.

One of the electrodes of a quadrant electrometer was "put to earth," the other was connected to an insulated stand on which was placed a porcelain dish containing a small quantity of distilled water. The instrument was in a rather sensitive state. A high-resistance Daniell, through which a current never passed, gave a deflection of 94 divisions either way. A small fragment of potassium was cast on the water. The spot went rapidly to the left, indicating a negative charge on the porcelain dish, and a positive charge on the escaping hydrogen. A second fragment of K was thrown on the liquid in the insulated dish. The spot moved 28 divisions to the left, then turned and went up the scale to the right 300 divisions. A third piece of K was thrown on the liquid in the insulated basin, and the spot moved 40 divisions to the right. This behaviour was extremely perplexing. The connexions were looked up, and the experiment repeated with like irregular results.

Na was used instead of K, and although the deflections then obtained were also irregular, the tabulated results showed a contrast. When Na was used, 40 per cent. of the deflections were *first* to the left; when potassium was used, 70 per cent. of the deflections were *first* to the left. Speaking broadly, this seemed to indicate that with K the hydrogen came off charged positively, with the sodium it came off charged negatively; and as I thought that such a result would throw some light on the atomic charges I tried very hard to eliminate what I then believed to be accidental exceptions, and to prove that in reality such was the case. But I tried in vain.

Retaining the same method of testing the electrification, other combinations, such as H_2SO_4 and Na, H_2SO_4 + Zn, HNO_3 and K, &c., were tried, and in most cases irregular deflections such as those above described were obtained. Ultimately I got two constant and definite results: (1) Na thrown on strong and pure acetic acid invariably left a positive charge on the insulated dish, the escaping hydrogen being negative; (2) a fragment of zinc thrown into strong HCl invariably left a negative charge on the insulated dish, the escaping hydrogen being positive.

This last is a gross and unmistakable result. In fact its very magnitude was for some time a source of embarrassment. I shall not stop to describe the steps by which the next experiment was reached, but shall proceed at once to describe it; and I shall venture to give it somewhat in detail, as the title at the head of the paper is mainly founded on it.

The electrometer was not in a very sensitive state. The high-resistance Daniell aforesaid gave a deflection of 38 divisions on either side. A glass beaker $7\frac{1}{2}$ inches high and 5 inches in diameter was placed on the insulated stand. A porcelain dish, $2\frac{3}{4}$ inches in diameter and $1\frac{1}{8}$ inches high, was nearly filled with a 10 per cent. solution in distilled water of strong HCl, and placed at the bottom of the glass beaker just described. The insulated stand was now connected to one pair of quadrants, the other pair were put to earth. The "spot" stood at 378 on the scale. Three small fragments of granulated zinc were now dropped into the dilute HCl in the insulated dish. A very slight effervescence at once appeared. This gradually increased but never became violent. No trace of spray could be detected at the end of the experiment above the lower half of the beaker. In 4 minutes from dropping the zinc the spot could be perceived moving, and in $4\frac{1}{2}$ minutes more it moved 28 divisions to the left, indicating the charge on the dish negative and the escaping hydrogen positive. The insulated stand, &c., was now disconnected from the quadrants. The spot maintained its position on the scale. In $1\frac{1}{2}$ minute after, the quadrants were again connected to the insulated stand: the spot moved instantly 20 divisions more to the left. In $1\frac{1}{2}$ minute more it had moved 10 divisions further to the left, but with a slower pace, and it presently stopped and turned back, at first slowly, taking 5 minutes to go back the 68 divisions to the zero. In 4 minutes more it had moved 80 divisions to the right. The insulated stand was once more disconnected from the quadrants, and at the end of 2 minutes they were re-connected, when the spot instantly bounded up 55 divisions further to the right. It continued to move in the same direction until the effervescence ceased owing to the acid being exhausted. A quantity of the zinc survived. On short-circuiting the quadrants the spot returned to within 4 divisions of the original zero.

As the reaction between zinc and hydrochloric acid proceeds, the quantity of chloride of zinc in solution continually increases, and so it appears demonstrated that when hydrogen passes through hydrochloric acid it acquires a positive charge, when it passes through chloride of zinc it acquires a negative charge. I believe that this inference may be safely very much generalized, but for the present I forbear. In confirmation of it, however, it may be well to mention that at any stage of the last experiment a deflection may be obtained to right or left as required by adding an excess of saturated chloride of zinc (for the first), or of hydrochloric acid (for the second).

When it is known that the sign of the charge on escaping hydrogen depends upon the substance it has been in contact with, the very irregular results with K and Na already mentioned become less mysterious.

J. ENRIGHT.

Newton's Laws of Motion.

THERE is a point in connection with Newton's laws of motion which the text-books on dynamics, which found the science upon those laws, seem to me to leave very inconveniently and unnecessarily mysterious. The point to which I allude is the meaning of the words "rest or uniform motion in a straight line" in the first law. The difficult words are "uniform" and "straight," which of course are each of them meaningless until it is explained what the motion is with reference to; but this explanation is not given explicitly in any of the books on dynamics which I am acquainted with; and a comparison of their various statements leaves me in some doubt as to what is intended to be implied. May I therefore appeal to those of your readers who accept Newton's laws to say whether the following is correct?

I find that Law III. is interpreted by the most influential authorities, such as Maxwell and Tait, to mean that force occurs only as one side of a mutual action, consisting of two equal and opposite forces between two portions of matter. I am therefore led to suppose that the freedom from force action, which is spoken of in Law I., should be explained (by means of Law III.) as meaning isolation from the influence of all other matter; and that Law I. must be considered as containing a definition of an arbitrary meaning to be given in dynamics to the words "rest or uniform motion in a straight line," namely, that it is the motion possessed by any particle isolated from the influence of all other matter, which influence is to be traced by its mutual character. Law I. would then go on to say, as an experimental result, that all isolated particles move with reference to one another in a way consistent with this definition.

In order to reach this conclusion I find it necessary to interpret some statements in text-books in a somewhat awkward fashion (*e.g.* Maxwell, "Matter and Motion," article xl.), and to suppose some others to be incorrect; hence my doubts, and my appeal for their resolution.

W.

August 9.

On the Constant P in Observations of Terrestrial Magnetism.

ON page 304 of vol. ii. of their excellent treatise on "Practical Physics," Messrs. Stewart and Gee give the usual expression for the constant depending upon the distribution of magnetism in a pair of magnets employed for measuring terrestrial horizontal force; namely—

$$P = \frac{A - A'}{\frac{A}{r^2} - \frac{A'}{r_1^2}}.$$

Instead of this awkward and troublesome form, I would suggest

$$P = \frac{r_1^2 r^2}{r_1^2 - r^2}\left(1 - \frac{A'}{A}\right),$$

which can be readily deduced from Gauss's original equations, and is much better adapted to logarithmic computation; especially when r and r_1 remain constant throughout a series of observations, and Gaussian logarithms are used to form the factor $(1 - A'/A)$.

WM. HARKNESS.

Washington, D.C., August 1.

The Stature of the Human Race.

IN your "Notes" of last issue, p. 348, you mention General Pitt-Rivers conducting a party of the Royal Archæological Institute to Woodcutts, where skeletons dug out show that the people who inhabited the ancient Romano-British village were of very inferior stature, the males being only on an average 5 feet 2 inches, and the females 4 feet 10 inches. I think it would be a very interesting inquiry to ascertain the average height of the human race in the past, as it appears to me from such data as I have been able to collect that the human race has continuously increased in average stature. I have measured a great many Roman coffins, where I happened to come across them, and my average shows that the Roman could not have greatly exceeded 5 feet 5 inches. In taking measurements of ancient armour, I find that the English aristocracy have decidedly increased in average height within 500 years. For a paper I read before our local Society, I measured twenty-five mummies in the British Museum as nearly as I could through the cases, making estimate for wrapping, and I found the average height of males 61 inches, females 55 inches. The mummy of the celebrated Cleopatra measures about 54 inches, about the height of the present average European girl of 13. The most ancient mummy of an Egyptian king yet discovered measured 52 inches. With research I have no doubt interesting data could be obtained on this subject. Limiting the matter to my own observations, I have formed the idea that the average stature of the human race increases at about the rate of 1·25 inches per 1000 years.

WM. F. STANLEY.

Cumberton, South Norwood, August 13.

A Spider allowing for the Force of Gravity.

THE manœuvres of the small hunting spider, so common o[n] the West Coast of Africa, are always attractive, and my intere[st] in them had been specially aroused by seeing a house-fly, which had previously narrowly escaped capture, swoop down on his mortal enemy and touch him on the back with his claws (as though twitting him on his failure), the spider apparently taking no notice whatever. On seeing, therefore, one of these spiders stalking a small moth on my wall in Cape Coast Castle, I devoted my attention to the operation.

After moving off several times the moth at length settled on the ceiling, and I thought the chase was over. The spider, however, followed on to the ceiling, and approaching within striking distance (about two inches) anchored his web; then moving round in a circle from the moth until he was about equi-distant from his anchor and his prey, he made his spring. He had evidently calculated how much loose web he would require to reach his prey, for when he fell (as was inevitable from the force of gravity) he was suspended in mid-air by the loose web. The spider regained the ceiling by his own web, having narrowly missed a good meal. C. B. LYSTER.

19 Waterloo Crescent, Dover, August 12.

The Lunar Eclipse of August 3.

IT would be interesting to know if the following phenomenon was observed at other places. At 9.30 p.m., local time, at Hamburg, a small cumulus cloud was observed a little distance below the moon, and the darkened part of the lunar surface was taken to be part of the cloud, from its upper edge being flattened. Ten minutes later the cloud had passed away, but the

Sketch of Lunar Eclipse of Wednesday, August 3, 1887 (as observed at Hamburg).

flattened appearance on the moon remained, and it was evident that the earth's shadow was distorted, as seen in the annexed sketch. Several persons noted the peculiarity, which was visible until about 10.30 p.m. in a very clear sky. H. H.

August 8.

BOTANY OF SAN DOMINGO.

THE vegetation of this, the largest of the West India Islands next to Cuba, has long been almost totally unknown to botanists. The absence of all but the scantiest data about its flora has made any general conclusions as to the main facts of the geographical distribution of plants in the West Indies very uncertain. It has usually been supposed that any attempt to explore any part of the island botanically would present almost insuperable difficulties. The following extracts from a letter from San Domingo received at Kew from Baron Eggers, who has laboured so assiduously in the investigation of West Indian botany, will be read therefore with much interest. W. T. THISELTON DYER.

Puerto Plata, Sto. Domingo, July 11, 1887.

I HAVE now been about three months in this island. I arrived in Samana on April 14, and the following day in this place. After having spent a couple of weeks in exploring the lower mountains here (2600 feet), I proceeded to Santiago, where again I spent some time in exploring the Vega Real and the Monte Christi range. From Santiago I went further into the interior to Jarabocon and the Valle de Constanga (3860 feet), from where I made an expedition up to the highest peaks I could find (Pico del Valle, 8680 feet), and which I succeeded in climbing, though with considerable hardship and fatigue. From this Sierra I returned to Santiago, and from thence to Puerto Plata, where I have latterly been exploring the region to the east towards the rivers Yasica and Jamao.

This, in short, is an outline of my travels here. I have been so far very fortunate, as I have succeeded in penetrating to regions where no European seems ever to have been before: my collections are very rich—about 1200 species—and my health has not suffered from the rather hard life here.

This island is, to a considerable extent, in a state of uncivilization: the roads are frightful, and hardly deserve that name; in fact, there is not one single good road in the whole island. You could hardly believe that the principal road from Santiago to Puerto Plata, on which the greater part of the traffic of the island goes, in the rainy season is impassable often for weeks. With regard to the vegetation, it does not strike me as being very luxuriant. It is much less so than I expected, and is certainly less luxuriant than that of Dominica.

The Cacti, which are a good criterion with respect to dryness of climate, are seen very frequently in the Vega by Santiago; higher up, the mountains in the interior are covered with pine forests to an immense extent. There the soil is gravelly and rather sterile. I found the pine growing from 600 feet up to the very highest peaks. The Sierra and Monte Christi, a coast range, consists of Tertiary limestone, and has no pines at all. But here you find also Cacti, Acacias, and Agaves not unfrequently. Palms are comparatively scarce—only about six or seven species are known (*Oreodoxa*, *Sabal*, *Thrinax*, *Euterpe*, and one called "Yarey" here, which I believe is a species of *Thrinax*), comparatively few *Orchideæ*, and no *Cycadeæ* at all. I believe in the south, near San Domingo, there is a *Zamia;* and, on the whole, the eastern part of the island is more moist, especially near Samana Bay and along the river.

Of remarkable plants I have found here a *Clavija*, which seems to be known only from Trinidad among the West India Islands, *Phyllocoryne jamaicensis*, a *Stanhopea* or *Lælia*, and several tree ferns. In the high mountains, of course, I found a greater number of interesting species: several *Tupæ*, two *Ericaceæ*, two Fuchsias, of which one has a most beautiful large pendulous flower, *Ranunculaceæ*, Ferns, *Loranthus*, and others which of course were all unknown there. The *Juglans cinerea* grows here at a height of about 1800 feet; I obtained a number of seeds.

Among *Coniferæ* I should especially mention a splendid *Taxodium*, the wood of which is dark red and very odorous. It is called Sabium here.

The *Cacti* are, no doubt, very rich and interesting, but as they require to be preserved in alcohol, and the means of transport are so very difficult, I have not made any collections of them this time. The beautiful *Rudolphia rosea* grows from the coast up to 4000 feet.

On the stems of the pines a number of curious Bromeliads are growing, none, however, very conspicuous; at about 1000 feet a bulbous Oxalis with white flowers is found, commonly among the pines in the sandy soil. A number of herbaceous *Synanthereæ* were found among grasses in the upper regions above 7000 feet.

The *Podocarpus* of Jamaica I did not see here at all. A number of beautiful *Echites* are found in the lowlands, as well as some striking Orchids (*Bletia*, *Læliopsis*); also two remarkable Coccolobas, the immense-leaved *C. macrophylla*, and another species with somewhat lesser leaves. The first-named has, as you know, large dark purple flower spikes of 2 feet long; the other, on the other hand, only short spikes with small white flowers. On these Coccolabas are found several nice Epidendrums.

The savannahs are frequent and extensive here, and afford a number of smaller plants of various descriptions.

In several parts of the island there are tracts of mahogany, which are cut for export.

The climate is generally cooler than in the smaller islands. I found the nights quite fresh. In the higher mountains, of course, it was quite cold at night. On the Pico del Valle I passed one night. We had a large fire blazing all night; in the morning, at 6 o'clock, the thermometer only showed 11° C.

Rivers and brooks are innumerable, but on account of the freshets and the violent current after rain, hardly any aquatic plants are seen, at least in this part of the island. I missed the beautiful *Pontedera* of Porto Rico.

I send you to-day, by mail, seeds of the only palm which I have been able to obtain, a species of *Euterpe*, which is common here above 1200 feet, and the fruit of which is much eaten by half-wild hogs. It is called "Manacla" here, and grows to a height of about 30 to 40 feet.

Towards the end of the year I propose continuing my explorations of the West Indies, having in view a further investigation of this island, especially of the east and south, and furthermore of the Bahamas (especially Andros) and the two islands of Tobago and Grenada, both of which, I believe, are very little explored. The northern part of Dominica is also still *terra incognita*, unless something has been done there since my visit in 1879 and 1880. This island is particularly interesting to me. I believe it is one of the most luxuriant of the West India Islands.

CONSTITUTIONAL FORMULÆ AND THE PROGRESS OF ORGANIC CHEMISTRY.

If the mere increase in the number of known facts were an accurate measure of the growth of a science, the question as to the progress of organic chemistry would be easily answered. Let the reader open a text-book on chemistry of fifty or sixty years ago, and he will find, sheltering itself under the wing of the inorganic chemistry of that day, the half-fledged science of organic chemistry. Then let him turn to Beilstein's gigantic *Handbuch der organischen Chemie*, with its more than two thousand large closely-printed pages—a mere classified catalogue of the known facts, written moreover in the highly-condensed elliptical style appropriate to catalogues. Here is increase.

But life is not measured by days, nor chemistry by new compounds; and the reader might resent the invitation to appraise the progress of organic chemistry by this rough quantitative method. A qualitative analysis is necessary here.

But how? The really important facts, even with the aid of the most judicious selection, could hardly be packed within the compass of a single article; nor would they be interesting, or, in such compression, even intelligible, to the non-chemist. There are of course the usual *pièces de résistance* in the shape of the coal-tar colours, and the various naturally-occurring compounds that have been artificially prepared; but probably the general reader has heard enough of these already, and might feel inclined to ask whether organic chemistry has nothing further to say for itself.

There is, however, a peculiarity of organic chemistry which distinguishes it from the remainder of the science. The aim of all chemistry is to ascertain the constitution of matter, and the said peculiarity of organic chemistry is, that it expresses its views on this important subject greater detail, more precisely (or, as some will have more dogmatically), than inorganic chemistry. Its articl of belief on this head are embodied in its constitution formulæ.

Here we touch on matter which every chemist will once recognize as debatable. But, for good or for ev these constitutional formulæ are, apart of course fro the dry facts, the main scientific outcome of organ chemistry: they form the particular contribution whic organic chemistry has been able to make towards solvin the central problem of all chemistry—the constitution matter. At present they crown the edifice of organ chemistry. Are they the keystone of an arch, or a me meaningless architectural embellishment? This is th most general question which organic chemistry can put itself at the present moment, and an attempt to answer is the most fitting mode of reviewing the past work of th science. Let us therefore turn our attention to thes constitutional formulæ, and ask ourselves what they are what their meaning is, their scope, their justification.

According to some unfriendly critics, constitution formulæ have done incalculable harm to chemistry b causing chemists to desert accurate experiment an observation for idle speculation—to substitute for th arduous work of the laboratory the easy task of stringin together atomic symbols, according to certain rules, o paper. There may in some cases have been some sma measure of truth in this accusation—in other words, ther may have been some occasional abuse of constitutiona formulæ; but the injustice of the accusation as a whol is sufficiently proved by the fact that the most suc cessful experimenters of the day in the domain of organi chemistry are enthusiastic supporters of constitutiona formulæ, and confess to having been guided by thes formulæ at almost every step of their researches. Thi actively-hostile attitude towards constitutional formulæ i fortunately becoming daily rarer.

Another class, not of detractors, but of rather lukewarn friends, of the constitutional formula, regard it as a con venient mnemonic device, by the aid of which the com position of otherwise hopelessly complex compounds ma be successfully impressed on the memory. It is perfectl true that constitutional formulæ do perform this importan function; but an impartial review of the case will, w imagine, lead to their being rated somewhat more highl than this.

A third class may be described as the undiscriminatin admirers—the injudicious friends—of the constitutiona formula. To them the constitutional formula is a fina expression of the position of the atoms in the molecule— a picture of the molecule itself. This is a phase of belie which many pass through in making their first acquaint ance with organic chemistry, and its existence is due t the circumstance either that the teacher is so muc engrossed in impressing the complex array of facts an theories upon the mind of the student that he has no time to introduce philosophic limitations and doubts, o that he considers such an addition only calculated t render an already somewhat tough intellectual fare totall indigestible by a beginner. However this may be, it i certain that the faith of the beginner is quite as frequentl appealed to as his reason.

We shall best be in a position to discern the meanin and to estimate the value of these constitutional formula if we consider to what necessity they owe their origi and how far they fulfil the purpose for which they wer devised.

The atomic theory, as propounded by Dalton, satisfie for a time the requirements of chemists. For eve properly-analyzed compound a more or less simple atom proportion could be calculated, and this atomic propo tion was expressed in the empirical formula of the con pound. These empirical formulæ were combined in equations, and the equations formed a complete expression of the reactions, so far as the weights of matter taking part in them were concerned. Now it was experimentally proved that every definite compound possessed a constant qualitative and quantitative composition, and it seemed to chemists as something of the nature of an axiom that to a given composition one and only one compound could correspond. So convinced were they of the truth of this unproved and, as the event showed, totally erroneous proposition that, when in 1823 and 1824 the first cases of *isomerism*, or identity of composition together with diversity of properties, were discovered by Wöhler and Liebig, the results obtained by these eminent chemists were generally set down to faulty analysis. But the cases of isomerism multiplied rapidly, and chemists had to make their account with this altered state of things. But here the inadequacy of the empirical formulæ became evident. Wherever a case of isomerism occurred, the empirical formula was ambiguous, and the equations in which it was employed were neither a complete nor a precise expression of the reactions.

To some, the discovery that constancy of composition no longer involved constancy of properties may have seemed to sap the very foundations of chemistry as then understood. But this was not the case. The discovery necessitated an extension of the atomic theory, not its abolition. In fact, isomerism afforded a remarkable proof of the correctness of the view that matter consisted of atoms. On the alternative hypothesis that matter fills space continuously and homogeneously, isomerism is incapable of explanation; as it is inconceivable that the same given quantities of the same given kinds of matter, continuously and homogeneously filling space, should produce more than one compound. A difference of properties in such a case bespeaks a difference of *arrangement* of the component parts; and further, as each such compound displays, even in the state of the finest mechanical subdivision, perfect uniformity, the component parts, by the arrangement of which the difference of properties is produced, must be exceedingly small. We are thus led back to the atomic theory, whilst at the same time the extension is indicated which it was necessary to make in this theory in order that isomerism might find its proper place and explanation under it. It was necessary to determine, so far as possible, the *mode of arrangement* of the atoms in the various compounds. The results of this attempt are embodied in the constitutional formulæ which have been employed by chemists at various times.

The method resorted to in solving this problem was very similar to that which had been employed in determining the ultimate composition of compounds. Just as when, after isolating from a compound, or introducing into a compound, some particular kind of elementary matter, chemists concluded that the compound actually contained that particular kind of matter, so, when in a reaction a particular group of atoms was eliminated bodily from a compound, or introduced bodily into a compound, they concluded that this group existed as such in the compound. Unfortunately, the conclusion is not always quite so warrantable in the case of atomic groups as in the case of elements. The reaction, for example, by which an atomic group is eliminated from a compound involves the destruction of the parent compound, and in this process, which is generally more or less violent, it is only too easy for the atomic groups to undergo re-arrangement. In this way, alcohol (C_2H_6O), from the fact that it may be split up into ethylene (C_2H_4) and water (H_2O), was at one time regarded as containing these two atomic groups—a view which at all events is not that at present held. We thus see that from two different reactions, two totally distinct and mutually incompatible constitutional formulæ may be deduced for the same compound.

It would carry us too far to trace all the steps by which constitutional formulæ gradually became more precise and less self-contradictory, but a few important discoveries may be mentioned which have mainly tended to bring about this result. In the first place, the development of the idea of the molecule as distinct from that of the atom, and the discovery of a means of determining the molecular weight of bodies, led to the division of isomerides into two classes: those in which the proportions of the various atoms were the same, but the total number of atoms in the molecule was different—this mode of isomerism being distinguished as *polymerism;* and isomerism proper, in which both the proportions of the various atoms, and the total number in the molecule, are the same in the different compounds. But the knowledge of the molecular weight aided chiefly in the construction of constitutional formulæ by determining the exact number of atoms in the molecule, and thus facilitating the task of arranging these atoms by stating precisely how many atoms had to be arranged. The law of valency also exercised a most important influence, simplifying matters by greatly limiting the number of legitimate arrangements. In fact, in the case of some of the simpler compounds, such as methane (CH_4), ethane ($CH_3 . CH_3$), propane ($CH_3 . CH_2 . CH_3$), methyl alcohol ($CH_3 . OH$), and others, only one mode of arrangement is, according to the laws of valency, possible for each compound.

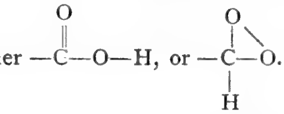
A modern constitutional formula, therefore, takes the various atoms of a compound in the proportions indicated by the empirical formula, and in the absolute number prescribed by the molecular weight, and arranges them in that way which, within the limits of the laws of valency, will best account for the reactions of the compound.

Let us consider what elements of uncertainty are involved in each of the various operations here enumerated.

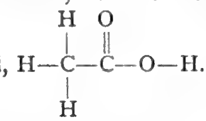
The correctness of the empirical formula of a compound, as calculated from its percentage composition, depends upon the correctness of the atomic weights assigned to its component elements. In the case of organic compounds, to the consideration of which we shall confine ourselves here, the atomic weights of the component elements may be regarded as determined with a degree of probability approaching to absolute certainty. (This does not, of course, refer to the question whether the atomic weight of an element has been determined within 1/10,000 more or less of its true value, but whether, for example, in the case of carbon the atomic weight is 12, or some multiple or sub-multiple of 12.) The empirical formulæ of correctly analyzed organic compounds may therefore be regarded as standing on as sure a foundation as almost anything in the range of science which is a matter of deduction and not of direct observation or experiment. As regards the second point, the molecular weight, an almost equal certainty may be said to prevail in most cases in which the compound can exist in the state of vapour. Avogadro's law, that "when two gases or vapours are at the same temperature and under the same pressure, the number of molecules in unit of volume is the same in both gases or vapours"—this law, originally advanced as an hypothesis, has been shown to follow as a mathematical deduction from the kinetic theory of gases, a theory almost as well established at the present day as the atomic theory itself. Avogadro's law places in our hands a means of determining the molecular weight of substances which are capable of existing in the form of vapour, the only uncertainty attaching to its determinations being that occasioned by the tendency which many compounds exhibit, either to undergo decomposition, or to be incompletely vaporized, in passing into the gaseous state. But in the case of all compounds capable of existing undecomposed in the gaseous state throughout any considerable range of temperature, the molecular weight may be determined with a very high degree of probability. In cases where the compound is not volatile without decomposition, recourse must be had to indirect means in the determination of the molecular weight, and there is consequently more or less uncertainty in such determinations. As regards the third operation in the construction of a constitutional formula—the arrangement of the atoms so as to satisfy their valencies, and at the same time to account for the reactions of the compound in question—both parts of this process, but particularly the latter, involve more or less uncertainty. The valency of an element is frequently a variable quantity, and the validity of a constitutional formula for a compound will depend upon our attributing to each element the valency which it really possesses in that compound. In the case of organic compounds, however, this source of uncertainty is reduced to a minimum. Carbon is, with one certain and one or two doubtful exceptions, always a tetrad; hydrogen and oxygen are constant in their valency; and the character of nitrogen as a triad or as a pentad is generally easy to determine. The chief source of uncertainty lies in the difficulty of expressing the reactions of a compound by its constitutional formula, great scope being left here for arbitrary interpretation. To this is due the fact, on which the opponents of constitutional formulæ lay so much stress, that in the case of numerous compounds the accepted formulæ have varied from time to time. This could, however, scarcely be otherwise. A formula constructed on the basis of an insufficient number of reactions would have to be altered as soon as new reactions were discovered with which it was not in harmony. And it must be admitted that, in the case of most well-studied compounds, very few changes have been made in the constitutional formulæ since these were constructed on the principles of valency. In the case even of the more complex compounds, the constitutional formulæ show a tendency to become finally settled as soon as sufficient experimental material has been accumulated.

In this light, then, a constitutional formula is to be regarded as a symbolic expression, constructed according to the laws of valency, and embodying in a very condensed form the reactions of a compound. By a knowledge of the rules according to which such a formula is constructed —by a knowledge of chemical precedent, as it were—we ought, from an inspection of the formula, to be able to predict the reactions of the compound; to say beforehand, for example, how many substitution-products of a particular class a given compound ought to yield, and so on. The value of a good working hypothesis lies in the fact that it can predict: if it can predict nothing, it is worth nothing. Now, with regard to the question before us, we find that if we correctly embody in a constitutional formula a certain number of reactions—a number sufficient to warrant its construction—it will correctly predict an enormous number of reactions which were not in the least contemplated during its construction. Let us take the example of acetic acid:—

Starting with methyl iodide, which admits of only one constitutional formula, we convert it into methyl cyanide by heating it with potassium cyanide, $CH_3I + KCN = CH_3(CN) + KI$, thus substituting a monad group, CN, for the monad iodine atom. The constitution of this methyl cyanide is, however, not rendered clear by this reaction: it might be either $CH_3 . CN$ or $CH_3 . NC$ according as the cyanogen group is united to the carbon of the methyl by means of carbon or by means of the nitrogen. Both these compounds are in fact known. The one formed in the foregoing reaction has the first of these two constitutions, inasmuch as, when heated with acids or alkalies, it parts with its nitrogen in the form of ammonia, yielding acetic acid, $CH_3 . CN + 2H_2O = CH_3 . COOH + NH_3$. In this *hydrolysis*, or decomposition of the compound with assumption of the elements of water, the nitrogen atom of the cyanogen group is removed, whilst the carbon atom remains in combination; and we therefore conclude that it was by means of this carbon atom, and not by means of the nitrogen atom, that the CN-group was united to the carbon of the methyl group—a conclusion confirmed by the behaviour of the isomeric methyl cyanide, in which the carbon atom of the NC-group can be eliminated, leaving the nitrogen attached to methyl. The only question remaining to be solved is the constitution of the monad group, COOH, which might be either



From what is known from other sources concerning the mechanism of the process of hydrolysis, the first formula is the more probable; that it is correct is shown by the behaviour of acetic acid towards the trichloride of phosphorus, by which reagent the hydroxyl group (OH) can be removed and replaced by a monad chlorine atom, whilst the resulting acetyl chloride ($CH_3 . COCl$) may be reconverted by the action of water into acetic acid ($CH_3 . CO . OH$). Acetic acid therefore contains a monad group, OH, exchangeable for chlorine; and the first formula is correct. Uniting this COOH-group (carboxyl) to the methyl group, and expanding these radicles, we have the fully-dissected formula of acetic acid,



What does this formula tell us? What does it predict? Of the four hydrogen atoms, three are directly united to carbon, and one is distinguished from the others by being indirectly united to carbon by means of oxygen. We know that hydrogen, when directly united to oxygen (for example, in water) may be displaced by electro-positive elements such as metals; and we find that in acetic acid one hydrogen atom is distinguished from the others by this property. We know that hydrogen atoms in direct union with carbon (as in the hydrocarbons) may be displaced by electro-negative elements such as the halogens. This we find to occur in the case of acetic acid: there are three atoms of hydrogen which may be successively displaced by chlorine and other electro-negative atoms or groups. That these three chlorine atoms are attached to the same atom of carbon, and that therefore the three atoms of hydrogen which they have displaced are also so attached, is shown by the fact that potassium trichloracetate, when warmed with a solution of potassium hydroxide, yields chloroform ($CHCl_3$)—

$$CCl_3 \vdots CO . OK.$$
$$H \vdots OK$$

The same reaction with ordinary potassium acetate (a higher temperature being, however, required in this case) yields marsh gas (CH_4)—

$$CH_3 \vdots CO . OK.$$
$$H \vdots OK$$

In both these reactions the molecule is divided at the point of union of the carbon atoms. Apart from this disruption, each carbon atom retains the same atoms in combination with it after the separation which were attached to it before. In the reaction with phosphorus trichloride already referred to, the molecule of acetic acid is divided at the point of union of the hydroxyl group (OH) with the acetyl group ($CH_3 . CO$). That such separations are possible without disturbing the atomic arrangement of the separated groups renders the construction of constitutional formulæ possible. But the point to be noted is that all the foregoing reactions and many others—in fact all the reactions of acetic acid—are satisfactorily explained by the constitutional formula, and are, so to speak, embodied in the formula.

A constitutional formula is thus founded on reactions and predicts reactions. In this lies its chief value. A constitutional formula which is not founded on reactions possesses a very slight value indeed. The constitutional formula of a complex mineral silicate, for example, is not an expression of the reactions of that silicate, inasmuch as the silicate has not hitherto been induced to yield any variety of reactions worth mentioning: it is merely the simplest, and perhaps the most symmetrical, way of arranging the component atoms consistently with their valency, and in accordance with certain analogies in the constitution of salts of oxygen-acids. Not even the molecular weight of the silicate is known, and this knowledge is the first step towards the construction of a constitutional formula which shall have any great value. But the beginner, who has not always the genesis of the various formulæ before his eyes, is apt to put all constitutional formulæ into one category, and to view all with equal trust or distrust according as his temperament happens to be sanguine or sceptical.

The chief opponents of constitutional formulæ are to be found among inorganic chemists. Constitutional formulæ are essentially a creation of organic chemistry. We have seen that they mainly originated in the necessity of explaining the phenomena of isomerism. Now isomerism, which is the rule in organic chemistry, is entirely the exception in inorganic chemistry. The constitutional formulæ of inorganic chemistry are thus an artificial growth: they are the result of an attempt to transplant into inorganic chemistry methods and analogies derived from organic chemistry, and it cannot be affirmed that these borrowed growths have altogether flourished in the new soil. Where the organic formulæ have guided the chemist through the labyrinths of the various classes of compounds, predicted reactions, laid down the number of possible isomerides, and shown the way to the synthesis of natural compounds so high in the scale of complexity as alizarin and indigo, the same methods applied to inorganic chemistry have led to no tangible result higher than that of checking a few doubtful formulæ by means of the laws of valency. The reasons of the failure have already been indicated. But the partial failure of constitutional formulæ in inorganic chemistry is hardly an argument against their use in organic chemistry, where they have achieved the most signal success.

Up to this point we have regarded the constitutional formula simply as a symbolic device, by means of which reactions and cases of isomerism may be expressed and predicted. The question now arises: Is it anything beyond this? A constitutional formula is primarily a certain definite arrangement of atomic symbols. Is there anything like this atomic arrangement in the molecule itself, or even anything corresponding with it?

It is in the highest degree improbable that there is anything *like* it in the molecule itself, but quite possibly there is something *corresponding with* it. That the constitutional formula cannot be like the molecule in the sense of being a picture of it is manifest from a variety of considerations. To mention one out of several: a constitutional formula represents the atoms as points connected with one another in a certain definite way by lines of attraction, without reference to any actual positions in space which these atoms may be supposed to occupy; for the sake of convenience they are represented as lying in the plane of the surface on which the formula is drawn. Now the kinetic theory of gases informs us that the atoms within the molecule are not to be conceived of as occupying their positions in a state of rest: each executes some form of vibration or rotation. This view is quite compatible with the existence of definite relations of attraction between given atoms within the molecule. To borrow an illustration from astronomy, we might in the constitutional formula of acetic acid, for instance, regard the two carbon atoms as the two suns of a double star, and the atoms directly attached to the carbon suns as planets—one with a satellite. The parts may execute their respective motions without disturbing the stability of the whole, any more than the stability of the solar system is disturbed by the motions of its parts. Now, it is evident that a constitutional formula which represents the atoms as motionless in a plane cannot be a true image of the molecule—cannot be *like* it.

That the constitutional formula, however, in some way *corresponds with* the molecule, is shown, not only by the chemical evidence which we have already discussed, but, what is more important, by a number of physical considerations. That the physical properties of a substance are dependent on the arrangement of the atoms within the molecule is evident from the fact that in isomeric compounds the melting-point, boiling-point, specific gravity, and other physical properties generally vary for each isomeride. A comparison of the physical properties of similarly-constituted compounds shows that in many cases very definite relations can be traced between constitution and physical properties.

Very important information has been gained in this way by studying the behaviour of organic compounds towards light. Thus a number of these compounds when in the liquid state, or in solution, cause the plane of a ray of polarized light, if passed through them, to turn through a certain angle. It was observed by Le Bel that all such optically active substances contained in their constitutional formulæ at least one *asymmetric* carbon atom—that is, a carbon atom united to four dissimilar atoms or groups; and an ingenious hypothesis has been put forward by Le Bel, and in greater detail by Van't Hoff, to account for this concatenation. The researches of Gladstone and Dale, Landolt, Brühl, and others on the molecular refraction of organic liquids have demonstrated an intimate connexion between the refractive power of a liquid on the one hand and its constitution on the other, so that observations on the refractive power may be employed in ascertaining the constitution of such compounds; and Perkin has recently shown that the "magnetic rotatory power" of organic liquids—the power which such liquids possess, when placed in a strong magnetic field, of turning the plane of the polarized ray—may be utilized in the same manner. Again, the selective absorption which organic liquids exercise on light of different wave-lengths is closely connected with the constitution of these liquids; and the presence of certain organic radicals in the formula of a compound is manifested by certain definite absorption-bands which make their appearance in the photographed spectra of the infra-red (Abney and Festing) and of the ultra-violet (Hartley).

Other remarkable relations between constitution and physical properties are manifested in what is termed the *molecular volume* of organic liquids at their boiling-points—a subject first investigated by Kopp, and later by Thorpe, Ramsay, Lossen, and others. By the *molecular volumes* of compounds are understood the relative volumes which quantities of these compounds taken in the proportion of their molecular weight occupy. Kopp found that the molecular volume of a liquid organic compound at its boiling-point is the sum of the atomic volumes of its elements; and that, whereas the atomic volumes of carbon and hydrogen are constant, the atomic volume of oxygen varies with its mode of combination, having two distinct values: one value for an oxygen atom attached with both its affinities to the same atom of another element, and a second value for an oxygen atom attached to two different atoms.[1] Sulphur exhibits a similar definite variation in atomic volume in accordance with the mode of distribution of its affinities.

[1] Kopp distinguished "intra-radical" and "extraradical" oxygen. The above is a re-statement of his views in terms of modern formulæ.

In all the foregoing instances we have a successful correlation of the results of physical chemistry with those obtained by the pure chemist in the deduction of constitutional formulæ. Many who withheld judgment, or even condemned, when the chemist was his own witness, may listen to him more favourably now that he is supported by the independent testimony of the physicist.

These investigations into the physical properties of organic compounds are of relatively recent date. There is little doubt that as they are extended new and important laws will be deduced. Much is to be hoped from thermo-chemistry, incoherent as many of its utterances as yet are. By following the path of physico-chemical research, chemists may even hope to arrive at a dynamical representation of the molecule which shall be as much more powerful as an instrument of research than the present merely statical constitutional formula, as that is more powerful than the empirical formula which preceded it. It is hazardous to try to fix a limit to scientific advance in any direction, but it is probable that the modern constitutional formula represents the limit to what purely chemical research can accomplish in determining the constitution of matter. Much will still have to be done by purely chemical research in working out the details of the existing system: the constitution of the more complex compounds will be ascertained on the lines of our present formulæ; new valuable natural compounds will be synthesized. But mere chemical reactions can probably never settle questions of intramolecular dynamics; in these help must come from physical chemistry. Moreover, the physical methods of research supplement the chemical methods in one important particular. By chemical methods we can never study the molecule as it actually exists. Our synthetical methods give us information concerning the molecule only at the moment of its formation; our analytical methods equally confine themselves to the moment of its destruction. The physical methods supply this want: they enable us to study the existing compound. Of these physical methods, one of the most promising, although one of the most recent, is the optical method, which has yielded results of the utmost importance both in inorganic and in organic chemistry. The ray of light which passes from the fixed star to the earth gives us information concerning the composition of the atmosphere of the fixed star; and it is perhaps not too much to hope that the ray of light which has threaded its way through and between the molecules of a compound, and has been modified by its contact with these, will, if properly interrogated, furnish some information concerning the structure of these molecules. Indeed, in the case of the rotation of the plane of polarized light by organic liquids, of their absorption spectra and their indices of refraction, this information has in a measure been obtained.

To sum up. The constitutional formula is not an ultimate expression of the whole truth as regards molecular structure. But it is certainly a very useful and convenient symbolical expression of certain aspects of the truth. We all hope that it may one day be superseded by some higher and more complete generalization. But it will be absorbed and assimilated, not rejected and contradicted, by that generalization. *Non omnis morietur.*

THE YALE COLLEGE MEASUREMENT OF THE PLEIADES.[1]

THE Messrs. Repsold have established, and for the present seem likely to maintain, a practical monopoly in the construction of heliometers. That completed by them for the Observatory of Yale College in 1882 leaves so little to be desired as to show excellenc not to be the exclusive result of competition. In mer size it does not indeed take the highest rank; its apertur is of only 6 inches, while that of the Oxford heliomete is of 7½; but the perfection of the arrangement adapting it to the twofold function of equatoria and micrometer, stamps it as a model not easy to be sur passed. Steel has been almost exclusively used in th mounting. Recommended as the material for the objective cell by its quality of changing volume under variations o temperature nearly *pari passu* with glass, its employmen was extended to the telescope-tube and other portions o the mechanism. The optical part of the work was don by Merz, Alvan Clark having declined the responsibilit of dividing the object-lens. Its segments are separabl to the extent of 2°, and through the contrivance o cylindrical slides (originally suggested by Bessel) perfec definition is preserved in all positions, giving a rang of accurate measurement just six times that with a fila micrometer. (Gill, "Encyc. Brit." vol. xvi. p. 253 Fischer, *Sirius*, vol. xvii. p. 145.)

This beautiful engine of research was in 1883 place in the already practised and skilful hands of Dr. Elkir He lost no time in fixing upon a task suited both to tes the powers of the new instrument and to employ them t the highest advantage.

The stars of the Pleiades have, from the earliest time attracted the special notice of observers, whether savag or civilized. Hence, on the one hand, their prominenc in stellar mythology all over the world; on the other, thei unique interest for purposes of scientific study and com parison. They constitute an undoubted cluster; that is t say, they are really, and not simply in appearance, groupe together in space, so as to fall under the sway of prevailin mutual influences. And since there is, perhaps, no othe stellar cluster so near the sun, the chance of perceptibl displacements among them in a moderate lapse of tim is greater than in any other similar case. Authentic dat regarding them, besides, have now been so long garnere that their fruit may confidently be expected at least t begin to ripen.

Dr. Elkin determined, accordingly, to repeat the surve of the Pleiades executed by Bessel at Königsberg durin about twelve years previous to 1841. Wolf and Pritchar had, it is true, been beforehand with him; but the wid scattering of the grouped stars puts the filar micromete at a disadvantage in measuring them, producing minut errors which the arduous conditions of the probler render of serious account. The heliometer, there can b no doubt, is the special instrument for the purpose, an it was, moreover, that employed by Bessel; so that th Königsberg and Yale results are comparable in a stricte sense than any others so far obtained.

One of Bessel's fifty-three stars was omitted by Di Elkin as too faint for accurate determination. He adde however, seventeen stars from the Bonn *Durchmusterung* so that his list comprised sixty-nine, down to 9·2 magni tude. Two independent triangulations were executed b him in 1884–85. For the first, four stars situated nea the outskirts of the group, and marking the angles of quadrilateral by which it was inclosed, were chosen a reference-points. The second rested upon measure of distance and position-angle outward from Alcyon (η Tauri). Thus, two wholly unconnected sets of posi tions were secured, the close accordance of which testifie strongly to the high quality of the entire work. The were combined, with nearly equal weights, in the fina results. A fresh reduction of the Königsberg observa tions, necessitated by recent improvements in the valu of some of the corrections employed, was the preliminar to their comparison with those made, after an interval o forty-five years, at Yale College. The conclusions thu laboriously arrived at are not devoid of significance, an appear perfectly secure, so far as they go.

[1] "Determination of the Relative Positions of the Principal Stars in the Group of the Pleiades." By William L. Elkin. Transactions of the Astronomical Observatory of Yale University. Vol. I., Part I. (New Haven: 1887.)

It has been known for some time that the stars of the Pleiades possess a small identical proper motion. Its direction, as ascertained by Newcomb in 1878, is about south-south-east; its amount is somewhat less than six seconds of arc in a century. The double-star 61 Cygni, in fact, is displaced very nearly as much in one year as Alcyone with its train in one hundred. Nor is there much probability that this slow secular shifting is other than apparent: since it pretty accurately reverses the course of the sun's translation through space, it may be presumed that the *backward* current of movement in which the Pleiades seem to float is purely an effect of our own *onward* travelling.

Now the curious fact emerges from Dr. Elkin's inquiries that six of Bessel's stars are exempt from the general drift of the group. They are being progressively left behind. The inference is obvious, that they do not in reality belong to, but are merely accidentally projected upon, it: or, rather, that it is projected upon them; for their apparent immobility (which, in two of the six, may be called absolute) shows them with tolerable certainty to be indefinitely more remote—so remote that the path, moderately estimated at 21,000,000,000 miles in length, traversed by the solar system during the forty-five years elapsed since the Königsberg measures, dwindles into visual insensibility when beheld from them! The brightest of these six far-off stars is just above the eighth (7·9) magnitude; the others range from 8·5 down to below the ninth.

A chart of the relative displacements indicated for Bessel's stars by the differences in their inter-mutual positions as determined at Königsberg and Yale, accompanies the paper before us. Divergences exceeding 0″·40 (taken as the limit of probable error) are regarded as due to real motion; and this is the case with twenty-six stars besides the half-dozen already mentioned as destined deserters from the group. With these last may be associated two stars surmised, for an opposite reason, to stand aloof from it. Instead of tarrying behind, they are hurrying on in front. An excess of the proper movement of their companions belongs to them; and since that movement is presumably an effect of secular parallax, we are justified in inferring their possession of an extra share of it to signify their greater proximity to the sun. Hence, of all the stars in the Pleiades these are the most likely to have a measurable annual parallax. One is a star a little above the seventh magnitude, distinguished as *s* Pleiadum; the other, of about the eighth, is numbered 25 in Bessel's list. Dr. Elkin has not omitted to remark that the conjecture of their disconnexion from the cluster is confirmed by the circumstance that its typical spectrum (as shown on Prof. Pickering's plates) is varied in *s* by the marked character of the K line. The spectrum of its fellow-traveller (No. 25) is still undetermined.

It is improbable, however, that even these nearer stars are practicable subjects for the direct determination of annual parallax. By indirect means, however, we can obtain some idea of their distance. All that we want to know for the purpose is the *rate* of the sun's motion; its *direction* we may consider as given with approximate accuracy by Airy's investigation. Now, spectroscopic measurements of stellar movements of approach and recession will eventually afford ample materials from which to deduce the solar velocity; though they are as yet not accurate or numerous enough to found any definitive conclusion upon. Nevertheless, M. Homann's preliminary result of fifteen miles a second as the speed with which our system travels in its vast orbit, inspires confidence both from the trustworthiness of the determinations (Mr. Seabroke's) serving as its basis, and from its intrinsic probability. Accepting it provisionally, we find the parallax of Alcyone = about 0″·02, implying a distance of 954,000,000,000,000 miles, and a light-journey of 163 years. It is assumed that the whole of its proper motion of 2″·61 in forty-five years is the visual projection of our own movement towards a point in R.A. 261°, Decl. + 25°.

Thus, the parallax of the two stars which we suspect to lie between us and the stars forming the genuine group of the Pleiades, at perhaps two-thirds of their distance, can hardly exceed 0″·03. This is just half that found by Dr. Gill for ζ Toucani, which may be regarded as, up to this, the smallest annual displacement at all satisfactorily determined. And the error of the present estimate is more likely to be on the side of excess than of defect. That is, the stars in question can hardly be much nearer to us than is implied by an annual parallax of 0″·03, and they may be considerably more remote.

Dr. Elkin concludes, from the minuteness of the detected changes of position among the Pleiades, that "the hopes of obtaining any clue to the internal mechanism of this cluster seem not likely to be realized in an immediate future;" remarking further: "The bright stars in especial seem to form an almost rigid system, as for only one is there really much evidence of motion, and in this case the total amount is barely 1″ per century." This one mobile member of the naked-eye group is Electra; and it is noticeable that the apparent direction of its displacement favours the hypothesis of leisurely orbital circulation round the leading star. The larger movements, however, ascribed to some of the fainter associated stars are far from harmonizing with this preconceived notion of what they ought to be. On the contrary, so far as they are known at present, they force upon our minds the idea that the cluster may be undergoing some slow process of disintegration. M. Wolf's impression of incipient centrifugal tendencies among its components certainly derives some confirmation from Dr. Elkin's chart. Divergent movements are the most strongly marked; and the region round Alcyone suggests, at the first glance, rather a very confused area of radiation for a flight of meteors, than the central seat of attraction of a revolving throng of suns.

There are many signs, however, that adjacent stars in the cluster do not pursue independent courses. "Community of drift" is visible in many distinct sets; while there is as yet no perceptible evidence, from orbital motion, of association into subordinate systems. The three eighth-magnitude stars, for instance, arranged in a small isosceles triangle near Alcyone, do not, as might have been expected *a priori*, constitute a real ternary group. They are all apparently travelling directly away from the large star close by them, in straight lines which may of course be the projections of closed curves; but their rates of travel are so different as to involve certain progressive separation. Obviously, the order and method of such movements as are just beginning to develop to our apprehension among the Pleiades will not prove easy to divine.

A. M. CLERKE.

NOTES.

STRENUOUS efforts have been made to secure that the arrangements for the observation of the total solar eclipse of August 19 shall be adequate. "A large number of astronomers," says the *Times* of the 15th inst., "will be distributed along the central line, fully equipped with instruments suited to the particular work they intend to do. The Russians themselves have most energetically organized a very complete set of observations, meteorological and otherwise, at widely-distant stations, viz. Krasnoiarsk in Siberia, Perm in the Ural Mountains, and Viatka in Central Russia; while Prof. Mendeljew goes to Pavlovsk, near St. Petersburg; Prof. Bredichin, of the Moscow Observatory, to Kineshma; and Dr. Podsolnotschnaja will be stationed near Tver. Several foreign astronomers will also visit Russia, and have received very hospitable treatment at the hands of Prof. Struve and the other Russian authorities. From

England, Dr. Copeland, of Lord Crawford's Observatory, and Father Perry, of Stonyhurst, have accepted an invitation from Prof. Bredichin to two members of the Astronomical Society, and have already joined him at Kineshma; and Mr. Turner, from the Greenwich Observatory, will occupy a station selected by Prof. Struve. Prof. Young and Prof. M'Neill, from America, have gone to Tver; and two other American astronomers will also make observations. Prof. Tacchini and Dr. Riccò, from Italy, have gone to Viatka; and two German delegates and one French have also been sent." We may add that there will be an American photographic and spectroscopic station in Japan.

SEVERAL very good speeches were delivered last Saturday in the course of the debate on the Education Estimates. Mr. Mundella did excellent service by insisting, as he had often done before, on the necessity for a higher standard of education in our elementary schools. A great many people seem still to be of opinion that the State discharges all its obligations in this matter if it secures that children shall learn to read and write. But what is the good of teaching children to read and write if they are not also taught how to put the power, when they have acquired it, to a proper use? The chances are that reading and writing, if education goes no further, will soon be forgotten. Long ago this was pointed out by M. Thiers, who showed that children in France who could read and write at the age of eleven ceased to be able to do either before they entered the army as conscripts. If education is to be of real value, it must be carried on to an age when boys and girls are capable of taking an interest in "things of the mind," and they must receive instruction in subjects which they are likely to find attractive. This was urged with much force by Sir John Lubbock, who argued that history and natural science should receive far more attention than is now devoted to them in elementary schools, and that manual instruction ought to be added to the list of the optional class subjects.

FROM the Aiken (South Carolina) *Recorder* of the 19th ult we learn that Dr. Henry William Ravenel died on July 17, after a protracted illness. He was a native of the State in which he died, and early in life botany was his favourite pursuit, and fungi his specialty. Soon after graduating he engaged in cotton planting, and continued it for twenty years. Subsequently he devoted more time to botany, and during the last few years of his life he was Botanist to the South Carolina State Department of Agriculture. The infirmity of deafness prevented him from taking any other post. He published a few short papers, chiefly on the plants of his native State; but he was more widely known from his "Fungi Caroliniani Exsiccati," of which he issued a number of fasciculi; and the "Fungi Americani Exsiccati," which he prepared in conjunction with Dr. M. C. Cooke. He was a member of several scientific Societies, and in 1886 the degree of LL.D. was conferred on him by the University of North Carolina.

THE Curatorship of the Natural History Department of the Science and Art Museum of Dublin is now vacant, owing to the resignation, through ill-health, of Mr. A. G. More. Mr. More has been associated with the Institution for twenty years. He succeeded to the Curatorship six years ago, on the death of Dr. Carte, and the condition of that portion of the Museum over which he presided testifies to-day to his abilities as an administrator, and to the exceptional skill of himself and those who have been associated with him. As a botanist he is best known as joint author with the late Dr. Moore of the "Cybele Hibernica"; as a zoologist his name is honourably associated with British ornithology. Numerous notes and papers, scattered throughout various journals, give evidence of his scientific attainments and activity; and by no means an inconsiderable portion of his experience lies buried in publications on the Irish fauna and flora, for, with characteristic good-nature, he has always been willing to help local naturalists with his experience and critical knowledge. His loss is greatly to be deplored, and we wish the directorate good fortune in the choice of his successor.

THE Autumn Congress of the Sanitary Institute of Great Britain will be held at Bolton on September 20 and following days, under the presidency of Lord Basing. The Council invite papers on subjects included in the programme, and will be very glad to receive the personal co-operation and support of all who are interested in the diffusion of sanitary knowledge.

ON Saturday last, M. Jovis, accompanied by M. Mallet, made a balloon ascent from Paris, hoping that he might reach a height greater than that attained by any previous aëronaut. The balloon began to ascend at 7.15 a.m., and was visible until 8.10, when it disappeared, having reached, as was supposed, a height of between 7000 and 8000 feet. About eleven o'clock it came down in Belgian Luxembourg. The altitude reached was 22,000 feet. This is far below the "record" of Messrs. Glaisher and Coxwell, who rose to a height of 37,000 feet.

THE eighth Bulletin of Miscellaneous Information issued from the Royal Gardens, Kew, has just been published. It contains a series of valuable notes on the Tree Tomato (*Cyphomandra betacea*), the Chocho (*Sechium edule*), the Arracacha (*Arracacia esculenta*), and the Cherimoyer (*Anona Cherimolia*). All these food-plants have been recently introduced from the West Indies to the East Indies. The notes are preceded by the following statement:—"The introduction of the Arracacha was first attempted, at the instance of the Government of India, in 1879, but, after many failures, was only successfully accomplished in 1883. The Chocho was introduced to Ceylon by means of a single plant, which survived the journey direct from Jamaica to Ceylon, in January 1885. The Tree Tomato and Cherimoyer were introduced by seeds, which travel well and are more convenient for distribution than plants. In a few years, no doubt, all these plants will be widely distributed throughout the East, and they will be found useful additions to the vegetable diet of both Europeans and natives. Already the Chocho introduced to Ceylon as recently as 1885 is to be found in the local markets; and the Tree Tomato is mentioned 'as a most valuable acquisition to Southern India.' All the four plants here mentioned are likely to thrive at hill-stations in India and in all districts suitable for coffee and cinchona cultivation. They are sub-tropical rather than tropical in their requirements, and hence no doubt they will be found of service in South Africa, in certain parts of Australia, Northern New Zealand, and in hilly districts generally throughout our tropical possessions. The information here summarized will indicate their usefulness as food-plants, and the sources both in the Old and New World from which future supplies of seeds and plants may conveniently be obtained."

SOME doubt has existed as to whether the Chinese have not one or more kinds of plants in use as ginger that are unknown elsewhere. In the Annual Report on the Botanical and Afforestation Department, Hong Kong, for the year 1886, Mr. Charles Ford, Superintendent, says he has taken steps for cultivating all the kinds of plants generally included by the Chinese as ginger, with the hope that he may be able to study them in such a manner as to secure all possible information in connexion with this subject. While at San Ui he was fortunate in obtaining from cultivated plants good flowering specimens. These he dried, and, together with specimens of the roots (properly rhizomes), forwarded to the Director of Kew Gardens for a study of them to be made there, where they can be compared with other kinds, or with specimens of the same kind from other places. The specimens he procured were, without doubt, *Zingiber officinale*, the species commonly in cultivation in other parts of the world. It is possible that some other plant, which is not a true ginger, may be used in making the celebrated Canton preserved ginger, but all the information Mr. Ford has yet obtained points to the species *Zingiber officinale* as the only kind which the Chinese use for this purpose. The ginger cultivated on the Lo-Fau Mountains has a wide reputation amongst the Chinese as being of unusual efficacy in medicine. This superior quality may be derived from peculiarities of soil or climate which communicate to the plant exceptional properties.

A REMARKABLE relation is shown to exist by Dr. C. Bender (*Ann. der Physik und Chemie*, 1887, 8 B., p. 873) between certain physical constants and chemical valency. In experimenting upon the density, expansion, and electrical resistance of several salt solutions, and mixtures of the same, the curious fact was noticed that a very simple relation existed between the number of gramme-molecules of the various salts required per litre of water at 15° C. to make up solutions the physical constants of which should remain unaltered on mixing. It is a well-known fact that on mixing two chemically-inactive salt solutions the physical constants generally diverge very considerably from the arithmetical mean of those of the constituents. But Dr. Bender finds that it is possible to prepare "corresponding" solutions, which on mixture shall retain their physical constants unchanged, the constants of the mixtures forming the arithmetical means of those of the constituent solutions; and further, the strengths of these "corresponding" solutions expressed in gramme-molecules per litre bear extremely simple relations to each other. For example, with respect to density and expansion, a solution of sodium chloride containing 1 gramme-molecule per litre at 15° corresponds with a solution of potassium chloride also containing a gramme-molecule, or a barium chloride solution containing half a gramme molecule, barium being divalent; corresponding with these are also a solution of ammonium chloride containing $\frac{5}{4}$ gramme-molecule, and a lithium chloride solution in which $\frac{5}{4}$ gramme-molecule is dissolved in a litre of water. With respect to electrical conductivity, the following also correspond:—Solutions of NaCl, LiCl, and $\frac{1}{2}(BaCl_2)$, each containing n gramme-molecules; and of KCl and NH_4Cl, each containing $\frac{3}{4}n$ gramme-molecules per litre. Hence "corresponding solutions" are those whose gramme-molecule contents, respect being had to valency, stand in a simple relation to each other.

THE *American Meteorological Journal* for the months April to July last contains a reprint of a lecture delivered by Prof. Cleveland Abbe, in December last, before the Franklin Institute, on some popular errors in meteorology. We can only draw attention here to a very few of the points taken up. The author first attacks the astro-meteorological predictions made up for a long time in advance, and shows that every effort to demonstrate any appreciable influence of the moon or planets on our atmosphere has signally failed. He refutes the singular belief that animals or birds know more about future weather than man himself, and attributes their migrations and hibernating habits to the results of experience of many past ages, or to natural causes beyond their control; and he shows that what is true of animals is still more clearly true of vegetables, so that nearly all the rules for weather-prediction founded on the behaviour of plants, on the falling of soot in the chimney, &c., relate simply to hygroscopic phenomena, of which a hygrometer will give more accurate indications. The efforts to show that the destruction or growth of forests affects the climate are objected to on the ground that we have not enough observations of rainfall and temperature properly comparable with each other to justify any conclusion whatever. With reference to the fact of less rainfall being caught in gauges high above the ground, the author explains that, although the drops grow as they descend through clouds, they rarely grow after they have nearly reached the ground; the stronger winds to which the gauge is exposed when set high up, carry the drops to one side, and so the higher gauge catches less than the lower one.

THE Monthly Weather Review of the United States for May contains much useful information, and possesses additional interest from the fact of its publication so soon after date. Eleven barometric depressions are traced in the North Atlantic, two of which traversed the ocean from coast to coast. Among the notices of meteors, one of extraordinary size seems to have fallen in a field near Wellsburg, N.Y., making a pit 40 feet wide and 20 feet deep; an effort is to be made to find the meteor. A special feature in these Monthly Reviews is the reports of fogs in the vicinity of the banks of Newfoundland and in the trans-Atlantic routes. Notes on their possible prediction have been published by Sergeant E. B. Garriott in the last three issues of the Review, and ship-masters have been requested to send special reports relative to the fog-banks observed. From the observations already made it is assumed that the differences in the temperature of the air which cause the development of dense fog, are occasioned by the *deflection* of the regular prevailing air-currents by cyclonic areas advancing from the interior of the continent. A knowledge of the movements of these cyclonic areas would, in the opinion of Sergeant Garriott, allow of the prediction of fog in time to send telegraphic warning to ships leaving British ports. Further investigation of the subject by the Signal Office will show whether these hopes are capable of practical realization.

THE Danish Meteorological Institute has published its *Meteorologisk Aarbog* for 1885, with the exception of that portion relating to the colonies, which appears to be one year in arrear of the other parts. The work is divided into three sections. (1) Observations taken in the kingdom of Denmark at 10 principal stations, 102 climatological stations, and 171 for rainfall. At 8 of the principal stations the observations are printed *in extenso*; and there are also monthly and yearly *résumés*. The correction for gravity at lat. 45° is given for the means of the barometric observations, in accordance with the recommendation of the International Meteorological Committee (Paris meeting, 1885). (2) Colonial stations, containing observations in the Faroe Islands, Iceland, Greenland, and one station at Santa Cruz in the West Indies. (3) Observations of air and sea temperature, &c., taken on 21 light-vessels round the Danish coasts. These latter observations are very valuable for determining various questions connected with the range of sea-temperature of the coasts, and the migrations of fish, &c. The Reports of the Meteorological Council show that such observations have been taken for some years in this country, although not regularly published. The Danish observational system dates from 1861, when it was under the charge of the Agricultural Society. The Meteorological Institute has published its year-books since 1873.

AT a recent meeting of the Pekin Oriental Society, Dr. Dudgeon read a paper on "*Kung-fu*, or Taoist Medical Gymnastics." *Kung-fu* means labour, and is applied to the science of movement, including, among other things, massage, shampooing, and other operations on the body practised with the object of preventing and curing disease, and for the comfort and sense of bracing which they confer. One of the thirteen departments in the Chinese great Medical College is that of pressing and rubbing. An early Chinese work on this subject was translated by the Jesuits in 1779, and first drew the attention of Europe to the subject and stimulated inquiry. Ling, a Swede, introduced the movement cure into Europe; but here it rests on definite anatomical knowledge, whereas in

China it can lay claim to no such foundation. The Taoists adopted the practice at a very early period to ward off and cure disease; but in later times charms, incantations, and magic seem to have taken its place. Dr. Dudgeon described the general principles of the art, including active, passive, and breathing movements, and the *rationale* of the Chinese system of medicine on which it is founded. The life of man depends upon the existence of air circulating throughout the system. The vital principle is supposed to reside at a point one inch below the navel; from here the two principles of nature emanate. Thence, according to Chinese notions, proceeds the breath in expiration, and thither it goes in inspiration. The great object of life and also of *Kung-fu* is to nourish this original air, and avoid disease by preventing the admission of depraved air. Dr. Dudgeon gave a description of the various movements prescribed for various diseases. Some of these are complicated, and many ridiculous, but the practice appears to hold its place still in Chinese medicine.

AT a recent meeting of the French Société d'Encouragement, M. Grosfils, of Verviers, described a new method he had hit upon for preserving butter. The principle of it is, to hinder the crystallization of salicylic acid added to the butter, and so maintain its antiseptic power indefinitely. This he effects by means of lactic acid, which is a pretty strong solvent of salicylic acid. The composition he had arrived at consists of 98 parts of water, 2 parts of lactic acid, and $\frac{1}{5000}$ of salicylic acid. This will preserve good butter indefinitely, even at high temperatures and in hot countries. M. Grosfils estimates that the butter, supposing it retains 5 per cent. of its weight of liquid, will retain 1 part of salicylic acid to 100,000. Lactic acid beyond 2 per cent. gives a slightly acidulated taste which might affect the saleability of the butter: this may be removed by simple washing with water, or, better, with skim-milk containing a little bicarbonate of soda. The preparation of a kilogramme of butter by M. Grosfils' process does not cost more than one or two centimes.

IT appears that, after some years' experiment, M. Jovis, Director of the Aëronautic Union of France, has found a satisfactory varnish for textile materials. It is of great flexibility, contains no oleaginous base, and, while adding little to weight, confers great impermeability. A piece of calico coated with it will retain hydrogen several days, and is not only not disaggregated by the matters applied, but even by use increases their dynamometric force; a matter of great importance for marine cordage, sails, tents, &c. The varnish is also suitable for paintings, wainscoting, &c., and it is exempt from mouldiness. It can be exposed to very varied temperatures without alteration. Lastly, the sub-products can be utilized for coating walls, railway-sleepers, &c. Such is the account presented to the Société d'Encouragement, to which the Aëronautic Union has applied for help to give this new industrial branch a worthy development.

WE have received the Transactions of the Norfolk and Norwich Naturalists' Society for 1886-87. This is the eighteenth annual volume issued by this flourishing Society. The papers are numerous and varied, beginning with the presidential address of Sir Peter Eade, which is devoted to the subject of germ life, more particularly as it affects human and animal life. Mr. Seebohm follows with two papers on the birds of the Lena Delta and of the extreme north of Alaska, and Mr. Harvie-Brown contributes a paper on the birds of Priest's Island. Sir Peter Eade gives an account of two land tortoises (*Testudo græca*) in confinement; and there are two papers on new or rare Norfolk plants. Mr. J. W. Gurney, Jun., has a paper "On the Periodic Movements of Gulls on the Norfolk Coast," and the Rev. H. A. Macpherson writes on "Hybrid Finches." Mr. Francis Day gives descriptions of some remarkable forms of eels found in Saham Mere, Norfolk; and Mr. Southwell has a paper on the "Smelt Fishery in Norfolk," as well as his annual report on the herring fishery from the ports of Yarmouth and Lowestoft. Mr. A. W. Preston continues his meteorological notes. Two papers of more than local interest are contributed by Lieut.-Colonel Feilden and Mr. Herbert Geldart, the former on zoological, the latter on botanical, researches carried on during a voyage to Hudson's Bay on board the *Alert*, which, in the summer of 1886, visited and relieved the various meteorological stations in that locality. There are some interesting communications in the form of "Miscellaneous Notes and Observations"; and last, but by no means least, is Part II of the "Fauna and Flora of Norfolk," being Section II. of a list of the birds observed in the county by Messrs. Gurney and Southwell.

THE journal *Caucase* states that the Imperial Society of the "friends of natural science, ethnography, and anthropology" are devoting particular attention to the zoology of the Caucasus. In 1885 the Society sent a mission to study the fauna of Erivan and of the coast of the Black Sea, and this year it has sent out two expeditions, one to study the fauna of the coast of the Caspian, the other that of the environs of Tiflis and the Lakes Gotchka, Paleoston, and others.

THE death is announced of Dr. Johann Krejci, Professor of Geology at the University of Prague and a member of the Bohemian Parliament.

THE Imperial Leopold-Caroline Academy of Naturalists, at Halle, celebrated its two-hundredth anniversary on August 7.

A VOLCANIC eruption lately occurred in the Island of Galita, on the Algerian coast. The streams of lava were numerous, and the light of the fire was visible for forty miles around.

ON July 26 a severe shock of earthquake was felt at Obernzell, Wegscheidt Messnerschlag, in Lower Bavaria.

A SEVERE earthquake was noticed in Ecuador on August 2, at 6.29 p.m. Great damage was done in many cities, but Cuenca suffered most, many of the houses falling in, and others being seriously damaged. Shocks of earthquake were also felt in several places in Indiana, Kentucky, Tennessee, and on the eastern banks of the Missouri.

THE additions to the Zoological Society's Gardens during the past week include a Red and Blue Macaw (*Ara macao*) from Central America, presented by Dr. and Mrs. T. W. Allright; a Carrion Crow (*Corvus corone*), European, presented by Mr. George Nicholson; a Fieldfare (*Turdus pilaris*), presented by Colonel Verner; a Hive of Bees, presented by Mr. T. Bates Blow; four Geckos, four Frogs from Italy; two Lineated Chalcis (*Chalcides lineatus*) from the South of France; two Dark-green Snakes (*Zamenis atrovirens*), two Natterjack Toads (*Bufo calamita*) from Germany, purchased; a Bennett's Wallaby (*Halmaturus bennetti*), two Viscachas (*Logostomus trichodactylus*), three Wood Hares (*Lepus sylvaticus*), born in the Gardens; a Bronze-spotted Dove (*Chalcopelia chalcospilos*), two Hybrid Spotted Zenaida Doves (between *Zenaida maculata* ♂ and *Z. auriculata* ♀), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

NEW VARIABLE OF THE ALGOL TYPE.—Mr. E. Sawyer announces in No. 159 of *Gould's Astronomical Journal* his discovery that the star 155 (*Uran. Argent.*) Canis Majoris is a variable of the Algol type. A diminution in the light of the star was first observed on March 26; the star was then observed again on March 29 and 30, and April 6, 7, 9, and 10, and appeared on each occasion to be of about its normal brightness. On April 11 at 8h. 15m. it was again found to be faint, but had recovered brightness by 9h. On April 19 another minimum was observed and the recovery of light successfully watched. The next night seemed to show the commencement of another minimum, but the star was low at the time of observation. The epoch would appear therefore to be some aliquot part of eight days; if the observation of April 20 is accepted, it will be about 1d. 3h. It is uncertain, as yet, whether the star has been observed at actual minimum; but the diminution of light remarked has amounted to about half a magnitude. As the star is the first certainly variable star in the constellation, it will probably be called R Canis Majoris. The place of the variable for 1875·0 is R.A. 7h. 13m. 49s., Decl. 16° 9′·7 S.

Mr. Sawyer gives in the same number of the *Astronomical Journal* some observations of Y Cygni, the new Algol-type variable discovered by Mr. Chandler last December. They give a general confirmation of the period, viz. 2d. 23h. 56m., deduced by Mr. Chandler from his own observations.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *AUGUST* 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 August 21

Sun rises, 4h. 56m.; souths, 12h. 3m. 1·1s.; sets, 19h. 10m.; decl. on meridian, 12° 9′ N.: Sidereal Time at Sunset, 17h. 9m.

Moon (at First Quarter August 25, 20h.) rises, 7h. 37m.; souths, 14h. 10m.; sets, 20h. 30m.; decl. on meridian, 2° 55′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	3 15	10 54	18 33	17 49 N.
Venus	8 39	14 10	19 41	6 18 S.
Mars	1 51	9 56	18 1	21 49 N.
Jupiter	10 49	15 57	21 5	10 46 S.
Saturn	2 18	10 13	18 8	20 16 N.

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

August.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
22	65 Virginis	6	20 34	21 26	92 307
27	B.A.C. 6081	6	17 59	19 14	51 277

August.	h.	
21	14	Venus in conjunction with and 9° 13′ south of the Moon.
22	0	Venus at greatest distance from the Sun.
23	13	Jupiter in conjunction with and 4° 12′ south of the Moon.
25	2	Mercury at least distance from the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Aug. 21,	20 8	*m*
			26,	19 47	*m*
Algol	3 0·8	40 31 N.	,, 22,	0 37	*m*
			,, 24,	21 25	*m*
δ Libræ	14 54·9	8 4 S.	,, 26,	21 6	*m*
U Coronæ	15 13·6	32 4 N.	,, 22,	22 36	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 21,	3 14	*m*
			and at intervals of	20 8	
X Sagittarii	17 40·5	27 47 S.	Aug. 24,	22 0	*m*
W Sagittarii	17 57·8	29 35 S.	,, 24,	20 8	*m*
U Sagittarii	18 25·2	19 12 S.	,, 21,	0 0	*M*
β Lyræ	18 45·9	33 14 N.	,, 23,	21 0	*m*
			,, 27,	2 0	*M*
δ Cephei	22 25·0	57 50 N.	,, 27,	0 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
From Pisces	60	11 N.	Swift.
Near κ Cygni	291	60 N.	Slow, brilliant trained meteors.

GEOGRAPHICAL NOTES.

THE *Bollettino della Società Geografica Italiana* for June contains a valuable contribution to the study of the ethnical relations in the Ogoway and Lower Congo basins, by the Cavaliere A. Pecile, who was associated for three years with Count Giacomo di Brazzà in his exploration of the new French protectorate in the equatorial region north of the Congo. All the multifarious tribes of this extensive region, which stretches from the coast inland to the Ubangi affluent of the Congo, are divided into two essentially distinct groups, that is to say (1) the original settled populations, either aborigines in the strict sense of the term, or such as have occupied their present homes from prehistoric times; and (2) those that have made their appearance in comparatively recent times on the Ogoway and Lower Congo continually pressing forward from the interior towards the coast. To the former group belong the Batekes, Adumas, Avumbos, Mbocos, Ondumbos, Mboshi, and many others; to the latter the Bakales, Pauens (Fans), Okandas, and Obambas of the Ogoway, and the Apfurus, Bayanzi, and others of the Congo and its northern affluents. One of the most important results of the author's researches is the light that he throws on this mysterious forward movement of the inland tribes, which is not confined to the equatorial regions, but extends almost uninterruptedly northwards to Upper Guinea and Senegambia. Here the chief aggressive populations are the Toucouleurs (mixed Berbers), Fulahs, and Mandingans, all now Mohammedans; in the Ogoway and Congo basins the Bakales, Fans, and Bayanzi, all still pagans, and mostly cannibals. These have already reached the coast at many points, pressing forward from a vast and almost impenetrable forest zone, which stretches from the seaboard eastward probably to the Niam-Niam country in the heart of the continent. But the author believes that he has discovered the very cradle of the fierce Bakale and Fan peoples about the head waters of the Ivindo (2° 30′ N.), where the old settlements still exist whence the first waves of migration flowed westwards. This general westward movement is described as taking place unconsciously, or through a sort of vague instinct attracting the over-crowded inland populations towards the centres of trade on the coast. Their interests naturally impel them in the direction whence come the European commodities so much coveted by all the inland populations. The Bakales appear to have preceded the Fans by many years, their migrations being chiefly directed towards the lagoons of the Lower Ogoway, where they are now settled between the local Galoa and Inenga tribes. The Bayanzi, who have acquired the ascendency along the right bank of the Lower Congo, seem to have come originally from the same regions as the Fans, whom they resemble in physical appearance, character, language, and usages. But while the latter are "land-lubbers," displaying absolute horror of the water, the Bayanzi have always been great fluvial navigators, so that their original home may have been the Upper Ubangi, slowly advancing down this great artery to its junction with the Congo. In general the settled aborigines are of blacker complexion and more decided Negro type; the intruders much fairer, taller, with more regular features, less woolly hair, more animated and intelligent expression. At the same time they are also more ferocious and very decided cannibals. This point, about which some doubts had been expressed, was confirmed in a startling way by the fate of three Aduma boatmen belonging to the Expedition, who happened to be left behind near a Fan village on the banks of the Ogoway, and whose skeletons were afterwards found carefully picked (*diligentemente scarnati*) by the villagers. The Fans are continually on the look-out for captives to supply their cannibal feasts, whereas the somewhat more pacific Batekes are anthropophagists rather through the necessity of procuring a flesh diet in their present territory, which is nearly destitute of large game. A chief source of their supplies are the unfortunate slaves, or the humbler members of the tribe, who are denounced by the medicine-men as the cause of any calamity, such as the sickness or death of a chief, and who are always sacrificed and eaten to propitiate the evil spirits, and at the same time to satisfy the craving for human flesh.

THE BRITISH ASSOCIATION.

THE Manchester meeting of the British Association promises to be brilliantly successful. It will probably be attended by a larger number of persons than have been present at any former meeting; and, as we have repeatedly noted, ample preparations are being made for the hospitable reception of visitors. The meeting will be rendered especially interesting by the foreign men of science who will take part in the proceedings. To the lists, already printed, of these distinguished visitors we may now add the names of the American chemist Dr. Alfred Springer, and Dr. H. F. Weber, of Zürich. Dr. Sterry Hunt, F.R.S., of Montreal, has also expressed his intention of being present.

From an article in the *Times* of the 15th inst. we reprint the following account of the work which is expected to be done in most of the Sections:—

"Coming down to the Sections, we find the Presidential Chair of Section A (Mathematics and Physics) occupied by the Astronomer-Royal of Ireland, Sir Robert S. Ball, who is not only among the most eloquent of scientific orators, but one of the two great recognized wits of the Association, the other being a brother Irishman, Dr. Haughton. We may therefore expect something unusual in the way of presidential address from Sir Robert. The subject of the address will, we believe, be that part of the science of theoretical mechanics known as 'The Theory of Screws.' Its treatment will be peculiar and somewhat imaginary; it will indeed be 'a dynamical parable,' and contain a little more humour than is usually met with in such addresses. The general proceedings of the Section are likely to be of considerable interest. The report on the very important subject of electrolysis may possibly lead to a lengthy discussion, in which some of the more distinguished foreign visitors may be expected to take part. There may also be a discussion on the report of the Committee on Electrical Standards. Sir William Thomson will most likely exhibit his milliampere balances and read a paper on their application. Some interesting electrolytic results may be expected from Owens College, and Mr. Haldane Gee will exhibit a comparison-magnetometer. Electricity will ocupy a prominent place in the Section. Mr. W. H. Preece will probably read a couple of papers. More results of Profs. Thorpe and Rücker's new magnetic survey may be communicated. Prof. Hull will treat of the effect of continents in altering sea-level. The Ben Nevis Observatory will have another word to say on high-level meteorology, and some papers on heat will come from the Glasgow University laboratory.

"The Geological Section will be presided over by that able palæontologist, Dr. Henry Woodward, of the British Museum. Dr. Woodward, in his address, may be expected to touch on some of the more important topics that have been recently engaging the attention of geologists—the progress of the geological survey, the relations which exist between palæontology and biology, and recent special researches in various directions. There will, we believe, be a discussion by a combined meeting of this Section with the Section of Economic Science, on the question of gold and silver, in which the geologists will mainly deal with the subject of supply. Another important discussion will be on the burning topic of the arrangement of museum collections—whether palæontologists should arrange their finds to suit themselves, or whether the fact of their extinction should be ignored and these specimens be mixed up with their extant fellows.

"The Geographical Section will this year have the honour of being presided over by the chief of the Metropolitan Police, Sir Charles Warren, himself an experienced practical geographer. He will probably in his address deal with one branch of the leading geographical topic of the day—the uses of the study of geography to the practical statesman. In this, the popular Section of the Association, there will be not a few papers of popular interest. The King of the Belgians is sending over two representatives to speak on the Congo Free State, while Capt. Coquilhat, an old Congo official, will read a paper on his own account. Mr. A. Colquhoun, who is in England on short leave, has promised a paper on Burmah and another on Formosa. Mr. John Forrest, the Surveyer of Western Australia, will read a paper on that colony. Mr. Steains, a young engineer just returned from the Botocudo country in South America, will have something racy to tell of an almost unknown people. Dr. L. Wolf, of Leipzig, who has done so much important work on the southern tributaries of the Congo, will give to the Section the results of his journeys. One of the most important papers will be on the new survey of Siam, on which Mr. M'Carthy, the official surveyor, has been engaged for seven years, and the beautiful maps of which he has brought home with him. Various aspects of geographical education will be brought forward by Mr. H. T. Mackinnon, Mr. E. G. Ravensten, and others, while the subject of Antarctic exploration may receive some attention.

"With Dr. Giffen as President of Section F (Economic Science) we may expect an address which will be worth listening to by all interested in our national progress. The subject will be 'The Recent Rate of Material Progress in England,' and the lessons to be adduced will doubtless come home to all in their suggestiveness, so far as holding our own with other nations is concerned. The papers which are promised for the Economic Section are likely to be of unusual interest. The bimetallist movement has a very strong hold in Manchester, and it is expected that Prof. J. S. Nicholson, of Edinburgh, will be present to advocate this cause. Very valuable light will be thrown on the subject by Mr. E. Atkinson, of Boston, who has been commissioned by the United States Government to inquire into European feeling on this important issue. M. Walrus and Mr. Dana Horton will also contribute papers on monetary matters. The status and working of limited liability companies is another subject of special interest in Lancashire; this will be dealt with by Mr. G. Auldjo Jamieson, of Edinburgh. There will also be an important discussion on a topic which is attracting attention all over the country—the depression in the value of land, and the reasons which have brought it about. Dr. Arthur Ransom will contribute an interesting statistical investigation on phthisis areas in Manchester and Salford. Another day will be devoted to a group of papers on subjects connected with foreign trade. Mr. F. Hardcastle, M.P., will read on the classification of the exports of cotton piece goods in Board of Trade returns; Mr. A. E. Bateman, of the Board of Trade, will have a paper on the statistics of our foreign trade, and what they tell us; Mr. Marshall Stevens will write on freights; and Mr. W. Westgarth and Prof. Leone Levi will deal with Australian and American protectionism.

"The economists will also give a day to education, especially in its technical aspects. With this Mr. W. Mather will deal, while Sir Philip Magnus will read a paper on schools of commerce. An interesting paper on farthing dinners in elementary schools will be contributed by Mr. Sargant, of Birmingham, who has made careful and minute observations on the working of the system. Two reports will also be presented to this Section—on the monetary standard, and on the lists by which wages are regulated in the cotton trade. The latter is an elaborate document, and will in all probability give rise to much interesting discussion.

"From Prof. Osborne Reynolds, as President of the Section of Mechanical Science, we are sure to have an address that will be of as much interest to the student of pure science as to those who deal only with its applications. As might be expected, the Manchester Ship Canal is sure to receive considerable attention in this Section, and we may expect a lively discussion on the papers by Mr. W. Shelford on 'Improvements of Access to the Mersey Docks,' Prof. O. Reynolds on 'The Tides in the Mersey,' and Mr. E. Leader Williams on 'The Manchester Ship Canal.' Another paper of the same class will be that by Mr. T. A. Walker on the Severn Tunnel, which is likely to be of special interest. Other papers likely to be of more than average interest will be those of Mr. Gisbert Kapp on the maximum work of dynamos, Mr. H. White on improved railway sleepers, Mr. A. S. Biggart on the Forth Bridge works, Mr. Arthur Rigg on a revolving engine, and Mr. Henry Davey on expansive working in directing pumping-engines.

"Prof. Sayce, as President of the Anthropological Section, is sure to give an address of real human interest. The science of anthropology is young, but it embraces many lines of inquiry. Prof. Sayce will very wisely confine himself to his own line—to the study of language and the evidence we may derive from it as to the history and development of mankind. He may broach some theories that will surprise orthodox anthropologists, and will have a good deal to say upon the Celts. We believe Canon Isaac Taylor is writing a paper on a subject kindred to that discussed by Mr. Sayce, and is expected to occupy the whole of Friday morning. Mr. Stuart Glennie will also contribute a paper on the same subject, and Mr. Akin Kàroly promises some contributions to the remote history of mankind. Mr. Flinders Petrie's collection of Egyptian squeezes and photographs will form the subject of a paper by the Rev. H. G. Tomkins. The report of the Egyptian Photographs Committee, and that on the North American Indians, will both be of considerable interest."

To this statement it is only necessary to add that Dr. E. Schunck will preside over Section B (Chemistry), and Prof. A. Newton over Section D (Biology).

THE JAPAN EARTHQUAKE OF JANUARY 15, 1887.[1]

SOON after the occurrence of the earthquake of January 15 last, which caused considerable damage to property in and near Yokohama, the authorities of the Imperial University directed the writer to visit the places which had been affected by the shock, and to make a full report of all the circumstances. The results thus arrived at form the subject of the present paper. Before proceeding with this, however, it seems desirable to give some particulars respecting the principal shocks which have been felt in the Empire since 1879.

The earthquake of February 22, 1880, is the severest that has been experienced in the Plain of Musashi during the last ten years. The damage done to buildings was very much greater than on the recent occasion. Its origin was in the Bay of Tōkyō.

On October 25, 1881, Nemuro, in Yezo, was visited by a somewhat destructive shock. Fissures were opened in the ground, and the damage to property was not inconsiderable.

The well-known Atami Spa and its neighbourhood were convulsed on the morning of September 29, 1882, by a sudden and severe movement, which damaged embankments, destroyed an historical monument, and did sundry other mischiefs.

The earthquake of October 15, 1884, originated in the Bay of Tōkyō, and affected the Plain of Musashi. It overturned a considerable number of chimneys, cracked walls, and broke articles in museums and elsewhere. In Tōkyō, the greatest horizontal movement, in a soft ground, was 42 mm., or double the amount observed on January 15 last. However, the total damage, taking the whole affected area into account, was smaller.

The seismic waves in the disturbance of October 30, 1885, extended over the whole of Northern Japan and part of Yezo, shaking a land area of 34,738 square miles. But, though of great extent, they fortunately did little harm.

On July 23, 1886, quite a destructive earthquake visited Shinano and the neighbouring provinces, overthrowing several houses, and forming fissures in roads and hill-sides. The shock also stopped the flow of a hot spring at Nozawa. The part most severely shaken was a mountainous district some 2000 feet above the sea, including the famous active volcano of Asama, and many extinct craters. This case was an unusual one, as most of the larger earthquakes in Japan extend along the sea-shore.

Next in the list comes the severe shock of last January.

It thus appears that this Empire is visited by a more or less destructive earthquake *almost once a year*, and that the Plain of Musashi is affected in like manner *at intervals of a few years*.

The shock of last January was of most unusual violence. It originated near the coast, about 35 miles south-west of Tōkyō, and the seismic waves propagated nearly 200 miles to the west and north-east along the Pacific seaboard. On the north-west they approached but do not quite reach the shore of the Japan Sea. They shook, in all, about 32,000 square miles of land area.

In Tōkyō the disturbance began at 6h. 51m. 59s. p.m., with slight tremors. After thirty seconds from the commencement, the greatest horizontal motion (21 mm.) was recorded. The time taken to complete one to-and-fro motion of the ground was 2·5 seconds. The maximum vertical motion was only 1·8 mm., being, as usual, very small compared with the horizontal movement. The principal motion continued for more than two minutes, during which time no less than *sixty distinct shocks* occurred. The maximum velocity and maximum acceleration, which measure the overthrowing and shattering power of earthquakes, have been calculated from the above numbers, and found to be respectively 26 mm. and 66 mm. per second. These numbers, considering the range of motion, are small; or, in other words, the oscillations of the ground were comparatively gentle and slow, which serves to explain the fact that but little harm was done to property in the capital. In Yokohama, Hipp's seismograph registered a horizontal motion of 35 mm.

The origin of the shock was in a narrow band of country running from west to east in the province of Sagami, parallel to the coast, at a distance from it of about seven miles. It emanates from the western or mountainous parts of the province, passes through the southern foot of Oyama (4125 feet above the sea-level), and reaches the Bay of Yokohama in a total distance of about 30 miles. I believe the most probable cause of the shock to have been faulting or dislocation of the earth's crust along the band above named. This inference is supported by the fact that the parts of the country through which the western half of the band passes consists of rocks of different geological formations, interwoven in such a way that their junctions present lines of weakness favourable to earth-snaps. The topographical features of the district—high mountains on the north, and comparative low plateau and sea-shore on the south—also lend strength to this conclusion. Unequal distribution of loads on the earth's surface tends to facilitate bending and folding of the rocks.

It is along the above-named axis or band that the effects were most striking. They were mainly confined, however, to a small breadth on either side of it, so that places as little as two or three miles to the north or south experienced a well-marked diminution of seismic energy. This is not the first instance in the history of the severer shocks in which the destructive effects have been practically limited to a small area near the origin.

More especially on the hilly or western portion of the origin, land-slips and cracks were numerous. The cracks mostly took place in banks, hill-sides, or other situations favourable for their formation. The writer counted no fewer than seventy-two in a distance of seven miles, the largest measuring a foot wide and five hundred feet long, and all of them running parallel to the axis of origin, which is also parallel to the general contour of the country. Several wells became turbid. In some of artesian character the water permanently decreased; in others it increased. There is a ferry across the large river Banyū where it is crossed by the axial band; but the water was so agitated by the shock that for some time afterwards the boat could not be used. The water in one of the rivulets on the west became muddy. The shock was severely felt on board of vessels in Yokohama harbour, the people in many of them rushing on deck under the impression that they had been run into. The effects upon these vessels were doubtless caused partly by motion communicated through the cables, and partly by agitation of the water due to movements of the sea-bottom. The earthquake was preceded by the usual warning roar or rumbling, as of distant cannon, emanating apparently from the western part of the origin-band. In that district, too, the after-shocks on the same night were five in number, while in Tōkyō there was only one. There were four tremors near the origin during the night of the 16th.

Dwelling-houses in country towns and villages are always built of wood. Their frame-work is of timbers from four to seven inches square, crossing one another at right angles. The uprights are placed about three feet apart, and stand on rows of squared stones or boulders, the intervening spaces being filled with bamboo-laths, on which is laid the mud-plaster that forms the walls. Tiles and straw are principally used for the roof-covering. In the district near the origin these wooden houses shook with great violence. Several of them were more or less twisted, cracked, or unroofed. Sliding doors, covered with paper or of wood, which serve as shutters, partitions, and windows in Japanese houses, broke and were shot out of their grooves. The joints between the frames were in some cases badly loosened. Although there are thousands of wrecked houses, in the district of origin, on the verge of falling down, and looking as if a strong breeze would be enough to blow them over, the buildings of this class nevertheless withstood the violence of the earth movements so far as to escape actual demolition. The writer saw only two small rotten hovels which had

[1] Paper by S. Sekiya, Professor of Seismology, Imperial University, Japan. Reprinted from the Journal of the College of Science, Imperial University, Japan, vol. i. part iii. The earthquake, the distribution and effects of which are described in this paper, is the shock which formed the subject of an article in NATURE for June 2 (p. 107), in which one of the autographic records obtained by the author with Prof. Ewing's seismographs was reproduced in *facsimile*. The diagram in question, which showed a greatest horizontal movement of 7½ millimetres, was one of those referred to near the end of this paper as having been obtained on the stiff elevated soil where the University is built, and where the amplitude of the motion was little more than one-third of the motion shown by seismographs of the same construction on the lower alluvial soil.

been thrown down. This circumstance shows the tenacity of wooden framed structures. Prof. T. Mendenhall, in a report [1] on the recent catastrophe at Charleston, says:—"As was to be expected, buildings constructed of wood suffered much less than those of brick. The interior of wooden buildings, however, would often exhibit a scene of total destruction, furniture, book-cases, &c., having evidently moved with great violence."

Fire-proof stores, or *Kura*, suffered severely as to their walls. These buildings have wooden frames, strongly joined by horizontal and vertical pieces, and closely covered with laths, the whole making up a compact box-like structure. The roof is tiled, and carefully plastered with a mud which has a slight cementing property, to the thickness of from three to nine inches. This plaster is put on in several layers, each layer being added after the preceding one has dried. The whole process is an expensive one. The walls, on account of their great thickness and the poor tenacity of the mud, are easily cracked or stripped. As many as sixty or seventy per cent. of the *Kura* suffered from the recent shock. It is evident that these thick-walled structures should be replaced by brick buildings, which are equally fire-proof and much stronger.

It may be mentioned, however, that the frameworks of *Kura*, after having been entirely stripped, have withstood the most violent earthquake on record.

In Yokohama, houses are built of different types and with a variety of materials, so that they afford a fair field for the comparison of seismic effects. It is very fortunate that, judging from the effects wrought by the recent earthquake on both land and buildings, the seismic intensity in this town was less than one-third of that in the western or hilly parts of the origin-band. But for this, the results would have been highly disastrous.

The houses which suffered most were the composite structures of wood and stone. They are built of wooden frames encased with stone blocks, each of the latter measuring 2 feet 9 inches long, 9 inches wide, and 6 inches thick, and being clamped to the wooden planks inside by three iron nails. The nail, called *Kasugai*, is 5 inches long and $\frac{7}{16}$ inch square, and bent at right angles at its two ends. The stone is soft and brittle, being volcanic rock of the worst quality. In time the iron nails get rusty, and the stones are so acted on by rain and frost as to be easily cracked, or detached from the wooden frames, even by moderate shakings. These buildings, erroneously called European houses, already exist in abundance, and unfortunately increase each year in number. They are generally constructed with bad materials and on faulty principles; the object of the builders being to attain fair protection from fire, along with the appearance of a stone building, at the least practicable cost.

Two brick structures received serious damage, cracks having been formed, as usual, at the corners of the buildings and over the windows. The seismic vibrations, however, left no traces on the Town Hall, the Custom House, Prefectural Office, and other well-built structures of brick or stone.

In Yokohama, wooden houses sustained no damage worth mentioning. Joints were more or less loosened and tiles occasionally fell down from the roofs. The tiles that are fastened to the framework of wooden houses, to form walls, were in some cases detached in large quantities. There are decidedly many improvements which might be made in the present wooden buildings, both of Japanese and so-called European styles, especially in the arrangements of their joints, the scientific distribution of materials, &c. If these and other defects were properly remedied, such dwellings might be made pretty safe as against earthquakes. In sites little liable to danger from fire, one may find, in this country, wooden houses built three and even four centuries ago. Wood, no doubt, will continue for a long time to be the chief building material in this country.

In Japan, however, fire is a more constant and even more dread enemy than earthquakes, while terrible conflagrations are often brought about by destructive shocks. Hence, brick and stone should, and probably will in time come to be largely employed for building, especially in towns. The question, then, is to select certain types of brick or stone houses which are best calculated to resist earthquake shocks. Sheet and bar iron houses, as used in Australia, would make very efficient earthquake-proof buildings, although they are not free from several objections.

After the terrible catastrophe of 1883 in the Island of Ischia, the Italian Government appointed a Commission [1] to consider the reconstruction of the buildings in that island. The Commission, after investigating the different modes of construction most suitable for earthquake countries, submitted models of houses in wood, and in combinations of wood and masonry, which were adopted. The Commission recommended that buildings should be chiefly constructed with an iron or wooden framework, carefully joined together by diagonal ties, horizontally and vertically, the spaces between the framework being filled in with masonry of a light character. Not more than two stories above ground were to be allowed, &c., &c.

In Italy, brick houses are joined by iron tie-rods; and similar devices are now, to a certain extent, used in this country. Concerning the erection of brick or stone houses in Japan, much valuable information is to be obtained from the Italians, who, like ourselves, have lived for centuries amidst terrible shakings, and who, no doubt, have gained much experience in the constructive arts suitable to the conditions of our existence here.

A prominent feature in the effects of the recent earthquake was the overthrowing of brick chimneys in Yokohama, especially on the Bluff. Soon after the shock, circulars were sent round to the principal residents, asking for information as to the effects of the shock on the buildings occupied by them. More than fifty answers were received, and the facts embodied in them have been of great value in preparing this paper. The writer takes this opportunity of expressing his warmest thanks for the kind assistance thus rendered to him. From these answers, from the Police Reports, and from actual observations, fifty-three chimneys appear to have been destroyed. In one instance a heavily-coped chimney fell in a large mass through the roof, and severing a strong beam of 1 foot by 8 inches on the second story, penetrated to the ground floor.

About one-half of the chimneys thrown down during the shock were cut in two at their junction with the roof; while some dislodged the tiling and did sundry other damage to the buildings at their points of contact. Evidently the chimneys and the houses moved with unequal range and with different vibrational periods. Prof. Milne has more than once recommended that chimneys should be built thick and squat, without heavy ornamental mouldings or copings; and be, if possible, disconnected from the roofs. Those houses in which his suggestions had been adopted suffered no damage on January 15.

Generally, the relations of the seismic effects to the geological, topographical, and other features of the various localities were found to corroborate previous experience. That the seismic vibrations in hard ground are very much less than in soft soil was well illustrated on the recent occasion. At the University, where the ground is hard and firm, the seismograph recorded only 8 mm. horizontal motion, as compared with 21 mm. registered by a similar instrument placed on soft soil a mile distant. Totsuka is a small town, with a single long street running along the foot of a hill; one side of the street, however, is built on made-up ground. Most serious damage was done on that side, while the opposite houses suffered very much less, though not more than twenty feet distant. Houses built on cliffs and hill-brows received more damage than those situated at the base or on the flat summits of the same hill. To observe the effects of marginal vibration, the writer recently placed one seismograph at the steep edge of a loamy hill thirty-eight feet in height, and another similar instrument at its foot. The motions, thus far measured, at those two levels are found to be in the ratio of 2 to 1. A third instrument will shortly be set up on the flat summit of the same hill. Observations of a similar nature, on different rocks and at various heights, will form the subject of a further paper. It is probably owing to marginal vibration that houses on the Bluff of Yokohama are always heavy sufferers from earthquakes.

The extensive and rapidly increasing use of kerosene lamps in Japan constitutes a grave danger in severe shocks. The lamps now in common use are of very brittle materials, contain the most combustible of oils, and are usually poised on ill-balanced stands. In the great earthquake of 1855, at a time when kerosene was unknown in this country, fire broke out in Yedo at more than thirty points, setting a very large part of the city in a blaze. In the event of another such shock, the mischief which would be produced from this cause alone is awful to contem-

[1] The *Monthly Weather Review*, U.S. Signal Service, August, 1886.

[1] Proceedings of the Institution of Civil Engineers, vol. lxxxiii., Session 1885-86, part i.

plate. Great credit will be due to any one who can invent a convenient earthquake safety-lamp, which, it is to be observed, will also constitute a valuable safeguard in ordinary daily life. It is true, so-called safety-lamps are sold in Tōkyō, but they are very ineffective and miserable affairs. The use of metallic oil-holders would doubtless greatly lessen the danger.

During his inquiry the writer was shown sixteen lamps that had been broken in the recent earthquake. In one instance the kerosene caught fire, and it was with great difficulty that the residents extinguished it by the aid of wet mats.

MINERALS AT THE AMERICAN EXHIBITION.

ONE of the most conspicuous features of the American Exhibition is the remarkable collection of minerals brought over and exhibited by Mr. A. E. Foote, of Philadelphia. Many of the specimens, which are extremely fine, have been obtained during collecting-expeditions undertaken by Mr. Foote himself, and several new species and varieties have been made known to science through his indefatigable labours.

The central feature is a hexagonal pavilion covered with mica, and surmounted by a model of a snow crystal. Each side of the pavilion is devoted to a separate mineral region of the North American continent—except the first, which is filled with a collection of gems and ornamental stones. Here are rough and cut specimens of a precious ruby, topaz, opal, williamsite, with examples of malachite and azurite beautifully banded and taking a fine polish.

A lapidary who has had several years' experience in making rock-sections for the British Museum is constantly employed close by.

Minerals from the region near the Pacific coast come next. Wulfenite, a rare species, some the finest specimens ever seen, is here exhibited in large groups of orange-red crystals; also brilliantly red vanadinites and large bright crystals of chessylite or azurite associated with velvet tufts of malachite. All these are from the marvellous country that Humboldt called New Spain. The deep-red garnets from Alaska in their sombre settings of gray mica-schist are especially noteworthy. Among the minerals of the Rocky Mountain region are wonderful crystals of the green Amazon-stone; ore from the famous Bridal Chamber at Lake Valley, New Mexico, so rich that the heat of a match will cause it to melt and fall in drops of nearly pure silver. A space the size of a moderate-sized room produced about £100,000. The precious turquoise comes from Los Cerrilloz, New Mexico, where Montezuma got his chalchuhuitls that he valued above gold and silver. The Indians still make long pilgrimages for the sacred stone.

Most striking among the minerals of the Mississippi Valley and Lake region are the blendes and galenas from South-West Missouri, a district that now produces over one-half of all the zinc mined in the world. It was formerly so abundant that farmers built their fences with it. Masses of the lead-ore weighing ten tons were found within 12 feet of the surface. Here Indians formerly procured the lead for their bullets, placing the ore in hollow stumps and building a fire over it.

From Arkansas come fine rock-crystals or hot-spring diamonds, with powerful lodestones, arkansites, and hydrotitanites.

From the Lake Superior region come copper, chlorastrolites, and zonochlorite, a remarkable gem-like mineral.

In the case devoted to the North Atlantic coast region is rhodonite, so much used by the Russians in their ornamental work, in fine crystals. The mines at Franklin, N.J., produce also many minerals found nowhere else in the world, such as franklinite, named after the illustrious philosopher; anomolite, a new species recently described by Prof. G. A. König, of the University of Pennsylvania; troostite, jeffersonite, blood-red zincite, &c., &c. Cacoclasite, a new species in fine crystals, associated with pink titanite, comes from the same region, as do the remarkable crystals of apatite. These are among the finest specimens ever seen, and associated with them are the brilliant twin-zircons. From the apatite are manufactured hypophosphites to stimulate the appetite, and superphosphates to grow wheat and corn.

The last case devoted to the South Atlantic coast region contains amethysts, sapphires, aquamarines, tantalite, gummite, and uranolite, huge sheets of mica, &c., &c.

Next to the wall opposite is a very extensive collection illustrating the mineralogy of Pennsylvania, which, besides the well-known coal, iron, and other ores that have made the State famous, includes very extraordinary specimens of the rare mineral brucite, from which the medicine, Epsom salts, may be made; diaspore in fine crystals, corundum for polishing purposes, chromite for producing brilliant yellows, &c., &c.

Adjoining, in cases and drawers, are the college and educational collections indispensable for the studies of mineralogy, geology, and chemistry.

The collection of American Geological Surveys and other scientific works is very extensive, over fifty volumes from Pennsylvania alone being shown. We have devoted so much space to the description of the extensive exhibit made by Mr. A. E. Foote, of Philadelphia, that we can only refer to the minerals shown by Kansas and other States, by the Denver and Rio Grande and C. B. and Q. Railroads, and by various mining companies.

THE FOLK-LORE OF CEYLON BIRDS.

A CORRESPONDENT of the *Ceylon Observer* of Colombo, referring to the interest excited by Mr. Swainson's new book on "The Folk-Lore and Provincial Names of British Birds," notes some points in the folk-lore of the birds of Ceylon, obtained largely in conversation with natives. The devil-bird (*Syrnium indrani*) stands *facile princeps* for his evil reputation; his cry heard in the neighbourhood of villages is a sure harbinger of death, and the superstitious natives are thrown into great consternation by its demoniac screech. The legend about the bird is as follows:—A jealous and morose husband doubting the fidelity of his wife killed her infant son during her absence and had it cooked, and on her return set it before her. She unwittingly partook of it, but soon discovered that it was the body of her child by a finger which she found in the dish. In a frenzy she fled to the forest, and was transformed into a *ulania*, or devil-bird, whose appalling screams represent the agonized cries of the bereaved mother when she left her husband's house. The hooting of owls in the neighbourhood of houses is believed to bring misfortune on the inmates. The magpie robin, though one of the finest of the song-birds of Ceylon, is similarly tabooed; it has a harsh grating screech towards evening, which is considered ominous. The quack of the pond heron flying over a house is a sign of the death of one of the inmates, or of a death in the neighbourhood. If the green pigeon (*Nila kobocya*) should happen to fly through a house, as it frequently does on account of its rapid and headlong flight, a calamity is impending over that house. Similarly with the crow. But sparrows are believed to bring luck, and are encouraged to build in the neighbourhood of houses, and are daily fed. The fly-catcher bird of Paradise is called "cotton thief," because in ancient times it was a freebooter, and plundered the cloth merchants. As a penalty for its sins it was transformed into a bird and doomed to carry a white cotton attached to its tail. The red wattle lapwing, the alarm bird of sportsmen, has the following legend connected with it:—It is said to represent a woman who committed suicide on finding herself robbed of all her money, amounting to thirty silver pieces, by her son-in-law. The cry of the bird is likened to her lament: "Give the silver, give the silver, my thirty pieces of silver." Its call is heard at all hours, and the stillness of night is broken with startling abruptness by its shrill cry. Another story about it is that when lying in its nest in a paddy field, or a dry spot in a marsh, it lies on its back with its legs in the air, being in continual fear that the heavens will fall and crush its offspring. The story current about the blue-black swallow-tailed fly-catcher (*Kawudu panikkia*) and its mortal enemy, the crow, is that the former, like Prometheus of old, brought down fire from heaven for the benefit of man. The crow, jealous of the honour, dipped its wings in water and shook the drippings over the flame, quenching it. Since that time there has been deadly enmity between the birds. The Indian ground thrush (*Pitta coronata*) is said to have once possessed the peacock's plumes, but one day when bathing the peacock stole its dress; ever since the *Pitta* has gone about the jungle crying out for its lost garments. According to another legend, the bird was formerly a prince who was deeply in love with a beautiful princess. His father sent him to travel for some years, and on his return the princess was dead. He still wanders disconsolately about calling her name. It is also said that the peacock, being a bird of sober plumage, borrowed the brilliant coat of the *Pitta* to attend a wedding, and did not return it. The disconsolate *Pitta* wanders through the jungle calling on the peacock to restore its dress—hence the cry, *ayittam, ayittam* (my dress, my dress). The cry of the hornbill (*Kandetta*) is inauspicious and a sure sign of drought. The bird is doomed to suffer intolerable thirst; not being able to drink from any stream or rill, it has the power only to catch the rain-drops in its bill to quench its thirst, and keeps continually crying for rain.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE following is the list of candidates successful in the competition for the Whitworth Scholarships, 1887:—James Whitaker, 21, engineer student, Burnley, £200; John Calder, 20, mechanical engineer, Glasgow, £150; John Smith, 22, carpenter, Belfast, £150; Nicholas K. Turnbull, 21, mechanical engineer, Glasgow, £150; James C. Talbot, 23, engineer, Southampton, £150; Arthur F. Horne, 25, mechanical engineer, Moreton-in-Marsh (formerly of Glasgow), £150; Edward J. Duff, 23, engineer, Glasgow, £150; Robert N. Blackburn, 20, engineer apprentice, Liverpool, £150; William Thomson, 20, engineer apprentice, Glasgow, £150; William W. F. Pullen, 20, engineer apprentice, Cardiff, £100; Edwin Griffith, 20, engineer apprentice, Glasgow, £100; Frederick C. Tipler, 23, assistant chemist, Crewe, £100; Thomas H. M. Bonell, 24, analytical chemist, Swindon, £100; Richard J. Redding, 22, metallurgist, Plumstead (Woolwich), and Arthur W. Sisson, 25, mechanical draughtsman, Lincoln (equal), £100 each; Arthur H. Abbott, 22, engineer, Great Yarmouth, £100; George Hough, 23, engineer, Wolverton, £100; Harry G. Christ, 19, engineer apprentice, London, £100; Harry D. Griffiths, 21, engineer apprentice, Cardiff, £100; Denholm Young, 24, engineer apprentice, Edinburgh, £100; Benjamin G. Oxford, 20, engineer apprentice, Liverpool, £100; Bernard H. Crookes, 21, engineer student, Liverpool, £100; George J. Wells, 23, engineer, London, £100; John Eustice, 23, engine fitter, Camborne, £100; Augustus H. H. Bratt, 24, engineer, Plumstead (Woolwich), £100.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, August 3.—Dr. D. Sharp, President, in the chair.—Mr. J. W. Peers and Mr. R. G. Lynam were elected Fellows.—Jonkeer May, the Dutch Consul-General, exhibited a pupa and two imagos of *Cecidomyia destructor* (Hessian fly) which had been submitted to him by the Agricultural Department.—Mr. W. White exhibited, and made remarks on, a specimen of *Philampelus satellitia*, Linn., from Florida, with supposed fungoid excrescences from the eyes. Mr. Stainton said he was of opinion that the supposed fungoid growth might be the pollinia of an Orchis. Mr. Poulton expressed a similar opinion, and the discussion was continued by Mr. Pascoe and Dr. Sharp.—Mr. White also exhibited a specimen of *Catephia alchymista*, bred from a pupa collected last autumn on the south coast.—Mr. McLachlan sent for exhibition a number of oak-leaves infested by *Phylloxera punctata*, Lichtenstein, which he had received from Dr. Maxwell Masters, F.R.S.—Mr. Champion exhibited two rare species of *Curculionidæ* from the Isle of Wight—viz. one specimen of *Baridius analis*, and a series of *Cathormiocerus socius*. He remarked that *C. maritimus*, Rye, had been placed in recent European Catalogues as a synonym of the last-named species, but that this was an error. He also exhibited a series of *Cicindela germanica*, from Blackgang.—M. A. Wailly exhibited, and made remarks on, a number of living larvæ of *Antheræa pernyi*, *A. mylitta*, *Telea polyphemus*, *Platysamia cecropia*, *Attacus cynthia*, *Callosamia promethea*, and other silk-producing species. He also exhibited imagos of the above species, imagos of *Antheræa yama-mai*, and a number of species of Diurni from Sarawak.—Mr. Poulton exhibited crystals of formate of lead obtained by collecting the secretion of the larva of *Dicranura vinula* on 283 occasions. The secretion had been mixed with distilled water in which oxide of lead was suspended. The latter dissolved, and the acid of the secretion being in excess the normal formate was produced. Prof. Meldola promised t subject the crystals to combustion, so that their constitutio would be proved by the final test.

EDINBURGH.

Royal Society, July 15.—Special Meeting.—Dr. J. Murray Vice-President, in the chair.—Prof. Tait submitted a communi cation by Sir W. Thomson on the stability of the steady motio of a viscous fluid between two parallel planes.—Sir W. Turne communicated a note by Mr. George Brook on the epiblasti origin of the segmental duct in teleostean fishes, and birds.— Prof. T. R. Fraser read a preliminary note on the chemistry c strophanthin.—Mr. J. J. Coleman described a new diffusimete and other apparatus for the study of liquid diffusion.—A pape by Mr. Frank E. Beddard was communicated by Prof. Si W. Turner.—Dr. Murray read a paper on the mean height c the land of the globe. The lower limit he gives is, in roun numbers, 1900 feet. The higher limit, which he believes to b more nearly correct, is about 2100 feet.—Mr. J. T. Cunning ham, of the Scottish Marine Station, read a paper on th *Chætopoda sedentaria* of the Firth of Forth.

July 18.—Sheriff Forbes Irvine, Vice-President, in the chai —The Chairman intimated the foundation by Dr. Gunning c the Victoria Jubilee Prize, and the conditions of award whic have been approved by the donor. The first award of th prize was made to Sir W. Thomson, for a remarkable series c papers on hydrokinetics which he has communicated to th Society.—Mr. W. Durham read the second part of his paper o the laws of solution.—Prof. Tait communicated a paper b Prof. W. Burnside on the partition of energy between th translatory and rotational motions of a set of non-homogeneou elastic spheres. The rotational energy is equal to two time the translational energy.—Dr. H. R. Mill submitted a paper o the salinity, temperature, &c., of the Firth of Forth.—Prof. Tai communicated a paper by Mr. Albert Campbell on the direc measurement of the Peltier effect. Mr. Campbell has experi mented with three pairs of metals. His results agree in ever case with Prof. Tait's thermo-electric diagram. The agreemen in the case of iron and nickel is of special importance.—Dr Alex. Scott communicated a paper on vapour-densities at higl temperatures.—Prof. Tait read a paper by Dr. G. Plarr on th determination of the curve, on one of the co-ordinate planes which forms the outer limit of the positions of the point o contact of an ellipsoid which always touches the three plane of reference.—Mr. Buchan read a paper by Mr. A. Rankin o the mean temperatures of the various winds at Ben Nevi Observatory.—Prof. Crum Brown read a paper on ferric ferri cyanide as a reagent for detecting traces of reducing gases This reagent gives a test depending on the production o colour, which is a more delicate test than one which depends o its disappearance.—Prof. Tait communicated some results on the compressibility of water, of mercury, and of glass. The average compressibility of a 20 per cent. aqueous solution of common salt per atmosphere for the first 100 atmospheres is 0·0000316. It diminishes rapidly with the percentage of salt in solution. The compressibility of common lead glass is 0·0000027 at a temperature of 19° C.—Prof. Berry Haycraft submitted a description of experiments to show the truth of Sir J. Lister's theory of coagulation.—Dr. Murray communicated a paper by Mr. Adam Dickie on the chemical analyses of sea-water from the Clyde sea-area.—The Chairman mentioned the number of papers read during the session, classifying them under various heads. He also read the Jubilee address which had been presented to Her Majesty by the Secretary of State on behalf of the Society.

PARIS.

Academy of Sciences, August 8.—M. Janssen in the chair —Observations of the minor planets, made with the grea meridian instrument of the Paris Observatory during the firs quarter of the year 1887, communicated by M. Mouchez. Th right ascensions and polar distances are given of Leto Sophrosyne, Undine, Hebe, and nine other minor planets a various dates with Paris mean time, all comparisons bein referred to the ephemerides published by the Berlin *Jahrbuch* except those of Undine, which are referred to those publishe in No. 288 of the circulars of the Berlin *Jahrbuch*. Th observations were made by M. O. Callandreau.—Fresh docu ments on the relations existing between the chemical an mechanical work of the muscular tissue, by M. A. Chauveau with the co-operation of M. Kaufmann. In order to complete his series of preparatory determinations on the mechanical work of the muscular tissue, the author has attempted to determine the quantity of heat produced by the muscles which function effectively in the physiological conditions of the normal state. By the methods and new processes here described he claims to have overcome the great difficulties inherent to studies of this nature. His experiments show once more that a large amount of heat is generated while the muscle operates, and of this only a small quantity is absorbed by the work performed. Repeated experiments will be needed accurately to determine this quantity. From the experiments already made, he infers that it mostly ranges from one-seventh to one-eighth of the total, the coefficient of the latter being 0·000323 calories, and that of the heat transformed into work generally from 0·000041 to 0·000034 calories.—New fluorescences with well-defined spectral rays, by M. Lecoq de Boisbaudran. Here the author studies alumina with the earth $Z\alpha_2O_3$; but as this earth has not yet been obtained in a pure state, he has been compelled to employ a substance still mixed with some other rare earths, notably $Z\beta_2O_3$; $Z\alpha$, however, being greatly in excess of $Z\beta$. Alumina containing 1/1200 of $Z\alpha_2O_3$ impure, heated with sulphuric acid and moderately calcined to a red (between the fusions of silver and copper), yields a greenish-yellow fluorescence, faint and without measurable spectrum. With 1/50 of $Z\alpha_2O_3$ in the alumina, a green fluorescence is obtained, slightly yellow and dull. The spectrum consists chiefly of the bands of $Z\beta$, which apparently differ but little from those obtained by reversion with a solution of $Z\beta Cl_6$. The presence of $Z\alpha$ is indicated to the right of the two yellow and blue bands; but the green band of $Z\beta$ is the strongest in the spectrum, having two nebulous maximums, of which that to the right is the most intense. The author also announced that he had obtained some very fine fluorescences by highly calcining alumina containing a little didymium or praseodymium.—The partial lunar eclipse of August 3, observed at the Observatory of Bordeaux, by M. G. Rayet. Under a three-prismed spectroscope, mounted on the great equatorial (0·38 metre) the transition from the adumbrated to the luminous part of the disk appeared very abrupt. While the spectrum of the former was limited by the lines D and F, with a maximum of intensity towards E, that of the part in transition extended abruptly towards the red as far as Ångström's atmospheric group α. But the spectrum of the moon especially near the eclipsed part, was too pale to permit the use of a slit narrow enough to show the atmospheric lines. The α group and the very numerous lines near D were alone distinctly visible under the form of bands.—On the tides of the Tunisian coast, by M. Héraud. The observations made during the hydrographic survey of this coast have enabled the author to study the tidal movement, the existence of which in the Gulf of Gabes and on the adjacent seaboard has long been demonstrated. These tides appear to be the most important and regular in the whole Mediterranean basin; but they are perceptible only on the section of the coast to the south of Mehediah. They continually increase in magnitude as far as Gabes, where they acquire a maximum of 2 metres at the mean spring-tides, thence decreasing to 1 metre at Zarzis and on the Tripoli frontier. The tidal wave appears to come from the east, the mean period being apparently about 24 hours. All the observed circumstances would seem to show that the relation of the lunar to the solar wave is less than that of the absolute actions of the sun and moon.—A comparative study of the old, eruptive and sedimentary rocks of Corsica and the Eastern Pyrenees, by M. Ch. Depéret. During a recent trip to Corsica the author had an opportunity of determining some very close analogies between these two geological systems. Thus the central part of the granitoid mass at Ajaccio is formed of a porphyroid granite disseminated with black mica, passing thence on either side insensibly to a granulitic granite, a true transitional formation between the granulite type and granite. Analogous formations occur in the Eastern Pyrenees, as, for instance, in the granitoid mass stretching east and west between the valleys of the Aude, Têt, and Boulsane. Here also the central part, extending from the forest of Salvanère to Belesta, consists of a porphyroid granite passing on both sides imperceptibly over to a granite with two micas and granulitic texture. A comparative study of the eruptive and sedimentary rocks in both regions reveals similar resemblances. In Corsica the Cambrian limestone everywhere worked as marble is absolutely identical with that of the Pyrenees.

Berlin.

Physiological Society, July 1.—Prof. du Bois Reymond, President, in the chair.—Dr. Martius communicated the results of his researches, by the graphic method, on the movements of the heart. When a sound is passed into the œsophagus, and connected with a Marey drum, cardiopneumatic curves are obtained whose interpretation is still a matter of controversy. In order to arrive at an experimental decision on this point, Dr. Martius has recorded simultaneously on the same individual the cardiopneumatic curves from the œsophagus and the curve of impulse of the ventricular apex as obtained from the wall of the thorax. It appeared from this that the curve of ventricular impulse is of doubtful interpretation; its shape was always the same; but it was impossible to determine with any certainty which part of the curve corresponds to the systole, and which part to the diastole. Dr. Martius has therefore registered the occurrence of the heart-sounds by auscultation and making signals which were recorded on a rotating drum on which the curves of cardiac impulse were being registered, having first ascertained that his personal equation was without influence on the results. In this way he was able to show that the first sound of the heart, corresponding to the closing of the auriculo-ventricular valves, coincides with the first rise of the curve from the base-line, while the second sound, or closing of the semilunar valves, coincides with the second smaller rise of the curve. The *first* rise and fall of the curve corresponds therefore to the cardiac systole. The speaker explained the shape of the *whole* curve as follows:—At the commencement of the systole the auriculo-ventricular valves are shut, as also are the semilunar valves since the aortic blood-pressure has not yet been overcome. During this period the contracting cardiac muscles alter the shape of the heart, the apex moves forward, and so the curve rises. As soon as the pressure in the ventricle is greater than that in the aorta, the semilunar valves open and the blood begins to pour out of the ventricle; as the result of this the apex of the heart moves back, and the curve falls till it reaches the base-line at the conclusion of the systole and commencement of the diastole. At this instant the semilunar valves close and the shock thus produced is communicated to the heart, and makes itself evident on the curve as the second or valvular rise. Thus finally the first rise of the curve of cardiac impulse corresponds to that period of systole during which all the valves are closed; the first apex of the curve marks the instant at which the semilunar valves open; the first fall of the curve indicates that portion of the systole during which blood is flowing out of the ventricle; the systole ends with the commencement of the second or smaller rise in the curve. Dr. Martius has been able to strengthen this analysis of the cardiac movements, so important both physiologically and pathologically, by observing that the duration of the rise and fall of the curve of systole varies in different individuals: thus he finds, conformably with the explanation given above, that in patients with low aortic blood-pressure, the rising portion of the curve of cardiac impulse is very short, while the falling part is considerably lengthened, resulting from the low aortic pressure allowing the semilunar valves to open sooner. On the other hand, in a case of arterial sclerosis, he found the rising part of the curve considerably lengthened, since the aortic blood-pressure was greater, and was only overcome at a later period of the systole.—Dr. Goldschneider presented and explained plates illustrating the topography of the sense of temperature. The sense of heat and cold was determined for the whole surface of the body, and arranged in a series corresponding to twelve degrees of intensity. As a general result, it was found that the sense of cold is more extended than that of heat; that both senses are more developed on the trunk than on the extremities; that the sense of temperature is less acute in the median line of the body; that the distribution of this sense over the surface of the body is quite different from that of the sense of touch; and that the points of exit of the nerves possess little or no sense of temperature.

July 15.—Prof. Munk, President, in the chair.—Dr. Jacobsen gave an account of some acoustical experiments which he has carried out with a view to determining the law according to which the amplitude of vibration of a tuning-fork diminishes as it gradually comes to rest. According to theory, the diminution in the amplitude of vibration takes place in geometrical progression; Hensen had, however, found that the logarithmic decrement at first diminishes, and then, when the vibrations have become extremely small, increases again. The speaker has made experiments with tuning-forks, recording the vibrations of the arms by means of brushes writing on a rotating drum; in another series of experiments, which are not yet concluded, he has photographed the vibrations at equal intervals of time. The result of his work is that the vibrations diminish in geometrical progression, thus according with theory. —Dr. Wertheim gave an account of his experiments to determine the number of visual units in the central portions of the retina. In continuation of the experiments of Dr. Claude du Bois-Reymond, who has determined the number of visual units in the fovea centralis and found them equal in number to the cones, Dr. Wertheim, employing the same method, has determined the number of visual units to a distance of 2·5 millimetres from the centre. A sheet of tinfoil pierced with uniform holes was illuminated from behind, and then the distances were measured at which the holes began to be just visible as separate objects, as their image was made to fall on parts of the retina *successively* further and further towards the periphery. After having found in the fovea centralis the same number of visual units as had Dr. du Dois-Reymond, he then observed that their number decreases rapidly towards the periphery up to a distance of 1·5 millimetres, then remains constant for a short space, then diminishes again rapidly, and then gradually as far as the limits of the retinal area which he investigated. The speaker found that the first rapid decrease extends as far as the limits of the macula lutea. The anatomical statements respecting the limits of the yellow-spot and the number of cones outside this area did not permit of his drawing any conclusion, other than the above, from the optical experiments. The same numbers were obtained when red and green light was used.—Dr. Goldschneider has carried out a series of experiments to test Leyden's theory that ataxy, when not of central origin, is caused by injuries to centripetal nerves. By passing strong electric currents through the first phalanx of one finger he anæsthetized the second and third phalanx, and then found that the movements of flexion and extension of the finger no longer gave a regular curve of rise and fall as traced by the tip of the finger: the movements executed by the finger were irregular, sometimes going beyond and sometimes falling short of the desired extent. The sensation of passive movement was also considerably lessened. The speaker hence concluded that the ataxic movements are caused by the interference with the sensations arising from passive movements of the limbs. He added to this an hypothesis as to the nature of ataxy and the seat of the muscular sense in the limbs.

July 27.—Prof. Munk, President, in the chair.—Dr. Sandmann spoke on respiratory reflexes originating in the nasal mucous membrane. In order to study the possible connexion between asthma and diseases of the nose, which has been so often supposed to exist, the speaker has made experiments on the respiration in rabbits and cats whose nasal openings had been completely occluded. In addition to confirming the phenomena which had been already described by earlier observers, he found that the changes in volume of the thorax were the same as in normal animals, whereas the intra-thoracic pressure was considerably increased when breathing was carried on entirely by the mouth; similarly the respiratory undulations of the blood-pressure tracing were increased in amplitude. He next investigated more closely the respiratory reflexes which originate in the nasal mucous membrane; of these three are known—namely, inhibition of respiration, sneezing, and coughing, as a result of stimulation of the nose. Inhibition of respiration was observed to occur, according to the strength of the stimulation, either in the phase of expiration, or of inspiration, or merely as a more pronounced expiration. Sneezing was brought about by tickling the nasal mucous membrane, and was found to consist of a deep inspiration with simultaneous closing up of the pharynx and mouth by the application of the tongue to the palate, followed by an explosive expiration. When the stimulation is slight, only the deep inspiration is produced; if the stimulation is strong, the deep inspiration is followed by a somewhat lengthy inhibition of the same, which is frequently accompanied by slight expiratory movements; when the stimulation is of moderate strength an ordinary sneeze is the result. After section of the phrenic nerves the deep inspirations were no longer observed. Dr. Sandmann, by section and removal of the mucous membrane in rabbits, has further examined the various regional areas of the same, and found that sneezing can only be produced by tickling a limited area of the mucous membrane. On the rabbit this area is found in the entrance to the nose on the anterior surface of the lowest nasal muscle; but in addition to this place, the same reflexes may be produced by stimulation of the front part of the septum and roof of the nasal cavity. Sneezing cannot be produced by stimulation of any other portion of the nasal mucous membrane. In man the region of the posterior nasal openings is also connected with the reflexes involved in sneezing in addition to the regions mentioned above. An anatomical investigation of the areas whose stimulation leads to sneezing showed that they are supplied entirely by the ethmoid nerve. Stimulation of this nerve in the orbit was followed regularly by sneezing, which could therefore be produced to a certainty by stimulating the trunk of the nerve. The third kind of respiratory reflex—namely, coughing as a result of nasal stimulation—could not be experimentally produced in the cats and rabbits used in these experiments.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Distribution of Rain over the British Isles, 1886: G. J. Symons (Stanford).—First Lessons in Science: Dr. J. W. Colenso (Ridgway).—A Treatise on the Principle of Sufficient Reason: Mrs. P. F. Fitzgerald (Laurie).—Prolegomeni di Filosofia Elementare, Terza Edizione (Torino). —Bulletin de l'Académie Royale des Sciences de Belgique, No. 6, 1887 (Bruxelles).—Journal of the Royal Microscopical Society, August (Williams and Norgate).—Bulletin of the California Academy of Sciences, vol. ii. No. 6.—Boletin de la Academia Nacional de Ciencias en Cordoba, Junio 1886 (Buenos Aires).—Journal of the Anthropological Institute, May and August 1887 (Trübner).

CONTENTS.

THURSDAY, AUGUST 25, 1887.

THE HEALTH OF NATIONS.

The Health of Nations. A Review of the Works of Edwin Chadwick, with a Biographical Dissertation. By Benjamin Ward Richardson. 2 vols. (London: Longmans, 1887.)

DR. RICHARDSON'S two volumes afford much matter for reflection for all those who have endeavoured to improve the condition of the working classes in England during the last half century. They form a panegyric on Mr. Chadwick, and boldly claim for him the credit of having brought forward the principal social improvements of the Victorian era. We think that these wide claims are somewhat to be regretted, as they compel criticism where we should be anxious to speak only in praise; for we are scarcely prepared to go the length of ascribing almost entirely to Mr. Chadwick's influence the vast improvements in the social condition of the people which have taken place during that period.

Mr. Chadwick's active life commenced at a time when the dawn of a new state of things was appearing in this country, and indeed over the world; when, by the development of new means of communication and intercourse, all society was beginning to be completely revolutionized. He was a deep thinker, and seems to have understood intuitively the social problems which were arising; but he undoubtedly had the despot's view that whatever he thought good ought to be carried out. He may be said to have begun his career as Secretary to the Poor Law Board, and then as Commissioner. He was the member of that Board who most persistently urged the extension of the areas of administration, and the employment of paid officers instead of gratuitous service which rewarded itself by favouritism and jobbery. The Poor Law as amended at that time, and as worked by the then Poor Law Commissioners, was devised to abolish out-door relief to the able-bodied, and to apply a labour test for all able-bodied persons who sought the temporary relief of the workhouse; and Mr. Chadwick's fearless administration of that rule brought upon him much enmity from the supporters of the former system of local jobbery. In the half century which has elapsed since that time, there has undoubtedly been a gradual drifting back to the old methods; and it would certainly be an opportune time to make a new inquiry into the administration of the Poor Law on the lines pursued by Mr. Chadwick in 1832.

The investigations of the Poor Law Commissioners brought to light the vast importance of the sanitary problem, which, of all the social problems of that day, was probably the one that cried most for consideration. The chief advance in medical science during the hundred years previous to the Victorian era seems to have resulted from the discoveries by Jenner in regard to small-pox. Beyond this the art of prevention of disease, at the Queen's accession, rested mainly on the laurels gathered by Lind and Meade in the eighteenth century, and by Pringle during the great war. The principle of prevention enunciated by these early pioneers still remains the foundation of our sanitary system; but the practical application of those doctrines has received an enormous extension during the last fifty years; and the various essays and reports by Mr. Chadwick, collected by Dr. Richardson, show that he was undoubtedly the first person who made it his business to impress the nation with the fact that public health was a public question. From the official position occupied by Mr. Chadwick during the earlier years of the Queen's reign, he had an immense influence, which he exercised with all the energy of his nature, in bringing to the front the question of public health; and it may be safely affirmed that the remarkable Report of the Poor Law Commissioners in 1842, which was drawn up by Mr. Chadwick, laid down almost all the sanitary principles upon which the sanitary legislation of the last forty years has been based.

The Report of 1842 led to the Health of Towns Commission and other inquiries into public health, and paved the way for the Public Health Act of 1848. In one sense Mr. Chadwick was admirably adapted for this service. He was gifted with indomitable perseverance, and with a clear insight into what he wanted to obtain. He sought nothing for himself. His only object was to promote the views which he considered beneficial to the public, and to compel their adoption in whatever way he could. But unfortunately he was not gifted with that most valuable quality which may go a long way to secure results which talent alone may fail to obtain, viz. tact. Through this quality alone many of those changes and improvements, which necessarily injuriously affect some persons or classes of the community, can be brought into operation. Had Mr. Chadwick possessed tact, and been satisfied with obtaining reform in instalments and by slow degrees, he would probably have become one of the greatest powers in the country. But Dr. Richardson's description of the way in which Mr. Chadwick acted at the Poor Law Board shows how impossible it was for a man of his nature to remain long in a public department.

Whilst, however, Mr. Chadwick had the foresight to shadow out, in the Report published by the Poor Law Commissioners in 1842, all the improvements which have taken place up till this time, the working out of the various problems has been due to many others besides himself: and, prepared as we are to award a full meed of praise to Mr. Chadwick for his foresight and energy, by which he, and he alone, made the health of the nation a public question, we regret that the author of these volumes should have somewhat ignored the efforts of many of those who were mainly instrumental in raising the superstructure on the foundations laid by Mr. Chadwick. For it cannot be denied that in the early efforts at sanitation made by the Poor Law Commissioners, and enforced by the Board of Health, many grievous mistakes were committed. For instance, in the Report of 1842 the Poor Law Commissioners recommended, and their recommendation was largely adopted, that all refuse should be at once discharged into the drains and sewers, as the cheapest means of getting rid of it from the houses, although the sewers were avowedly at that time not constructed to remove the fæcal matter, and no provision was made to prevent it from lodging in them as fœtid mud, or passing into and polluting the water-courses: indeed, the Report stated that that danger was a smaller evil than the retention of refuse in the houses. This recommendation entailed a new class of evils, which has resulted in a very large expenditure and loss of life. No doubt the removal of refuse in this way was fairly simple, and certainly economical, until it created new evils whose remedy was very costly; but no one can say that the retention of the refuse in the houses might not have been prohibited, and the removal effected in some other manner, which, although possibly more expensive at the time, would not have been followed by disastrous consequences.

As an instance of the evils which the want of foresight entailed in the earlier introduction of the water-carriage system, the drainage of Croydon may be mentioned. This was executed directly according to the then views of the Board of Health. Soon after its introduction a most virulent fever broke out in Croydon, owing to the fact that the system totally ignored the ventilation of the sewers in any other way excepting into the houses themselves.

The application of sanitary science to practical life has arrived at its present state like most English matters, where action comes first and reflection afterwards; that is to say, in the elaboration of the early ideas at a great expenditure of money and experience, many blunders have been committed and many failures have ensued. The present condition of the practical application of sanitary science to the health of the nation rests upon the labours of many men. But although we may perhaps regret that Dr. Richardson's volumes attribute to Mr. Chadwick a larger share in the social changes which have taken place during the last fifty years than he is actually entitled to, yet all sanitarians are ready and willing to accord to him a very high place as a leader in the sanitary movement during Queen Victoria's reign. So long as he retained his office at the Poor Law Board, or in the General Board of Health, Mr. Chadwick laboured unceasingly to lay the foundations of our present system of public health; but in the erection of the superstructure we owe our gradual approach to practical perfection to many others, of whom it is only necessary to mention two or three.

Dr. Farr placed the vital statistics of the country upon a scientific basis. Mr. Humphry tells us that Dr. Farr received, in 1838, his appointment under the first Registrar-General, in consequence of his papers on benevolent funds, life assurance in health and disease, and various other statistical papers, and on the recommendation of Sir James Clark. Dr. Sutherland was an energetic worker in the Health of Towns Commission, and he, with Miss Nightingale, was the chief adviser of Mr. Sidney Herbert in his efforts to place army sanitation on a sound basis; and he has ever since continued as sanitary adviser of the War Office and India Office. Sir Robert Rawlinson is acknowledged to be the highest authority on modern sewerage. Sir John Simon began his admirable reports with the Public Health Act of 1848, and continued them until soon after the formation of the Local Government Board in 1875.

Having thus briefly mentioned some of those to whom credit should be given as prominent among the originators of the health movement which has prevailed in England during the last fifty years, we may consi what are the broad principles which underlie the repo and papers of Mr. Chadwick, edited by Dr. Richards and which, indeed, are the doctrines accepted to-c by most sanitarians. Practically, they advocate St socialism; and it is impossible to maintain large co munities in a due state of health and a due condition morality in any other way than under some form of St socialism. Our population is aggregating more and m into towns; but how little do we attend to the decenc or the amenities of life in the masses of population allow to assemble! A leading sanitarian some fo years ago wrote:—

"If there be citizens so destitute that they can affo to live only where they must straightway die—renting twentieth straw-heap in some lightless fever-bin, squatting amid rotten soakage, or breathing from t cesspool and the sewer; so destitute that they can b no water—that milk and bread must be impoverished meet their means of purchase—that the drugs sold th for sickness must be rubbish or poison; surely no civiliz community dare avert itself from the care of this abj orphanage.

"It may be that competition has screwed down the ra of wages below what will purchase indispensable food a wholesome lodgment. But all labour below that mark masked pauperism. Whatever the employer saves gained at the public expense. When, under such circu stances, the labourer, or his wife or child, spends an occ sional month or two in the hospital, that some fe infection may work itself out, or that the impending lo of an eye or a limb may be averted by animal food; when he gets various aid from his Board of Guardians, all sorts of preventable illness, and eventually for the e penses of interment, it is the public that, too late for t man's health or independence, pays the arrears of wa which should have hindered this suffering and sorrow.

"Before wages can safely be left to find their own le in the struggles of an unrestricted competition, the l should be rendered absolute and available in safeguar for the ignorant poor—first, against those deterioratio of staple food which enable the retailer to disguise starv tion to his customers by apparent cheapenings of bul secondly, against those conditions of lodgment which a inconsistent with decency and health."

Since these words were written it has been made t care of the community to remove refuse, to insure a go water-supply, to prevent adulteration of food, and to clo unhealthy dwellings; but many wretched dwellings exi and starvation wages still remain a disgrace to a coun which calls itself Christian. The whole of Mr. Cha wick's papers, and indeed the arguments of all the mo advanced sanitarians, are a protest against the doctri of "laissez-faire," which emanated from the school political economists in the earlier part of the centu And we are daily becoming more and more alive to fact that this doctrine of "laissez-faire" is incompati with the healthy existence of large communities. T form of socialism is one that should commend it to all thinking men, for it is quite certain that in th days of advanced intercourse and universal educatio helot class consisting of the many living in misery s by side with the few living in luxury is a condition things which cannot be permanently maintained. T fact that Mr. Chadwick was the first person to br this subject prominently forward and to compel Par ment to recognize that the public well-being is a public question, will always cause his name to be remembered with respect.

THE FORESTRY OF WEST AFRICA.

Sketch of the Forestry of West Africa, with Particular Reference to its Principal Commercial Products. By Alfred Moloney, C.M.G., of the Government of the Colony of Lagos. (London: Sampson Low, Marston, Searle, and Rivington, 1887.)

THIS, as its title indicates, is intended to form a handbook to the economic plant-products of Western Africa. Although the author is Governor of a British colony in this region, his remarks are by no means confined to British possessions, but are intended to include all that is at present known of economic interest connected with the plants of Western Tropical Africa.

Following Prof. Oliver, the author deems it expedient to divide Western Tropical Africa into two principal geographical regions. The first, called Upper Guinea, includes the western coast region from the River Senegal on the north to Cape Lopez immediately south of the equator; the interior drained by rivers intermediate between these limits, and the small islands of the Gulf, Fernando Po, Prince's Island, St. Thomas, and Annabon. The second region, called Lower Guinea, includes West Tropical Africa from Cape Lopez southward to the Tropic of Capricorn, including Congo, Angola, Benguela, and Mossamedes. Within the limits here indicated we have British possessions represented by "colonies" and "protected territories," and we have numerous possessions claimed by the French, Portuguese, Spanish, and German Governments, some of which have only lately been acquired in the European scramble for African territory. It is only right to mention that the term "possessions," as here applied, is somewhat a misnomer. There is little practically possessed, even by ourselves, except a slender coast-line: the interior is described as having no "territorial definiteness," and it is politically, no less than scientifically and commercially, unexplored. Capt. Moloney has wisely not attempted to treat separately of the economic products of these possessions. He has taken their present economic botanical productions in order of export value, and we find that these consist chiefly of palm oil, ground nuts, india-rubber, coffee, gum, dye-woods, cacao, cotton, fibres, and timbers. Palm oil, the produce of *Elæis guineensis*, a plant which covers immense tracts of country in Western Africa, is imported to this country to the value of nearly a million and a quarter annually. The yellow palm oil is obtained from the outside fleshy portion (sarcocarp) of the nut, while a white solid oil is obtained from the kernel. India-rubber is another West African product obtained chiefly from climbing vines belonging to the genus *Landolphia*. The author was one of the first to draw attention to the value of *Landolphia owariensis* as a rubber-plant, and it must be gratifying to him to find that the exports of "white African rubber," as the produce is called, have during the last four years risen from almost nothing to a value of nearly £36,000. What is known as "Yoruba" indigo, derived from a large tree, *Lonchocarpus cyanescens*, has evidently a commercial value, but at present it is used to mix with butter or "shea" to make the negroes' hair a fashionable gray!

Numerous West African plants are cited as yielding either gum tragacanth, copal, frankincense, gum-arabic, bdellium, or resin; what is called "ogea" gum, derived from an unknown tree, *Daniellia* sp., is used powdered on the body and as a perfume by women. The true frankincense-tree of Sierra Leone is *Daniellia thurifera*. Camwood, used largely as a dye, is derived from *Baphia nitida*; but although barwood is generally said to be derived from the same source, it fetches only one-sixth the price of the former. The medicinal properties possessed by numerous West African plants is a subject full of interest.

Various species of *Strophanthus*, the active principle of which was formerly used for poisoning arrows and is known to be of incalculable benefit in cardiac diseases, and the merits of the "miraculous berry" (*Sideroxylon dulcificum*) of the Akkrah and Adampe districts, which is credited with rendering the most sour and acid substances "intensely sweet", and of the "oro" plant of Sierra Leone, said to act as an irritant poison cumulative in its effects (which has been ascertained at Kew to be a species of *Euphorbia*), are among the numerous subjects requiring further investigation.

A most cursory glance at this book cannot fail to suggest the wonderful wealth both of botanical and industrial problems which are yet unsolved in connexion with West Tropical Africa. The "Flora of Tropical Africa," by Prof. Oliver, of which three volumes are published (the last in 1877), has made a beginning in the work of elucidating some of these problems; but in recent times few men have systematically pursued West African botany, and the entire absence of a resident botanist or of a properly-equipped botanical establishment in any of our West African colonies has left the plants of a most important region to be known only by the intermittent collections of travellers who have either perished there before their mission has been completed or have hastened home to avoid the effects of the deadly climate.

Nearly 200 pages of Capt. Moloney's book are taken up with condensed notes and references to the economic plants of Western Africa arranged in natural orders according to the "Genera Plantarum" of Bentham and Hooker. To many people both in West Africa and at home these notes, brought together by the assistance of an officer connected with the Kew Museums, will prove of great value. In the appendices are given a copy of the instructions for collecting plants, seeds, and useful plant-products issued by the Royal Gardens, Kew; an ornithology of the Gambia, by Capt. Shelley; a list of Coleoptera and of diurnal Lepidoptera of the Gambia, by the same writer; and a list of reptiles, batrachians, and fishes collected at the Gambia by Capt. Moloney in 1884-85.

The book is well got up and clearly printed, but it has the unpardonable defect of being published without a good alphabetical index. This greatly detracts from its value as a book of reference. It, however, is the chief fault we have to find with a work full of interesting matter for the first time brought together, and evidently prepared with great care.

D. M.

OUR BOOK SHELF.

Annals of the Astronomical Observatory of Harvard College. Edward C. Pickering, Director. Vol. xvii. (Cambridge: John Wilson and Son, 1887.)

THIS volume of the Annals of the Harvard College Observatory contains the description and theory of the instrument invented by Mr. S. C. Chandler, and called by him the almucantar, as well as the reduction and discussion of a series of observations made with it at the Observatory in 1884 and 1885. The instrument consists of a telescope mounted upon a base that floats in mercury, and the observation consists in noting the time of transit of a star across an almucantaral (or horizontal) circle, the particular horizontal circle which the inventor has found most convenient being that passing through the Pole, which he has called the "co-latitude" circle. If, therefore, the telescope be clamped at the given altitude, "the sight-line will mark accurately in the heavens a horizontal circle: and the transits of stars, as they rise or fall over this circle in different azimuths, will furnish the means of determining instrumental and clock corrections, the latitude, or right ascensions and declinations." Mr. Chandler believes that an instrument on the almucantar principle is capable of giving results more free from both accidental and systematic errors than those obtained from a meridian circle, and certainly the discussion of his observations contained in the volume before us goes far to justify such a belief. The probable accidental error of a single observation in zenith distance is $\pm 0''.404$, whilst for stars north of 60° declination it is as small as $\pm 0''.379$; the probable accidental errors of the clock corrections from a complete transit (including the residuals for Polar stars) are ± 0·047s. and ± 0·043s. for two observers. And these results have been obtained, it must be remembered, with a telescope of only 4 inches aperture and less than 44 inches focus. The chief advantage of the system is, however, that it gives measurements of both co-ordinates of a star which are absolutely free from the effects of flexure, and also of refraction as far as it depends on zenith distance. The almucantar certainly appears to be a valuable addition to our means of attacking difficult problems of practical astronomy.

The Distribution of Rain over the British Isles during the Year 1886. Compiled by G. J. Symons, F.R.S. (London: E. Stanford, 1887.)

MR. SYMONS explains that the delay in the appearance of this volume is due chiefly to the exceptional character of many of the phenomena of the year 1886, and partly to some observers not having had sufficient health, or courage, or interest in their records, to induce them to face the snowstorms of March 1 and December 26. The volume contains, besides articles upon various branches of rainfall work, the results of observations made at nearly 2500 stations in Great Britain and Ireland. In the various sections the compiler has brought together an immense mass of information, and he has taken great pains to present his facts clearly. There are several illustrations, in one of which he shows the fluctuations of annual rainfall from the year 1726 to 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. No notice is taken of anonymous communications.*

Slate Ripples on Skiddaw High Man.

THE slate ripples on Skiddaw are not, so far as I am aware, mentioned by writers on the Lake District, geological or otherwise. Their peculiar character puzzled me so much, af noticing them on Saturday, July 23, that I visited the spot ag on the 30th to see whether the origin which suggested itself me was probable.

Following the pathway from Keswick, you pass througl small gate a little way up the final ascent, from the dip betwe Skiddaw Low Man and High Man. Turning to the left alo the wire fence, one comes, where it ends, to the best-develop of these peculiar ripple marks; but they extend upwards fr here on the left (south) side of the pathway until you are m than half-way up to the first cairn. On the right (north) si the ripples begin later, extend higher, but are less distin They cease, apparently, simply from want of the clay foundatio which is an essential feature in their development.

The rippled areas are patches of bare clay or soil, from a f yards to half an acre or so in extent, coated with a thin layer the slates, which elsewhere form the cap of Skiddaw High Ma The slate fragments, however, instead of being confused, fo more or less regular lines, generally running north-west a south-east, but varying towards north and south or east and we when the patches are small and longest in these directions. T greater the slope the greater appears to be the average size of t slates. The larger fragments average a foot by four or f inches, always lying lengthwise along the lines, which are sev or eight inches apart. The clay is washed out beneath t stones, which therefore do not rise above the general level. T clayey intervals have numerous smaller bits of slate, and a scored at right angles to the lines by the action of rain a wind on these. Of course there are always loose fragments r on the chief lines.

Obviously the slates are arranged by the wind, apparen without much aid from water, as the slopes would not let it c lect. But it would be very interesting to have a compl explanation of the lines.

A suggestion, largely confirmed by my second visit, may any rate help to solve the point, even if it is inadequate by itse The hurricane force of Skiddaw storms, mostly from the sou west, no doubt drives before it the loose slates, sliding over t surface of the slates below. On reaching a bare patch, the fro edges of the slates are stopped by the clay. Finally a sudd gust tilts them over. Thus a first line is formed. More sla slide, or are tilted over, upon the first layer, which have mea while worked down to the general level by rain action. T second set slide over the first set and are in their turn tilted o on reaching the far side. Thus a second line is formed, and t rest follow in the same way. On slopes larger fragments a moved than on the level; hence such are there found in t lines. In small areas, with their long axes not perpendicular the prevailing winds, the general direction is modified by t natural position (according to the explanation here suggested) the first line.

There was a moderate gale on my first visit, and only a s breeze on my second, neither enough to move stones. But the latter occasion, hearing a strange hissing noise, I looked and saw a violent eddy, 20 or 30 yards across, whirling sm slates 20 to 40 feet into the air. This advanced from the sou west at the rate of 8 or 10 miles an hour, coming so close th some of the fragments fell around and on me.

Probably the lines are stationary, although the stones m pass from one to another. To test this, if possible, I took on the second occasion seven small Permian sandstone pebb from the shore and placed them a foot or less apart on the *win ward* edge of a conspicuous line, sheltering each behind a narr slate, hammered firmly into the ground. I am not likely to up again, but should any of your readers be on the spot a f months hence they might find the line in question by ascendi the path until the line of the Helvellyn range is above Skidd Low Man by about the breadth of a pencil held at arm's leng The line lies twenty-seven paces to the left of the path.

I might mention that the thermometer was at 45° on the t about half-past one or two, when the sun was clouded. Sc after four, at Crosthwaite, the same thermometer was at 63° the shade.

J. EDMUND CLARK

August 2.

Dr. Klein and "Photography of Bacteria."

ALTHOUGH I feel indebted to Dr. Klein for his appreciat of my work as expressed in his review in NATURE of Augus (p. 317), still I must ask him to allow me to correct a st ment which detracts very much from the credit of photo-micrography. Dr. Klein says:—"In connexion with this it must certainly appear remarkable that in the numerous and important publications on Bacteria by Koch and his pupils since 1877 to the present time we do not find a single illustration represented by micro-photography. All their published illustrations are drawings."

In addition to the works I have quoted (viz. Koch, "Mitth. aus d. K. Gesundheitsamt," Band I, 1881, illustrated with an extensive series of photographs; Hauser, "Ueber Faulnissbakterien," 1885, illustrated with an elaborately reproduced series; Van Ermengem, "Recherches sur le Microbe du Cholera," 1885, illustrated very successfully with twenty photographs), there are, since my work was published, Riedel, "Die Cholera," 1887, illustrated with most beautifully reproduced plates from negatives of comma-bacilli taken by Plagge and by Koch; "Zeitschrift für Hygiene," Zweiter Band, May 1887, two publications, illustrated with a series of photographs by the assistants at Koch's laboratory; Löffler, "Vorlesungen über Bacterien," May 1887, illustrated with reproductions from Koch's negatives. EDGAR CROOKSHANK.

24 Manchester Square, W.

THE LANDSLIP AT ZUG.

TO judge by the glimpses which I obtained of English newspapers during my late visit to the Alps, considerable misapprehension has prevailed in this country as to the nature of the disastrous landslip at Zug. For instance, one of the most important journals had a leading article on the subject, describing learnedly the fall of the Rossberg, the destruction of Plurs, and other like Alpine instances, with which the late calamity has no more connexion than the slipping of a piece of the Thames Embankment into the river would have with the fall of a peak of Snowdon. Hence, as I had the opportunity a short time since of visiting Zug, and in company with my fellow-traveller, the Rev. E. Hill, forming an opinion as to the cause of the accident, it may be worth while to give a few details. In drawing up this account I have used the abstract of a report by Prof. Heim, which takes the view which I had already adopted from examination of the locality, and has supplied me with a number of important details.

The newer part of the town of Zug stands on a plain which extends back from the lake to a considerable distance inland. Generally almost level, this at last shelves gently down, falling perhaps a dozen feet in the last hundred yards. The older part of the town occupies slightly rising ground between the water and hills which in England we should call mountains. Both parts, however, are not founded upon the rock, but upon a detrital deposit. Where are now the streets of Zug was once the lake: the streams from the adjoining hills have encroached upon its waters, and the town stands upon the delta which they have formed; the older upon the coarser more pebbly material, the newer upon the finer and sandy, where, in prehistoric times, the piles of lake-dwellings were driven.

A few years since the people of Zug thought to improve and beautify their town by building an esplanade in the place of the old irregular shore of the lake. It is faced by a wall of solid granite, which rests on a foundation of concrete, supported by piles. Outside this the water deepens rather rapidly: still no great depth is reached. Twenty metres from the edge of the quay it is 9 metres; at a distance of 100 metres it does not exceed 20, and even at a distance of 800 metres from the shore has only attained 45. The portion of the quay completed at the beginning of the present summer terminated for a time with a sort of bastion; north of that the piles had been driven for some distance, but no masonry had been laid. Rather more than 100 yards in this direction from the end of the new wall was a steam-boat pier, constructed as usual of wood.

Twice already in its history has Zug been the scene of disastrous landslips, once in the year 1435, and again in 1594; so that some few months back, when formidable cracks and indications of settlement began to appear in the new quay wall, considerable anxiety was aroused. Prof. Heim, among others, was consulted, and was not able, as a geologist, to offer much consolation, for he could only say that the foundation on which the whole place rested was, as will be seen, naturally defective. Still, as things had on the whole held together in the past, so, after this protest on the part of Nature, they might continue in the future. Certain remedial measures were suggested, and a careful watch was kept upon the new structures.

The catastrophe, however, occurred without further warning on July 5. Suddenly, about four o'clock in the afternoon, a large piece of land, occupied by houses and gardens, between the bastion and the steam-boat pier, seemed to break up, descend almost vertically, and become ingulfed in the lake. It was a scene of wild and awful confusion, unhappily not unaccompanied by loss of life. A steam-boat had just come up to the pier: the waves broke the hawser and drove the vessel more than a hundred yards back into the lake. Here, however, all

escaped unhurt, but the occupant of a small boat was upset and drowned, and the landlord of an adjoining restaurant, who had gone from his garden with some guests to see what was happening (for the ground seems to have gone in a series of quickly successive slips, not in one single fall), when the earth cracked beneath his feet, sprang in the wrong direction and was ingulfed in the muddy whirlpool. Three children also perished in one of the fallen houses.

Again about seven o'clock another and a larger slip took place; the destruction of property was greater, but this time without loss of life, for the people had taken the alarm and evacuated the houses. The dust from this ruin rose like a cloud, and was seen from the Rigi. Since then there has been no further slip; indeed, as we read, no further movement; for the cracks in neighbouring walls have been sealed up in many places, so that even a slight settlement could readily be detected.

The result of the landslip is as follows. A few months since there was a street in Zug running roughly parallel with the shore, terminated by a road leading to the steam-boat pier, and at the end, on the land side, was a good-sized hotel, while between the shed and the lake were gardens with cottages and other buildings. Where once were houses and gardens there is now a kind of bay of the lake. It is as though a pit had been excavated parallel with the shore, which, about 120 metres wide at the water-side, extends inland from 60 to 80 metres, widening as it does so on the eastern side to about 150 metres. This "harbour" is bounded by a low cliff, which rises gradually from a little above the water's edge to a height of about four yards; the surface, however, instead of being occupied by vessels, is a scene of the wildest confusion: slabs of pavement here, a pile of bricks there, the broken framework of a roof with its displaced tiles, a group of beams, some trees yet living, in one place the wooded gable of a house, project from the surface of the water, which is covered thick with timber and floating debris. A sadder scene of ruin it would be difficult to imagine. On the land side, part of the pavement of the street yet crests the little cliff, displaced near its edge by a series of vertical faults, with a throw of a few inches. Below, large slabs, with the squared blocks still in contact, lie at various angles on a slope of rubbish which just rises above the water. Houses, cracked and shattered, with their fronts in some cases partially fallen, look down on the scene of ruin, and not a few more in the neighbourhood are so injured that they will have to be rebuilt. It is stated that thirty-eight buildings were destroyed in the actual landslip, of which twenty-five were inhabited houses.

The cause of the landslip is made obvious by examination of the sections which the broken ground affords. That beneath the broken street will serve as an example. Under the pavement for about a yard is a stony deposit, the upper part probably made ground, the lower resembling a coarse gravel. As is natural, it is difficult to decide where undisturbed ground begins: it is enough to call the whole a stony soil, many of the fragments being from the size of the fist to nearly as big as the head. Probably, however, the lowest foot has been little disturbed. Then comes about fifteen or eighteen inches of a well-stratified gravel—rather iron-stained, the pebbles not exceeding a couple of inches in diameter; under this is about the same thickness of a rather peaty silt—either an old soil, or part of the lake floor, on which aquatic plants have grown; for what seem to be dead rootlets are abundant. Then comes a thick mass of gray silt. It extends downwards below the level of the lake—probably to a depth of many metres. This it is which has been the prime cause of the catastrophe. The thick substratum of silt, at times little better than a quicksand, has always formed an unsafe foundation. Too heavy a load, either locally by building too large a house, or generally by building many smaller dwellings, any weakening of the cohesion of the mass, exceptional seasons,[1] may at any time suffice to pull the trigger of a weapon which, so to say, is always charged. It is doubtful whether this part of the town can ever be regarded as absolutely safe: at the same time there have been but three slips in four centuries and a half, and no doubt precautions will be taken to reduce the danger to a minimum. It is possible that the building of the esplanade has been the immediate cause. Prof. Heim, however, does not so regard it, though I cannot say that his arguments entirely satisfied me. However, this is certain, that of the completed building only a few feet were damaged; the frontage which slipped was that into which piles alone had been driven.

The most remarkable thing about the slip is that the displacement has been nearly vertical. There has been but little outward lateral movement of the ruined buildings. As Prof. Heim words it in the above-named repor "Ground which formerly was from 6 to 2 metres abov the water is now from 2 to 6 metres below it." The silt substratum must have flowed outwards into the deepe water, or in some way been displaced laterally to allow o the surface thus sinking. In accordance with this it i stated that the piles driven for the new wall—which wer fixed in the silt alone—were thrust outwards for distance of from 100 to 300 metres from the shore, and wer pushed up above the level of the water. The catastrophe then, cannot be numbered with the bergfalls, or even wit the ordinary landslips, though perhaps an analogy ma be established with some sea-side slipping of cliffs; but i is none the less lamentable, for, in addition to five death many families have lost their all—goods, house, and eve the site itself being destroyed; and great additional ex penditure will be required before the neighbourhood ca be regarded as safe.

T. G. BONNEY.

[1] It is stated that the weather changed on the evening of July 5: storms and rain succeeding to a long period of dry weather. At the time the "ground water" beneath the town was rather above, the lake rather below, its usual level.

THE NORWEGIAN NORTH ATLANTIC EXPEDITION.

NOT surpassed by the records of the Austrian Novar Reise, nor by those of our own *Challenger* Exp dition, is the account of the Norwegian Expedition to th North Atlantic, the latest part of which is a Report o the Alcyonida, by D. C. Danielssen. Like the other par of this Report the present forms a quarto or rather sma folio volume, and contains over 160 pages of text with plates and a map giving the details of the geographic distribution.

The author was one of the staff on board the *Vøringe* and he now has the pleasure of describing the specime collected, but he has not had the assistance of that exce lent zoologist (Koren) whose able work on the Alcyoni of Norway had been executed in partnership wi Danielssen, and whose death all those interested natural science have to deplore.

The Alcyonids collected during the Norwegian Exp dition are almost exclusively deep-sea forms; the dept varying from 38 to 1760 English fathoms. Among the there are no less than nine new genera, which all belo to the sub-family of the Alcyoninæ, with 33 new speci of which two belong to Clavularia, one to Sympodiu one to Nidalia, and the rest to the several new gene There is also a new sub-family with a new genus a species described.

The author says quite truly, that, of all the large grou of the Alcyonaria, none have been treated more supe ficially by recent zoologists than that of the Alcyoni No doubt there are many reasons for this; the delica of their structure, combined with the difficulties of th preservation in a state for minute investigation, has some extent made their study a difficult one; and ev the repeated endeavours of Mr. Danielssen to obser them in a recent state were unsuccessful. In regard classification, the author for the moment follows that Milne Edwards; in this we think he is correct, and thoroughly agree with his reasons; for until the prese material in the museums of Europe and America has be properly worked out, and much fresh material has be collected, any attempt to give a definite classification the group will be so much lost labour.

In the diagnosis of the genera and species, especia of the latter, the form of the spicules, as well as th arrangement and position on the polyps, have been fou of great value, though minuter histological details ha not been used as much as they possibly will be in t near future. One very important and interesting fact mentioned, viz. the discovery in a species of a new gen Vœringia of a nervous system. On the uppermost p of the ventral surface of the œsophagus there is to be found a group of large ganglion cells containing extremely large nuclei with viscid protoplasm and prolonged filaments. Mention is also made of the grooves lined with long flagelliform cells, which, however, were some time since described by Hickson in a paper published in the Philosophical Transactions under the name of "Siphonoglyphe."

Another novel phenomenon was observed in a species of Nephthya, where several of the polyps seemed to be solely reproductive, and in them as soon as fertilization was effected, the tentacles became incurved over the oral aperture, which then became plugged with a viscid mucous, and apparently during the gravid period these polyps were nourished by the other polyps of the colony.

We must content ourselves with giving but a very brief summary of the forms described. The genus Vœringia is established for a series of branched Alcyonids with retractile polyps, in this differing from those of Duva; eight new species of this genus are described, to which also the *Alcyonium fruticosum*, Sars, is referred. Eight new species of the beautiful genus Duva are recorded. A new genus, Drifa, is established for an arborescent species, the spicules in which differ from those of both Vœringia and Duva; of the two species, one, *D. islandica*, exhibits an interesting structure; around the mouth and between its external opening and the base of the tentacles, there are eight little fringe-like protuberances, which form a ruff. An appearance of the same kind, only outside the circle of the tentacles, we have observed in a Plexaurid, but we are not certain but that it may be due to the sudden immersion of the polyps into strong spirits. For a graceful arborescent form with auto- and siphonozooids, which reminds us of Anthomastus, Verrill, the genus Nannodendron is proposed; the polyps are completely retractile. *Fulla schiertzi* is a new genus and species of another branching form with a somewhat flattened stem, showing a distinct bilateral symmetry, the branches only springing from the opposite sides of the main axis. Three new species of Nephthya are enumerated. For a species in which in addition to a well-marked siphonoglyphe there are also in the first part of the œsophagus two flap-like protuberances, the genus Gersemiopsis is made. The only species, *G. arctica*, was dredged in a depth of 658 fathoms. A new genus, Barathrobius, is made for two new species, in which the basal part of the colony is hard and often dilated, the polyps are retractile, appearing only, when fully withdrawn, as slight elevations above the mass of the branches. *Sarakka crassa* (n. g. et sp.) is a species with a very peculiar structure in its œsophagus, which seems to be constricted laterally into two independent portions; while *Crystallofanes polaris* is a form with few polyps on the stem but with a summit rich in polyps, borne on short branches which are placed in whorls round the stem; the polyps are retractile.

A new sub-family is made for a new genus and species *Organidus nordenskjoldi*; in this species the polyp cells are long, connected together so as to form an axis; these polyp cells are long, cylindrical, calcareous, with both the polyp body and its tentacles well provided with spicules. The author thinks that this sub-family shows some affinity to the Tubiporinæ, but it would appear to us to show more relationship to such forms as Gersemia and Eunephthya. *Clavularia frigida* and *Sympodium abyssorum* are described as new species.

This memoir is published in both Swedish and English, in parallel columns, for which the student cannot be too thankful; true, the English may strike the reader as a little quaint, and in the nomenclature of the spicules it is somewhat novel, but criticism would be out of place in the presence of so great a boon. The day is coming when a new classification of the spicules of the Alcyonaria must be made; at present, while new types are constantly being discovered, any such would be but premature, and we must be content with that laid down for us by Kölliker. Had the value of the labours of Valenciennes been properly appreciated, this might not now be the case. The almost overcrowded plates have been drawn by H. Bucher, Jun., with all that skill which we have before admired, though perhaps the drawings of the spicules convey too much the notion of their being perfectly solid.

We shall wait with great expectancy the publication of future memoirs of the other families of the Alcyonaria.

THE COLOURS OF THIN PLATES.[1]

THE physical theory, as founded by Young and perfected by his successors, shows how to ascertain the composition of the light reflected from a plate of given material and thickness when the incident light is white; but it does not and cannot tell us, except very roughly, what the *colour* of the light of such composition will be. For this purpose we must call to our aid the theory of compound colours, and such investigations as were made by Maxwell upon the chromatic relations of the spectrum colours themselves. Maxwell found that on Newton's chromatic diagram the curve representative of the spectrum takes approximately the simple form of two sides of a triangle, of which the angular points represent a definite red, a definite green, and a definite violet. The statement implies that yellow is a compound colour, a mixture of red and green.

In illustration of this fact, an experiment was shown in which a compound yellow was produced by absorbing-agents. An infusion of litmus absorbs the yellow and orange rays; a thin layer of bichromate of potash removes the blue. Under the joint operation of these colouring-matters the spectrum is reduced to its red and green elements, as may be proved by prismatic analysis; but, if the proportions are suitably chosen, the colour of the mixed light is yellow or orange. When the slit of the usual arrangement is replaced by a moderately large circular aperture, the prism throws upon the screen two circles of red and green light, which partially overlap. Where the lights are separated, the red and green appear; where they are combined, the resultant colour is yellow.

On the basis of Maxwell's data it is possible to calculate the colours of thin plates and to exhibit the results in the form of a curve upon Newton's diagram. The curve starts at a definite point, corresponding to an infinitely small thickness of the plate. This point is somewhat upon the blue side of white. As the thickness increases, the curve passes very close to white, a little upon the green side. It then approaches the side of the triangle, indicating a full orange; and so on. In this way the colours of the various orders of Newton's scale are exhibited and explained. The principal discrepancy between the curve and the descriptions of previous observers relates to the precedence of the reds of the first and second orders. The latter has usually been considered to be the superior, while the diagram supports the claim of the former. The explanation is to be found in the inferior brightness (as distinguished from purity) of the red of the first order, and its consequent greater liability to suffer by contamination with white light. Such white light, foreign to the true phenomenon, is always present when the thin plate is a plate of air inclosed between glass lenses. To make the comparison fairly, a soap film must be used, or recourse may be had to the almost identical series of colours presented by moderately thin plates of doubly-refracting crystals when traversed by polarized light. Under these circumstances the red of the first order is seen to be equal or superior to that of the second order.

[1] Abstract of Lecture delivered by Lord Rayleigh at the Royal Institution on March 25, 1887.

FIFTY YEARS' PROGRESS IN CLOCKS AND WATCHES.—I.

HOROLOGY being one of the oldest arts anb branches of science, it is almost] inevitaeld that advances in it should be of a mediocre and modest character, and not of a nature to claim great attention in these days of startling and sensational discovery. But nevertheless during the period we refer to much good work has been done. In chronometers, the secondary compensation error has been discovered and means found to rectify it. In clocks, the same has been done for the barometric error. Moreover, the difficulties connected with the correct working of gravity escapements have been overcome; so that scarcely a good turret clock is made without one now. Electricity also has been largely applied for driving or controlling clocks, or for controlling chronometers; and the measurement of minute fractions of a second has been attained by chronographic appliances of extreme accuracy. Articles explanatory of these subjects have appeared in the pages of NATURE[1] from time to time. In addition there has been a mass of subsidiary improvements which it is impossible to classify, and of which we shall have to describe the leading features in a somewhat desultory manner in the succeeding pages.

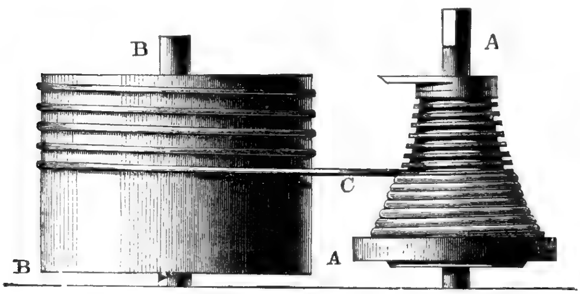


FIG. 1.—Barrel and Fusee.

Naturally, the first subject to claim our attention is that important mechanism which enables us to wind up and

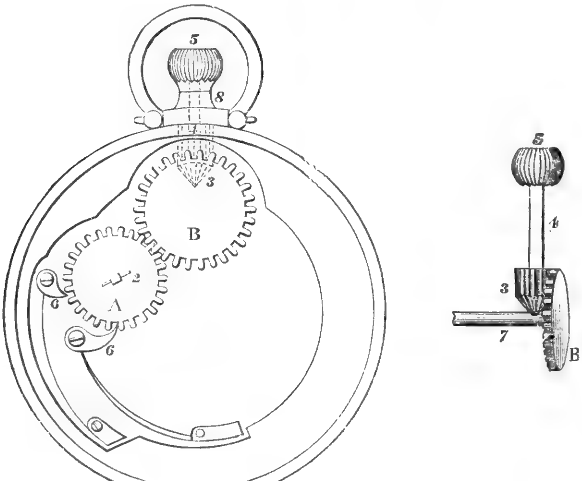


FIG. 2.—Prest's Keyless work.

set the hands of our watches without a key. And it is to be remarked that its introduction has led to the almost general abandonment in watches of the fusee (A A, Fig a contrivance of considerable antiquity, a picture of wh used to appear in nearly every popular book on mecha

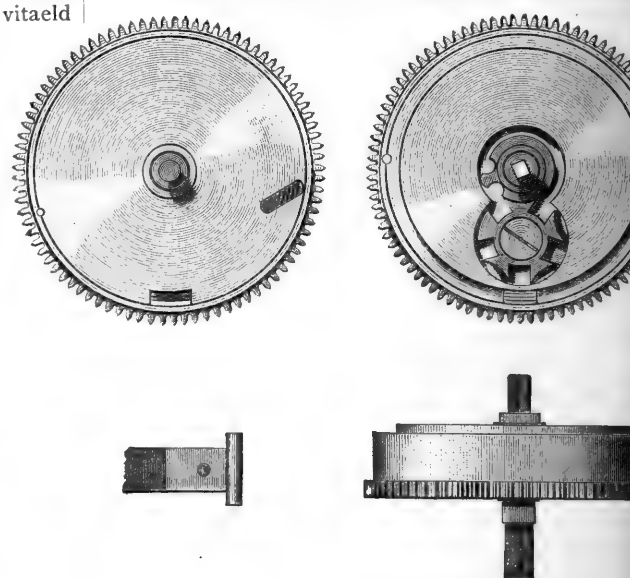


FIG. 3.—Going Barrel and Stop-work.

a few years ago. The discovery of such mechanism not made all at once; at first it was applied solely for

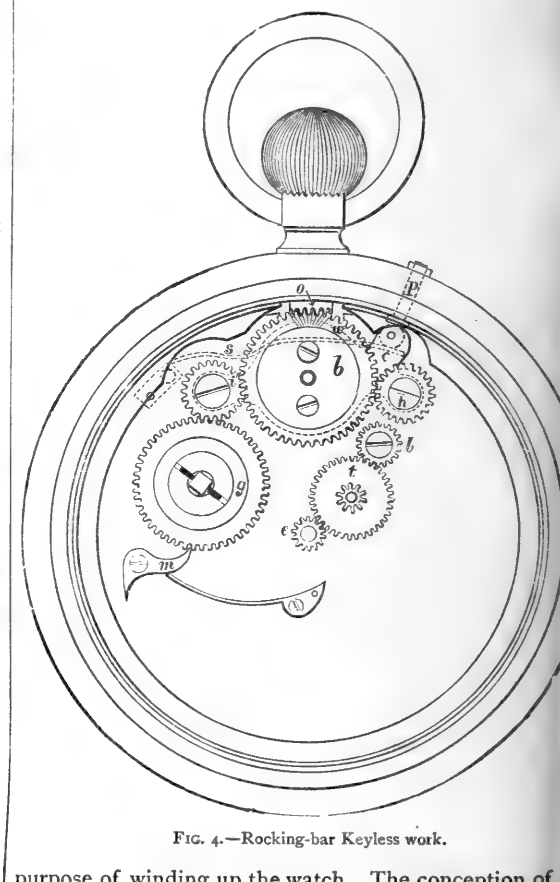


FIG. 4.—Rocking-bar Keyless work.

purpose of winding up the watch. The conception of present form of winding from the pendant is due to Pre

[1] See vols. xiv. pp. 529, 554, 573; xv. 9; xx. 345; xxiii. 59; xxvi. 107, 369.

of Chigwell, and Fig. 2[1] shows his plan. 2 may be considered the same as the square to which the key would be applied in winding, and the wheel A is fastened to it and is geared to B, which in turn engages the pinion 3, which is part of the stalk 4, passing through the pendant 8, and terminating in the crown-piece 5. On turning 5, A will revolve, winding up the watch in doing so; the clicks 6 6 prevent A from returning. It is now clear why the fusee must be dispensed with. With a fusee (whilst the watch is going) the square which you wind travels backwards, and it would naturally turn the crown-piece in doing so; this latter, meeting with resistance in the pocket, would obviously stop the watch. Fig. 3 shows the mechanism which takes the place of the fusee. It will be seen that the main wheel is attached to the barrel; the shaft (squared at its extremities) which passes through the barrel is connected with the main-spring; when the shaft is turned the main-spring is wound. The shaft, being held by the intervention of the clicks, cannot return, and the outside of the barrel being urged to follow it by the pulling of the main-spring, impels the main wheel and drives the train. Overwinding is prevented by means of the star wheel and finger-piece shown in the diagram. Every turn of the shaft causes the star to move on one division, but on passing the last division the circle, out of which the finger is cut, meets a convex instead of a concave surface, and further movement is arrested. There is much less difference between the pull of the main-spring when the watch is wound up and nearly down than might be expected. To obtain as much uniformity as possible a long thin main-spring is used, it is tapered, and very few turns of it are brought into service.

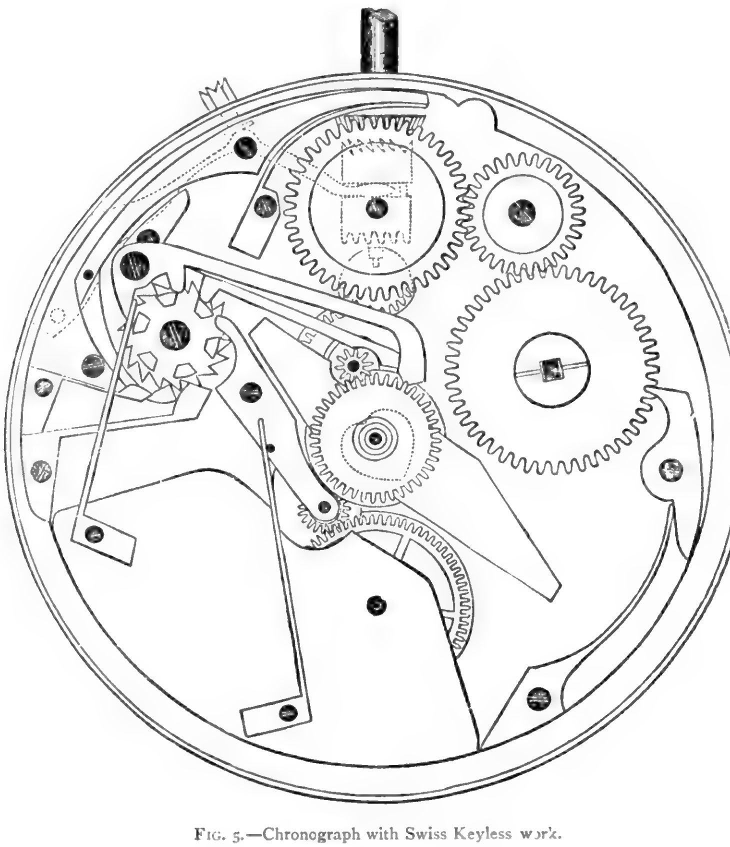


FIG. 5.—Chronograph with Swiss Keyless work.

About twenty-five years elapsed before any satisfactory method was established of causing the keyless mechanism to set the hands of the watch in addition to winding it. The method which was first adopted had the drawback that the hands could not be put backward when the watch was fully wound. At present two systems are principally employed, they are known as the English and Swiss. Fig. 4 shows the English or rocking-bar plan. Wheel *b* is in connexion with the crown-piece, and communicates with the square of the shaft passing through the barrel by means of the wheels *i* and *g*. Wheels *i* and *h* are on a lever, or rocking-bar, pivoted about the centre of *b*. *p* is a push piece acting against *c*, which is a part of the rocking-bar. When *p* is depressed by the finger or thumb, it lifts *i* and forces down *h* into connexion with *l*, which communicates with the pinion of the minute-hand *e*. If the crown-piece now be turned, the hands will follow; no winding is performed, because *i* has been lifted away from *g*. When the pressure is removed from *p*, a spring, *s*, puts the rocking-bar back again into its normal position, *i* engaging *g*, and *h* quitting *l*. The Swiss system is different in this: that connexion with the winding or set-hands wheels is made by a pinion faced with teeth on both sides, sliding up and down the stalk of the crown-piece. The normal position (as in the English system) is engagement with the winding-wheels, but when the push piece is depressed the pinion moves away from its engagement with the winding-wheels, and

[1] We are indebted to Mr. David Glasgow, Vice-President of the British Horological Institute, and the Messrs. Cassell for the use of Figs. 2, 3, and 4, and to Mr. F. J. Britten, Secretary of the British Horological Institute, for the use of Figs. 5, 6, and 7.

takes up with the set-hands wheels. In Fig. 5, which we shall refer to again further on, this arrangement can be readily perceived.

To understand repeating work—in which a good deal of progress has been made—it will be as well at first to refer to Fig. 6, which shows the mechanism of a clock chiming the quarters. On the left will be seen an anchor-shaped piece with teeth in it, called a "rack." At the foot of the rack will be seen a star wheel carrying a piece in form similar to a snail. This piece is called the "snail," and it has twelve gradations corresponding to the twelve hours. On the right will be seen another rack and snail which do duty for the four quarters. Both the quarter and hour racks are at present held free of their respective snails by the hooks shown in the diagram. The method of action is as follows:—At each hour the quarter rack, by means of mechanism connected with the going train of the clock, gets itself liberated from the hook and falls upon its snail. The distance through which it falls is determined by the depth of the depression in the

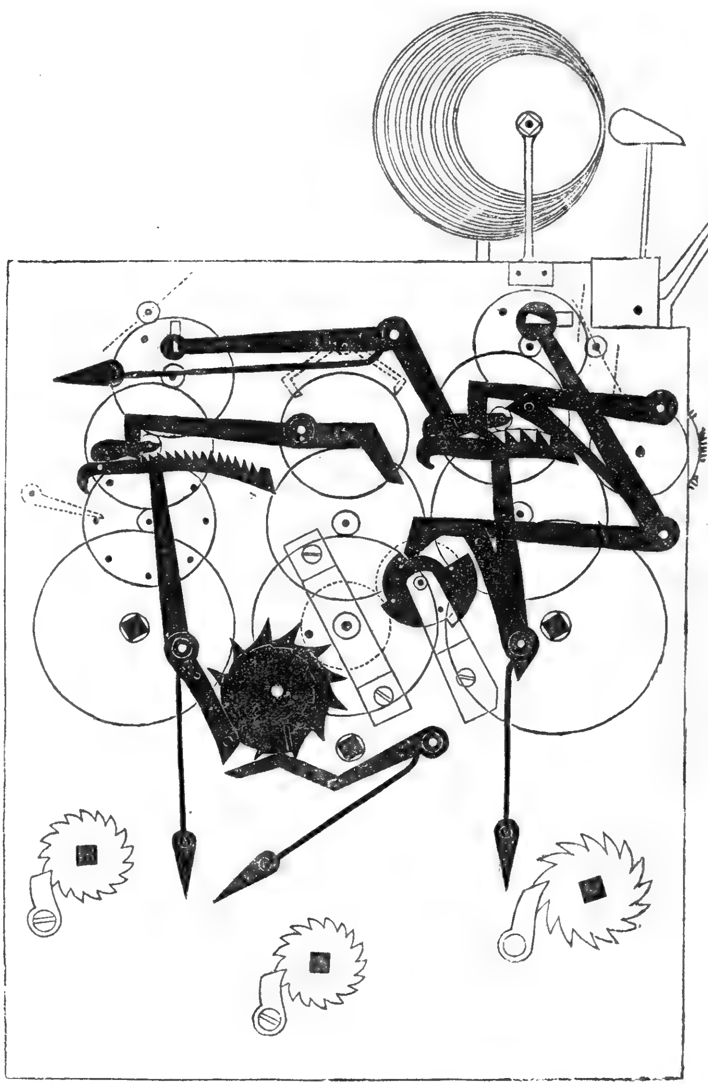


Fig. 6.—Hour and Quarter striking mechanism.

snail which is opposite to it. The quarter train having also got freed at the same time, proceeds to run, and winds up the rack again in doing so. The distance through which the rack has fallen determines the length of time the quarter train runs, and consequently the length of the chime. In falling the quarter rack also discharges the hour rack. The hour train is held until the quarters are finished; at their conclusion the hour rack is wound up by the hour train through the distance it has fallen which depends upon the depth of depression in its snail opposite to it), and the number of the hour struck is in proportion.

The light which the foregoing throws upon repeating work is with regard to the snail and rack arrangement. When you move the slide of a repeating watch you do two things. You wind up the main-spring, which actuates the repeating train, and the extent to which you are able to do so depends upon the depth of the depression in the snail which is opposite to the piece which you are moving. When you reach the bottom and press against the snail, it is so arranged that the snail shall give a little. The small play the snail has, the distance it can move under pressure, is sufficient to discharge the quarter rack on to its snail. In repeating work the quarter rack is also an "all or nothing piece," for this reason, that until it is discharged the hammer which strikes the hours is hung up, and should you not press down the slide sufficiently to reach the bottom of the depression in the snail no blows are struck, so it is not possible for the repeating work to give you a false answer. Fig. 7 illustrates repeating action. Clock-watches are watches which strike the hours and quarters spontaneously; their action is exceedingly complicated, and, unless their mechanism is seen, is almost incapable of explanation.

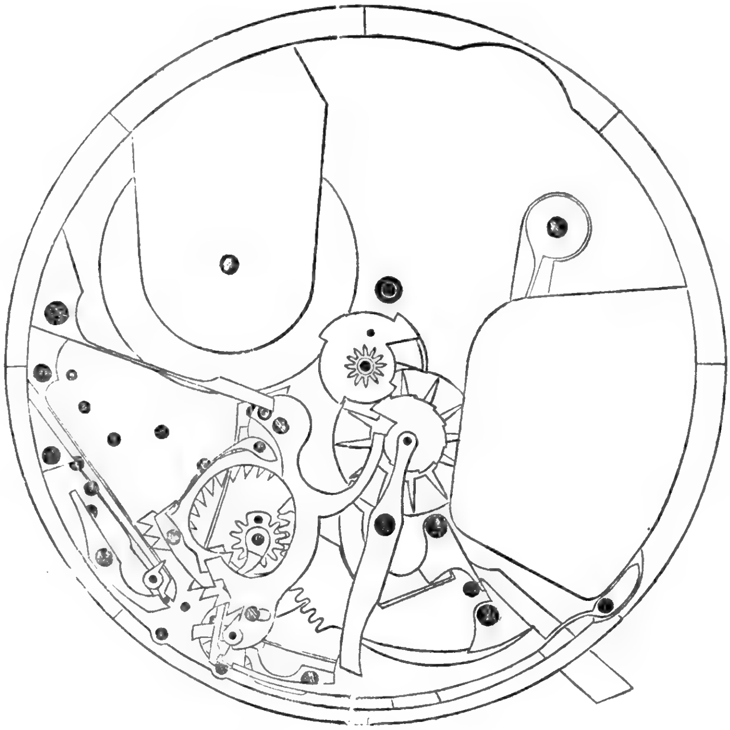


FIG. 7.—Repeating mechanism.

HENRY DENT GARDNER.

(*To be continued.*)

THE RECENT DROUGHT.

THE spell of dry weather recently experienced over the United Kingdom has been so unusually prolonged, and its effects have in many instances been so disastrous, that a brief inquiry into its history and general results may not be without interest. In the present article it is therefore proposed to take into consideration,—firstly, the conditions of barometrical pressure under which the drought occurred; and secondly, the actual deficiency of rainfall experienced in various parts of the country.

With respect to the first point it will readily be surmised by those who are in any way acquainted with the subject of our meteorological changes that the general distribution of pressure during the recent dry spell was anticyclonic. At times, and notably during the second half of June, the middle of July, and the early part of August, the anticyclonic conditions ruled supreme over the entire Kingdom. On other occasions, however, the influence of the high-pressure areas was confined to a portion of our islands, the favoured localities being usually those included within the eastern or the southern half of Great Britain. With these latter conditions the extreme western and northern districts were influenced to a very partial extent by the anticyclone, and to a much greater extent by areas of low pressure, the centres of which were, however, in nearly all cases at a considerable distance from our shores. On a few rare occasions the main disturbances were accompanied by shallow subsidiary depressions, which advanced directly over us, and occasioned the temporary bursts of showery weather which occurred from time to time. The most important and general instances of this kind were observed during the second week of July and towards the end of the same month; but in the former case there were isolated portions of our southern and south-eastern counties which remained altogether unaffected by the disturbed weather, while in the latter instance the showers were in many districts far too insignificant to be of any real value.

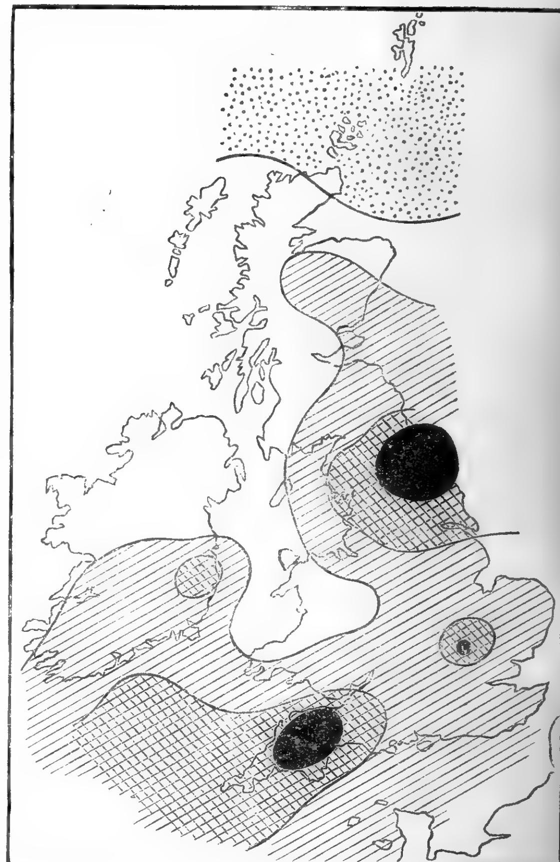
Although an endeavour has thus been made briefly to account for the unusual drought which occurred, one cannot but feel that beyond and irrespective of the various pressure movements which were reported from time to time there was a distinct *tendency* for the weather to remain dry and warm. Instances were not wanting of the prevalence of very disturbed conditions of pressure without any corresponding break up in the atmospherical appearance. Of this, two recent examples may be cited. On the afternoon and evening of August 12, a depression formed directly over England, while another appeared over Ireland. Under such circumstances a good deal of rain might naturally have been anticipated in all districts, and in ordinary seasons there can be no doubt that it would actually have fallen. As a matter of fact, however, in those parts of England which lay directly under the influence of the growing depression, the weather remained persistently fine, the appearance of the sky even giving but little indication of the atmospherical change which was in progress. On the evening of August 11 a similar though not quite so decided a movement in pressure also passed over without any rain in the districts more immediately concerned. The subtle influence which determines whether a season shall be dry or wet, hot or cold, is at present a profound mystery, but that something of the kind exists is abundantly evident to all who have endeavoured to work out the causes of our seasonal weather changes.

As regards our second point of inquiry, viz. the actual nature and extent of the recent drought, the meteorological records tell a most remarkable tale. The period embraced by the spell of dry weather began with the early part of June, and lasted in most districts until the middle of the present month, or about eleven weeks in all. I have therefore taken the trouble to abstract and total for this period the rainfall values given in the Weekly Weather Report of the Meteorological Office for 78 stations situated in various parts of the United Kingdom. The general results of the investigation are shown in the accompanying map.

A very brief examination of the map will suffice to show that during the extended period in question the aggregate rainfall was less than the average in all parts of the British Islands, with the exception of the Shetlands and a portion of Caithness. Over England (including also South Wales), the eastern and central parts of Scotland, and the southern half of Ireland, the total amount of rain was less than half the average. Over the north of England, the county of Hertfordshire, the greater part of the south-western district, comprising the counties of Somerset, Devon, and Cornwall, and a small tract of country surrounding Dublin, the aggregate amount was less than a third of the average; while in the north-east of England, in portions of Devon and Cornwall, and at Rothamsted, the rainfall did not amount to one-fourth of the average. The only exceptions to these general rules occurred over some portions of Eastern England, where, owing to heavy local thunderstorms, the aggregate was much greater than at surrounding, or even at neighbouring, stations. At Attleborough, in Norfolk—a station which is not included in the official report, and which has therefore not been employed in the preparation of the map—as much as 2·03 inches of rain were measured on July 31 in the short space of an hour and a half. A very similar local plump occurred at Ingatestone in Essex, where, during a severe thunderstorm on July 16, a fall amounting to 1·8 inch was recorded in about two hours. So far as I am aware, no local falls of anything like so heavy a nature were experienced either over our midland or our southern couties.

With regard to the frequency, or rather in the present instance to the rarity, of the summer rainfall, it appears that over the greater part of the midland, southern, and south-western districts the number of rainy days was less than 15 out of a total of 77. At Cirencester, Hastings, and Southampton, the number did not amount to more than 12, at Oxford to 11, and at Hurst Castle to 10; while at Dungeness there were only 8 days with rain, or about one-fourth of the average number. In the south-west of England the number of rainy days varied from 13 to 25, in the north-east from 15 to 23, and in the north-western district, including North Wales, from 19 to 26.

One other very important feature in connexion with the drought has been the prevalence of unusually long periods of absolutely rainless weather. Between the early part of June and the beginning of July there were in many of the English districts as many as 25 to 28 consecutive days without rain, and in some localities these numbers were greatly exceeded. At Falmouth, for example, there was no rain between June 7 and July 7, a period of 31 days, while at Dungeness and Cullompton there were periods of 35 rainless days, lasting in the former instance from June 4 to July 8, and in the latter



Districts in which rainfall was in excess of the average... ...

" " was less than half the average ...

" " was less than one-third of the average

" " was less than one-fourth of the average

from June 3 to July 7. As regards the London district it appears that the drought in its absolute sense was longer than any experienced since the year 1865. Between the first week in June and the beginning of July there were in London 25 consecutive days without rain; in June 1865 the number was 26. In the period of 21 years intervening between 1865 and the present year there were only three instances of an absolute drought lasting for as long as three weeks.

FREDK. J. BRODIE.

THUNDERSTORM IN LONDON.

An exceptionally severe thunderstorm was experienced in London and the suburbs on the evening of the 17th inst. It commenced with distant thunder at about 5.30 p.m., and by 6 o'clock the storm was fully over the southern suburbs. The lightning was very vivid, and the flashes were very frequent, following each other occasionally with but an interval of a few seconds. The thunder was very heavy, and at times quite deafening, the crash often following the lightning-flash almost instantaneously. The greatest violence of the storm occurred between 6.30 and 8 p.m., throughout the whole of which time the lightning and thunder were most intense. Thunder was heard till 9.30 p.m., and distant lightning seen till 10 p.m., so that the storm was over London for about four hours and a half. There was no evening as far as daylight was concerned, night setting in at the close of the afternoon, and the heavy clouds which covered the sky had the appearance of being doubly massive in contrast to the lightning as the flashes illumined the whole sky. The rain which accompanied the storm was very heavy, but the fall varied very considerably in different parts of the metropolis. Unfortunately at present the measurements at hand are by no means numerous, but a careful discussion of the rainfall of this storm would probably be of considerable scientific interest. The falls as yet available are: Brixton Hill 2·02 inches, Camden Town 1·42 inch, Clapham 0·97 inch, Greenwich 0·54 inch, Westminster 0·50 inch, and East Finchley 0·16 inch. At Brixton Hill the rain was intensely heavy for twenty minutes from about 6.10 to 6.30 p.m., during which time by far the larger part of the fall occurred; the observer not being on the spot until later in the evening, measurements were not made during the progress of the storm. There is ample evidence, however, to confirm the heavy fall at Brixton, as the roads were flooded in parts to the depth of from 12 to 18 inches, and the water rushed down the roadways with such force that it was thought a large reservoir had burst. Mr. Wallis, writing from the head-quarters of the "British Rainfall" at Camden Town, states that the total fall there was 1·42 inch, and heavy rain did not commence till 6·30 p.m. He gives the following rates of fall:—7 to 8 p.m. 1·24 inch, 7 to 7.30 p.m. 0·45 inch, 7.30 to 8 p.m. 0·79 inch; in 22 minutes, from 7.42 to 8.4, the amount measured was 0·66 inch; and in 10 minutes, from 7.45 to 7.55, the heavy fall of 0·50 was measured. The primary cause of the storm was due to a somewhat shallow barometric depression, the mercury at the centre standing at 29·7 inches, which passed completely over London during the evening.

This disturbance was central over the north of Devon at 8 a.m. 17th, and by 8 a.m. 18th was situated over North Germany, but from some cause, not yet understood, its rate of travel when passing over London was very much slower, and its energy more intense, than at any other stage of its existence. The weather had been dry during the first twelve days of August, as well as at the close of July, especially in the southern and eastern districts of England, where, indeed, a second drought, during the present summer, had prevailed, but which was much less marked than the drought of June and the early part of July, but yet severe, following as it did so closely on its predecessor, with so small a fall of rain intervening. After the 12th, however, the weather over England became disturbed, and the anticyclone which had prevailed gave place to cyclonic conditions, and a series of disturbances passed over our islands; it was one of these which resulted in this severe thunderstorm. Very little rain fell over the country generally in connexion with this storm, but other falls of rain occurred in many places about this time. In London, as well as in the Midlands, and the southern and eastern districts of England, a thunderstorm had been experienced in the early morning of the same day; the total fall of rain in London, as the result of the two storms, was 2·62 inches, a fall 0·34 in excess of the total average for August, all of which fell in less than twenty-four hours. CHAS. HARDING.

SPENCER F. BAIRD.

The news of Prof. Baird's death will be received by English naturalists with the most profound regret, the more so as no intimation of the indisposition of the celebrated American man of science had been communicated to his friends in this country, and the intelligence was therefore unexpected. By Englishmen who knew Prof. Baird personally the loss must be especially felt, but there are many who had never met him in the flesh, to whom the news of his decease must come as that of a dear friend. As one of the latter class, we venture to express our sympathy with our scientific brethren in America on the decease of one of their most eminent and respected colleagues. As chief of the Smithsonian Institution, Prof. Baird possessed a power of conferring benefits on the world of science exercised by few directors of public museums, and the manner in which he utilized these powers has resulted not only in the wonderful success of the United States National Museum under his direction, but in the enrichment of many other museums which were in friendly intercourse with the Smithsonian Institution. We know by experience that the British Museum is indebted beyond measure to Prof. Baird, and we need only refer to the recent volumes of the "Catalogue of Birds" to show how much our national Museum owes to the sister Museum in America for hearty co-operation. We had only to write and express our wants, and immediately every effort was made, by Prof. Baird's instructions, to supply all the desiderata in our ornithological collection, and this without the slightest demand for an equivalent exchange, though of course in the case of the British Museum every effort was made to reciprocate the good feeling shown towards that institution by the great American Museum. There must be many private collectors in this country who will indorse our acknowledgments to Prof. Baird for the unrivalled liberality which he has always shown in the advancement of the studies of every ornithologist who invoked his aid.

Of the celebrated trio, Baird, Cassin, and Lawrence, who together wrote "The Birds of North America," the last-named naturalist is now the only survivor, but Baird lived long enough to see the results of that great undertaking, which placed American ornithology on a sound working basis, and established an era from which progress has been both sound and rapid, until there is perhaps no country in the world where birds have been so thoroughly and scientifically studied as in America. This result is undoubtedly due to the influence of Prof. Baird in directing the scientific studies of his colleagues in the New World. His "Review of North American Birds" is really a wonderful work, and, though published twenty-five years ago, is of the greatest service to students of Passerine birds at the present day. Our only regret is that it was never completed. The celebrated paper on the distribution of North American birds, published in 1867, laid the foundation of the division of the Nearctic Region into natural sub-regions, which the multitudinous labours of travellers in recent years have tended to elaborate and confirm. Prof. Baird's last great effort in the cause of ornithology was the publication of the "History of North American Birds," in conjunction, this time, with Robert Ridgway and T. M. Brewer.

After the completion of that important work he was occupied chiefly with his duties as head of the Smithsonian Institution, and of the United States National Museum, and with the United States Fish Commission, of which he was also President. In 1884 the *Auk* announced that the bird-registers of the United States National Museum had reached 100,000 specimens in number, this splendid collection having been based on the nucleus of 3696 skins, the private collection of Prof. Baird; and the same journal states:—"As being, more than any other living person, entitled to the privilege, specimens numbered 100,000 and 100,001 are entered as donations from Prof. Baird, to whom they were presented by Mr. Geo. N. Lawrence, the oldest active American ornithologist. One of these, a common Crossbill, was shot by Mr. Lawrence, in New York City in 1850, and the other, a Flicker, on Long Island, in 1862."

We may add that, during an experience of twenty years, we have never heard from any ornithologist, European or American, a single unkind word concerning Prof. Baird, either in his public or private capacity. This is something to say in this age of jealousies and back-bitings. R. BOWDLER SHARPE.

NOTES.

LAST year the New South Wales Government, through their Agent-General, invited the British Association to meet at Sydney in January. The invitation has now been withdrawn. Strangely enough, the matter was treated as a party question in the New South Wales Parliament.

THE American Association for the Advancement of Science met in New York from August 10 to 17. Prof. S. P. Langley, the President, in his opening remarks, congratulated the members on the fact that the meeting promised to be most successful. Prof. E. W. Morse, of Salem, Mass., the retiring President, chose as the subject of his address, "What American Zoologists have done for Evolution." "Eleven years ago," said Prof. Morse, "I had the honour of reading before this Association an address in which an attempt was made to show what American zoologists had done for evolution. My reasons for selecting this subject were, first, that no general review of this nature had been made; and, second, that many of the oft-repeated examples in support of the derivative theory were from European sources, and did not carry the weight of equally important facts the records of which were concealed in our own scientific journals. Darwin was pleased to write to me that most of the facts I had mentioned were familiar to him, but, to use his own words, he was amazed at their number and importance when brought together in this manner. The encouragement of his recognition has led me to select a continuation of this theme as a subject for the customary presidential address—a task which is at best a thankless if not a profitless one. Had I faintly realized, however, the increasing number and importance of the contributions made by our students on this subject, I should certainly have chosen a different theme." Prof. Morse laid much stress upon the fact that "American biological science stands as a unit for evolution."

IN Europe the weather rendered almost useless the elaborate preparations which had been made for observations of the total solar eclipse of August 19. From the German stations the Berlin Observatory received a series of dismal telegrams, such as, "Fog and rain; no observations," "Nothing done; quite cloudy," "Cloudy; observed nothing." Partially successful observations were made in Germany only at Nordhausen and Eisleben. In European Russia observers were almost equally unfortunate. At Klin all attempts to get a glimpse of the eclipse were "completely frustrated by the dull gray sky and thick Scotch mist which quickly damped both one's clothes and one's spirits." At the last moment Prof. Mendeleieff, who was stationed at Klin to observe the form of the corona, its spectrum, and the course of the shadow, went up alone in a balloon, but he was too late to obtain important results. A balloon which went up at Tver was met in its ascent by torrents of rain. A glimpse of the sun was obtained at Tver only twice—at the contact, and when it was about seven-eighths obscured. At Spirovs, nearer St. Petersburg, totality is said to have been visible for twenty seconds. At Petroffsk, in the Government of Jaroslav, Prof. Glasenapp, of St. Petersburg, was lucky enough to be able to make six drawings and to get two photographs, while Prof. Stanoievitsch, of Belgrade, was successful in observing and photographing the spectrum of the corona. Fortunately there was a clear sky at Tomsk and other stations in Siberia.

IT is worth noting that an extraordinary amount of interest was excited on the Continent by the eclipse. It is calculated that in Berlin and the neighbourhood no fewer than 200,000 persons were waiting in the hope of seeing it, and in Russia great numbers of people flocked to many points of observation. This may, we hope, be taken as an indication that both in Russia and Germany there is a growing popular appreciation of some of the more striking truths of physical science.

THE Berlin Correspondent of the *Times* has brought together some interesting reports as to the effect of the eclipse upon the lower animals. Foresters state that the birds, which had already begun to sing before the eclipse took place, became of a sudden quite silent, and showed signs of disquiet when darkness set in. Herds of deer ran about in alarm, as did the small four-footed game. In Berlin a scientific man arranged for observations to be made by bird-dealers of the conduct of their feathered stock, and the results are found to deviate considerably. In some cases the birds showed sudden sleepiness, even though they had sung before the eclipse took place. In other cases great uneasiness and fright were observed. It is noticeable that parrots showed far more susceptibility than canaries, becoming totally silent during the eclipse, and only returning very slowly to their usual state.

IT is greatly to be regretted that the Government has found it necessary to abandon the Technical Education Bill. In announcing to the House of Commons the surrender of the measure, Mr. W. H. Smith said:—"We hoped that the Bill would have been received almost unanimously by the House, but it has met with opposition, and we are threatened with prolonged discussion of the measure, and on August 18 I cannot encounter the difficulties which are likely to be thrown in the way if we persist in the carrying through of that Bill in the course of the present session. It is, however, a measure which we should feel it our duty to introduce in the very earliest days of the next session, and I hope that the consideration which will be given to the subject in the interval will enable us to meet any objections raised by hon. friends on this side of the House, and by hon. gentlemen on the other side, so as to produce a measure which will rapidly obtain the concurrence of the House without exciting any party feeling of any kind whatever, for I should greatly deprecate any party or sectional feeling in a question of this kind."

IN the discussion on Tuesday evening of the vote to complete the sum of £147,385 for the British Museum, Sir J. Lubbock expressed much regret that the amount allotted to purchases for the Museum was £10,000 less than usual. It would be hard to conceive a more striking instance of misplaced economy, for, as Sir J. Lubbock pointed out, there is at the present moment an exceptional number of interesting specimens for sale. Mr. Molloy proposed that the Museum should be opened at night, and maintained that the sum required for the electric light would not exceed £1000 per annum. Mr. W. H. Smith, on behalf of the Government, promised that this question should be most carefully examined during the recess.

ON Tuesday evening, in connexion with the vote to complete the sum of £23,900 for learned Societies, &c., several Scottish members complained that science in Scotland receives anything but generous treatment. Sir John Lubbock was able to show that in some of their statements they did not take all the facts into account; but the demands made on behalf of the Ben Nevis Observatory were certainly not unreasonable. If a grant cannot be made to this institution, the Government might at least give up its claim to the sum of £130 paid annually to the Post Office for the use of the telegraph.

MESSRS. MACMILLAN will publish shortly a work on the nervous system and the mind, by Dr. Charles Mercier. It is intended to serve as an introduction to the scientific study of insanity. It will contain an exposition of the new neurology as founded by Herbert Spencer and developed by Hughlings Jackson; an account of the constitution of mind from the evolutionary standpoint, showing the ways in which it is liable to be disordered; and a statement of the connexion between nervous function and mental processes as thus regarded.

DR. ALFRED R. WALLACE arrived at Liverpool on Saturday last by the steamship *Vancouver* from Quebec, after his ten months' lecture tour in the United States and Canada. He saw a good deal of the country, and spent two months in California and the Rocky Mountains. During his stay at Boston and Washington he made the acquaintance of most of the American men of science.

A LETTER on Antarctic exploration has been addressed by Capt. C. Pasco, R.N., to Admiral Sir E. Ommanney, Hon. Secretary to the Antarctic Committee of the Geographical Section of the British Association. Capt. Pasco writes on behalf of the Antarctic Committee appointed by the Royal Society of Victoria and the Victorian branch of the Royal Geographical Society of Australasia. Much of the information contained in the letter has already appeared in NATURE (June 30, p. 211, and July 21, p. 277). Having dealt with the question of ways and means, Capt. Pasco says the Victorian Societies feel warranted in recommending the renewal of Antarctic research for the following reasons: (1) that the configuration of the Antarctic continent may be traced further, with the view to extend our acquaintance with the geography of the globe; (2) that a further insight into the geology of these lands may be obtained; (3) that it is desirable to increase the extent of the determined physiography of the world by ascertaining whether the recent volcanic disturbances in New Zealand and in the Sunda Islands—both situated on the line of weak earth crust which is believed to carry the volcanoes of Victoria Land—have produced changes of any kind in the Antarctic Circle; (4) the examination of Mount Erebus would appear to be practicable, as Ross reports that the coast became lower as it trended towards its foot, and the results of a visit to the locality should be of the highest interest; (5) that the question whether any secular climatic change is in progress may be investigated, and that the sea temperatures may be ascertained by means of the most modern appliances; (6) that the magnetic survey of these parts may be resumed, and that new data may be obtained for comparison with the elements recorded by Ross; (7) that the existence of whales or seals, or the occurrence of any commercial products, may be accurately observed. In concluding his letter, Capt. Pasco expresses a hope that the efforts of the British Association in favour of the renewal of Antarctic exploration may speedily receive the reward which they deserve. "We sincerely trust," he says, "the exploratory work in the Antarctic, commenced so well by the illustrious Cook, and continued by other brave seamen, but stayed ever since the return of that successful explorer and navigator Ross, may be resumed at once with energy, intelligence, and ample appliances."

THE *Jahrbuch* of the Norwegian Meteorological Institute for 1885 (the last published) shows the part which is taken by Norway in the general system of meteorological organizations. The work contains complete observations for twelve stations, and summaries for sixty-eight others, among which are seven lighthouse stations; for the latter the observations of sea-temperature are also published. Systematic observations were begun in Christiania as early as 1837 by the professors of the University, and were regularly published until the end of 1867. In the meantime (1860) the Director of the Christiania Observatory had commenced the publication of the observations taken at five telegraphic stations on the coast, in addition to those of the Observatory, and this series was continued until the end of 1886, at which time the Meteorological Institute was established under the present Director, Prof. H. Mohn, so that the present year-book forms really the nineteenth of the series. With the year 1874 the work took its present shape, in order to bring it more into conformity with the decisions of the Meteorological Congress at Vienna (1873); yet we observe that the wind-force is estimated according to the old land-scale 0–6, which is now seldom used, while in this country the scale is 0–12, and in other countries more usually 0–10. This diversity of scales leads to great confusion when dealing with wind-observations generally. The Institute publishes somewhat less than several other countries, but its staff is doing good work in connexion with the Reports of the Norwegian North Atlantic Expedition of 1876–78, and the Polar Expedition of 1882–83.

THE Imperial Academy of Sciences of St. Petersburg has issued a new edition of its "In tructions for use at Meteorological Stations" (St. Petersburg, 1887, 106 pp. 8vo, and 34 woodcuts) by Dr. H. Wild, Director of the Central Physical Observatory. The first edition appeared in 1869, since which time the number of meteorological stations of the second order in Russia has increased from 33 to 255. The work is written in the German language and is divided into three parts: (1) directions for the erection of the instruments and for taking the observations, (2) a description of the instruments used at the stations of the Russian system, and (3) a description of the instruments required for observations not always taken at ordinary stations. But the work is not accompanied by the tables generally annexed to such instructions. Siphon barometers are mostly employed in Russia, but latterly portable cistern barometers have been adopted for use at distant stations. Several of the instruments, all of which are clearly illustrated, are not in use in this country. Among them may be specially mentioned:—(1) A portable equatorial sun-dial on Fléchet's principle for places where no good time-piece is available, showing the mean time within five minutes. (2) A swinging-plate wind-gauge, which was generally in use in Switzerland while Dr. Wild was at Berne, and now is adopted in Russia. This instrument is described in the Report of the Meteorological Congress held at Vienna in 1873; it consists of a rectangular metal plate suspended like a sign-board, and shows the force of the wind by its displacement from the vertical position. (3) A rain-gauge on Prof. Nipher's principle, with arrangements for protection against the influence of high winds on the rainfall. (4) A nephoscope invented by Dr. Fineman, of Upsala, a model of which was exhibited at the meeting of the International Meteorological Committee at Paris in 1885. In the *Meteorologische Zeitschrift* for August, Dr. Köppen points out a discrepancy in the description of the somewhat rare phenomenon of glazed frost; but, as a whole, the treatise will rank as one of the best extant.

THE Report of the Meteorological Reporter for the Punjab for the financial year 1886–87 states that the most important feature in the meteorology during the year has been the failure of the cold weather rains (December to March). In January there was a certain amount of excess in the Delhi division, but in all other parts of the province there was a deficiency, and the following months were practically rainless. The monsoon rains (June to September) were generally good, notwithstanding their early cessation. The Report of the Sanitary Administration for the calendar year 1886, which has arrived simultaneously with that above mentioned, shows that the greatest annual rainfall was 53·3 inches at Abbottabad (Peshawar), and the least 4·3 inches at Muzaffargarh (Deraját). We omit the exceptional amount of 127·5 inches at Dharmsála (Jullundur). The inclusion of this excessive amount vitiates the general mean for the stations for the year, viz. 29·1 inches, which is more than three inches higher than it would be if this exceptional amount were omitted from it. The Meteorological Reporter gives 183° as the highest temperature in the sun's rays, being at Lahore on April 28, and it ranged from 172° to 175° in the five succeeding months. The maximum reading in the shade was 118° at two stations on the 13th, and the lowest maximum was 79° at Sirsa (Delhi), in January. The absolute minimum in the shade was 29° at Ráwalpindi, in February, giving a range of 89° in the shade temperature for the whole province. During the early part of this month of February a remarkable wave of low temperature passed over the Punjab; on the 9th and 10th remarkably low temperatures were recorded.

THE results of a long series of experiments upon the nature of the chemical action between acids and the metal zinc have just been published by MM. Spring and van Aubel in the August number of the *Annales de Chimie et Physique*. Although one of the first chemical reactions which come under the notice of students, it has hitherto been among the least understood. The method of experiment was to plunge a quantity of zinc, which contained a small quantity of lead, and whose surface was known, into such a volume of the acid of known strength as would suffice for the elimination of a volume of hydrogen $= Q$. The hydrogen was collected in a vessel divided into aliquot parts, q, of Q; with the aid of a chronograph the times t_1, t_2, &c., necessary for the production of successive volumes $= q$ were noted, and from the data obtained the rapidity of the reaction $\frac{q}{t_1}$, $\frac{q}{t_2}$, &c., at corresponding epochs was estimated, thus following the reaction step by step from beginning to end. The results were eventually represented graphically, the abscissæ representing successive quantities q, and the ordinates the rapidity. The action of the acid upon the zinc is not most rapid at the origin, but increases to a maximum when the acid is about half its original strength, afterwards diminishing proportionally to the concentration to the end of the reaction, so that the curve after passing the maximum becomes a straight line. The early stage previous to attaining the maximum is called the *mise en train* of the reaction, or period of induction, and it is conclusively shown that during this period the acid, by a slow action, prepares at the surface of the metal an infinity of little electric couples by exposing the minute grains of lead contained in the zinc. Hence, De la Rive's idea that the solution of a metal by an acid is mainly due to electrolytic action is substantially correct. It will be remembered that De la Rive showed that pure zinc is only attacked by acids with extreme slowness, but that if traces of copper, iron, or lead be present the evolution of hydrogen is much more rapid. One most interesting result emerges from the experiments of Spring and van Aubel: the curves from all the experiments with hydrochloric acid intersect about $-70°$ C., showing that at this temperature no action at all occurs between this acid, however concentrated, and zinc, and it is a known fact that zinc does not dissolve in liquefied hydrochloric acid gas, whose temperature of liquefaction is near $-70°$. Sulphuric acid acts upon zinc twenty-seven times more feebly than hydrochloric, and the electrolytic action appears to be the vastly preponderating one, the sulphate of zinc resulting from the action of H_2SO_4 upon the ZnO first formed by electrolysis; hence the true nature of this every-day reaction is probably as follows: $Zn + H_2SO_4 = ZnO + H_2 + SO_3$; $SO_3 + H_2O + Aq = H_2SO_4 + Aq$; $H_2SO_4 + ZnO = ZnSO_4 + H_2O$.

RUSSIAN geological literature has been enriched during the current year by a most valuable publication, which will be especially welcome to West European geologists. We mean the "Bibliothèque géologique de la Russie," published in Russian and in French by the Geological Committee, under the editorship of M. Nikitin. The first volume contains an index, as nearly complete as possible, of books, pamphlets, and articles published in Russia on geology, mineralogy, and palæontology during the year 1885. During the last few years geology has made rapid progress in Russia, and although all the chief works produced recently have been published either in the *Izvestia* or *Trudy* of the Geological Committee, or in the *Zapiski* of the Mineralogical Society, a great number of important papers are scattered in the Memoirs of the Academy and other scientific bodies, and thus often escape the attention even of Russian geologists. To West European geologists most of these papers have hitherto remained quite unknown. The "Bibliothèque géologique" mentions all of them, and gives short analyses, which sum up in Russian and French all the chief facts mentioned, and the conclusions arrived at, in the works and papers enumerated. The analyses are generally admirable, and most of them are due to the pen of Miss Mary Tswetaev.

M. MAINOFF's work on the "Juridical Customs of the Mordovians," published in the fourteenth volume of the Memoirs of the Russian Geographical Society for Ethnography, is a capital inquiry into the customs of this important branch of the Volga Finns, as they have shaped themselves under the double influence of the Russians and the Tatars. It is an important addition to the work by the same author which contained his anthropological measurements. M. Mainoff has devoted attention chiefly to marriage customs, but his work, which is the result of many years' acquaintance with the Mordovians, contains plenty of useful information on other subjects. It is worthy of notice that until quite recent times the Mordovians knew no such word as "relations" (French, *parents*), and that they used only the word *tev*, or *tevx*, which corresponds to the *gens* as explained by Mr. Lewis Morgan. Only those were considered as kinsfolk who had a common descent, and lived under a common roof. The compound family continues to exist among the Mordovians, but on a very limited scale. The remarks of M. Mainoff on the kidnapping of brides are very interesting. This form of marriage still survives among the Mordovians, but it takes place with the consent of the bride, and very often with the knowledge of her parents.

MR. HOWARD GRUBB, telescope-maker, was one of a group of gentlemen on whom the Lord Lieutenant of Ireland conferred the honour of knighthood on Monday last.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Mr. Thos. D. Wickenden; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. Charles Crocker; four Black-eared Marmosets (*Hapale penicillata*) from South-East Brazil, presented respectively, two each, by Mr. George Best and Mr. J. Crick; a Purple-faced Monkey (*Semnopithecus leucoprymnus*) from Ceylon, presented by Mr. H. Hart; a Ruffed Lemur (*Lemur varius* ♀) from Madagascar, presented by Mrs. M. Kestell-Cornish; a Moustache Monkey (*Cercopithecus cephus*), two Lesser White-nosed Monkeys (*Cercopithecus petaurista*), two White-crowned Mangabeys (*Cercocebus æthiops*), an African Civet Cat (*Viverra civetta*), a Blotched Genet (*Genetta tigrina*), a Two-spotted Paradoxure (*Nandinia binotata*), a White-crested Tiger-Bittern (*Tigrisoma* *leucolophum*), a Madagascar Porphyrio (*Porphyrio madagascariensis*), five Tambourine Pigeons (*Tympanistria bicolor*), three Schlegel's Doves (*Chalcopelia puella*) from West Africa, presented by Mr. J. B. Elliott; a Common Cormorant (*Phalacrocorax carbo*), European, presented by Mr. T. M. Oldham; a Great Eagle Owl (*Bubo maximus*), European, a Virginian Eagle Owl (*Bubo virginianus*) from North America, presented by Mr. Charles Clifton; a Hygian Snake (*Elaps hygiæ*) from Port Elizabeth, presented by Mr. W. K. Sibley; a Tarantula Spider (*Mygale*), from Bahama, presented by Mrs. Blake; a Sand Lizard (*Lacerta agilis*) from Jersey, presented by Mr. F. T. Mason; a Prince Albert's Curassow (*Crax alberti*) from Columbia, a Slender-billed Cockatoo (*Licmetis tenuirostris*) from South Australia, deposited; three Oyster-catchers (*Hæmatopus ostralegus*), European, purchased; a Blood-breasted Pigeon (*Phlogœnas cruentata*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

MAGNITUDES OF "NAUTICAL ALMANAC" STARS.—In order to expedite the publication of short articles upon astronomical and meteorological subjects, prepared at the Harvard College Observatory, Prof. Pickering has decided to print each as completed as successive numbers of a series, which, when a sufficient amount of material has been collected, will constitute the eighteenth volume of the Annals of the Observatory. Each number is to be published and distributed soon after it is prepared.

The first of this series of papers is a collection the stars employed in the standard lists of the *Nautical Almanacs* published by the Governments of Great Britain, the United States, France, Germany, and Spain, together with their magnitudes, as derived from four standard authorities: the *Harvard Photometry*, the *Uranometria Argentina*, Wolff's photometric observations, and the *Uranometria Oxoniensis*, the second and third being reduced to the photometric scale employed in the other two catalogues, the Harvard and Oxford scales agreeing closely. At present the magnitudes assigned to these stars in the respective Almanacs do not agree, nor do they represent the most accurate results available. Prof. Pickering therefore offered to the Superintendents of the Almanac Offices to supply a discussion of the best values of the magnitudes at present attainable; and favourable replies having been obtained in the cases of the French, Spanish, and American Almanacs, it is expected that the improved values here given will be used in those works in future.

The list embraces 800 stars, and of these the magnitudes of all but 64 depend at least upon two, and generally upon three, authorities; 132 stars being common to all four of the adopted standard catalogues of brightness. The average values of the residuals from the adopted means for these 132 stars are respectively: Harvard, 0·062; Argentine, 0·093; Wolff, 0·094; Oxford, 0·106. The average probable error of the adopted magnitudes is 0·09, assuming the absence of systematic error. The total number of residuals is 2188, of which only 67 exceed two-tenths of a magnitude, and only 17 three-tenths. There are only two cases of a residual exceeding four-tenths, both in the Oxford *Uranometria*; the one being the low star θ Ophiuchi, the other the double star θ Serpentis.

COMET 1887 *e* (BARNARD, MAY 12). — The following ephemeris for Berlin midnight is given by Dr. H. Kreutz (*Astr. Nachr.*, No. 2799). The comet is very favourably placed for observation, but is extremely faint.

1887	R.A. h. m. s.	Decl. ° ′	log *r*	log Δ
Aug. 24	18 41 48	7 40·8 N.	0·2320	9·9641
„ 28	18 49 6	7 16·5	0·2402	9·9868
Sept. 1	18 56 22	6 51·3	0·2484	0·0093
„ 5	19 3 34	6 25·7 N.	0·2567	0·0315

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 AUGUST 28—SEPTEMBER 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 August 28

Sun rises, 5h. 7m.; souths, 12h. 1m. 8·9s.; sets, 18h. 55m.; decl. on meridian, 9° 45′ N.: Sidereal Time at Sunset, 17h. 22m.

Moon (Full on September 2, 11h.) rises, 16h. 6m.; souths, 20h. 25m.; sets, 0h. 45m.*; decl. on meridian, 19° 44′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury ...	3 50 ...	11 15 ...	18 40 ...	15 26 N.
Venus ...	8 25 ...	13 47 ...	19 9 ...	8 6 S.
Mars ...	1 48 ...	9 48 ...	17 48 ...	20 57 N.
Jupiter ...	10 28 ...	15 34 ...	20 40 ...	11 10 S.
Saturn ...	1 54 ...	9 48 ...	17 42 ...	20 6 N.

* Indicates that the setting is that of the following morning.

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

August.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
28 ...	ξ′ Sagittarii ...	6 ...	18 54 ...	19 56 ...	36 304
Sept.					
1 ...	45 Capricorni ...	6 ...	0 15 ...	1 29 ...	108 330
1 ...	44 Capricorni ...	6 ...	0 33	near approach	216 —
2 ...	χ Aquarii ...	5½ ...	23 7	near approach	190 —

August.	h.	
28 ...	17 ...	Mars in conjunction with and 0° 49′ north of Saturn.
29 ...	11 ...	Venus stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Monocerotis ...	7 25·6 ...	9 33 S. ...	Aug. 31,		*m*
W Virginis ...	13 20·2 ...	2 48 S. ...	„ 31,	23 0	*M*
δ Libræ ...	14 54·9 ...	8 4 S. ...	„ 29,	4 57	*m*
			Sept. 2,	20 40	*m*
U Coronæ ...	15 13·6 ...	32 4 N. ...	Aug. 29,	20 18	*m*
S Libræ ...	15 14·9 ...	19 59 S. ...	Sept. 2,		*M*
U Ophiuchi ...	17 10·8 ...	1 20 N. ...	Aug. 31,	4 46	*m*
			and at intervals of	20 8	
X Sagittarii ...	17 40·5 ...	27 47 S. ...	Aug. 31,	22 0	*m*
W Sagittarii ...	17 57·8 ...	29 35 S. ...	„ 28,	0 0	*M*
R Scuti ...	18 41·5 ...	5 50 S. ...	„ 28,		*m*
R Lyræ ...	18 51·9 ...	43 48 N. ...	„ 31,		*M*
S Vulpeculæ ...	19 43·8 ...	27 0 S. ...	Sept. 2,		*m*
χ Cygni ...	19 46·2 ...	32 38 N. ...	Aug. 29,		*m*
S Sagittæ ...	19 50·9 ...	16 20 N. ...	„ 31,	0 0	*m*

M signifies maximum; *m* minimum.

THE FACTORS OF ORGANIC EVOLUTION.

WHILE reviewing, a short time ago, Mr. Herbert Spencer's essay on the above subject (NATURE, vol. xxxv. p. 262), I promised to consider the present standing of the question as to whether, or how far, use and disuse admit of being regarded as true causes of change of organic type. Of course there is no question about the effects of use and disuse as regards the individual: the only question is as to whether, or how far, these effects admit of being inherited, so that modifications of structure which are produced by modifications of function in the individual become causes of corresponding, and therefore of adaptive, changes of structure in species. The importance of this question is second to none in the whole range of biology. For not only is it of the highest importance within the range of biology itself—governing, by whatever answer we give it, our estimate of the importance of natural selection, and thus requiring to be dealt with on the very threshold of biological philosophy—but its influence extends to almost every department of thought. For, as Mr. Spencer remarks in his preface, upon the answer which this question may finally receive will depend in chief part the sciences of psychology, ethics, and sociology. If functionally-produced modifications are inheritable, the phenomena of instinct, innate ideas, moral intuitions, and so forth, admit of a scientific explanation at the present moment; otherwise they do not, or, at least, not in so distinct nor in so complete a manner. Therefore, we can hardly feel that Mr. Spencer exaggerates the importance of this question when he says of it, "Considering the width and depth of the effects which our acceptance of one or other of these hypotheses [namely, that functionally-produced modifications are inherited, or that they are not] must have upon our views of Life, Mind, Morals, and Politics, the question—Which of them is true? demands, beyond all other questions whatever, the attention of scientific men."

That functionally-produced modifications are inherited was the great assumption upon which Lamarck founded his theory of evolution. Erasmus Darwin adopted the assumption, and it was also accepted by Charles Darwin as representing a highly important factor of organic evolution, although subsidiary to that of natural selection. Lastly, Mr. Spencer has always upheld the assumption, and, as we shall subsequently see, has done more than anybody else in the way of its justification. On the other hand, of late years a growing tendency has been displayed by those evolutionists who out-Darwin Darwin, not only to assign to natural selection a monarchical government over the whole realm of organic Nature, but also, and consequently, to deprive use and disuse of those lesser sovereignties which were so freely accorded to them by the "Origin of Species." This tendency has now reached a climax in the publication of an essay, by no less an authority than Prof. Weismann, wherein the Lamarckian principles of use and disuse are denied *in toto*.[1] We may therefore best begin our stock-taking of the whole subject by considering what Prof. Weismann has said; for assuredly the doctrine of use and disuse as themselves useless could nowhere meet with an abler champion.

In the first place, he is committed to this doctrine as a necessary consequence of his own theory of heredity, according to which *any* change *acquired* by the individual cannot be transmitted to progeny. This theory regards the individual organism as nothing more than what may be termed a temporary receptacle of "germ-plasma"—this germ-plasma being handed on from generation to generation, without ever being affected by any changes that may take place in the organisms which contain it. And the only reason why such *appears* to be the case—or why in the course of generations one specific type gradually changes through inherited modifications into another—is because the germ-plasma itself is liable to variation, and when the variations happen to be of a kind which lead to favourable modifications of the store-houses (organisms), these store-houses are preserved by natural selection, and with them the peculiar variations of the germ-plasma, which are thus carried on to the next generation. Hence natural selection is really at work upon variations of the germ-plasma, and hence also no change occurring in an organism during its own life-history can at all affect its progeny—any more, for instance, than the chipping or the twisting of a vessel can modify the chemical constitution of whatever substance the vessel may contain. In short, it is only so-called congenital variations—or variations of germ-plasma—that can be inherited; and, therefore, it is only upon such variations that survival of the fittest is able to act. All variations afterwards superinduced in the organism—whether by way of mutilation, disease, acquisition of faculty, or degeneration of structure—are destined to be immediately extinguished by the death of the organism. Now, from this general theory it necessarily follows that the effects of use and disuse in the individual cannot be transmitted to progeny; for, if they could, the fact would be fatal to the theory. Hence it is, as above observed, that Prof. Weismann is committed by his theory of heredity to a denial of the Lamarckian assumption, which, as we have seen, was accepted by Darwin.

But besides this merely *a priori* ground of deduction from his own theory, Prof. Weismann stands upon the affirmation that there is, as a matter of fact, no real evidence of the effects of use and disuse being inherited. For, he maintains, all the supposed evidence on this head admits of being fully interpreted by quite another principle. When an organ (or any structure) falls into disuse, in the course of generations it atrophies, becomes rudimentary, and finally disappears. This fact is generally taken as proof of the inherited effects of disuse—seeing that it is so strikingly analogous to these effects in the case of individual organisms. But there is an alternative possibility. The *raison d'être* of the organ before it fell into disuse, was its utility: it was originally built up under the nursing influence of natural selection solely on account of its serviceability. When therefore from changed conditions of life, or for any other reason, the organ ceased to be serviceable, the premium which had been previously set upon it by natural selection was withdrawn; individuals which happened to present the organ of a size below the average were no longer eliminated in the struggle for existence, but were allowed to propagate. Thus, by free intercrossing, the average size became less and less in every succeeding generation, until eventually, according to Weismann, it must altogether disappear. In short, as the organ was originally built up by natural selection, when natural selection was withdrawn, is any other explanation required of the fact that the organ progressively dwindled?

Unknown to Prof. Weismann, this principle, under the name "Cessation of Selection," was enunciated by the present writer in a series of articles published in these pages so long ago as 1873–74. Attention is now drawn to this fact merely for the sake of informing biologists that the principle met with the full approval of the late Mr. Darwin, and also to state exactly the shape in which it was thus approved by him. For in one or two particulars the idea as published in NATURE differs from that which has been recently and independently arrived at by Prof. Weismann. As the issues of NATURE in question are out of print, and as the matter cannot be more briefly stated now than it was stated then, I may best begin by reprinting the portion of these articles which sets forth the principle of the cessation of selection, as this was accepted by Mr. Darwin.

"In a former communication (NATURE, vol. ix. p. 361) I promised to advance what seemed to me a probable cause—additional to those already known—of the reduction of useless structures. As before stated, it was suggested to me by the penetrating theory proposed by Mr. Darwin (NATURE, vol. viii. pp. 432 and 505), to which, indeed, it is but a supplement.[1] Epitomising Mr. Darwin's conception as the dwarfing influence of impoverished conditions progressively reducing the average size of a useless structure by means of free intercrossing, the present cause may be defined as the mere cessation of the selective influence from changed condition of life.

"Suppose a structure to have been raised by natural selection from 0 to average size 100, and then to have become wholly useless. The selective influence would now not only be withdrawn, but reversed; for, through Economy of Growth—understanding by this term both the direct and the indirect influence of natural selection—it would rigidly eliminate the variations 101, 102, 103, &c., and favour the variations 99, 98, 97, &c. For the sake of definition we shall neglect the influence of economy acting below 100, and so isolate the effects due to the mere withdrawal of selection. By the conditions of our assumption, all variations above 100 are eliminated, while below 100 indiscriminate variation is permitted. Thus, the selective premium upon variation 99 being no greater than that upon 98, 98 would have as good a chance of leaving offspring which would inherit and transmit this variation as would 99; similarly, 97 would have as good a chance as 98, and so on. Now there is a much greater chance of variations being perpetuated at or below 99, than at or above 100, for at 100 the hard line of selection (or economy) is fixed, while there is no corresponding line below 100. The consequence of free intercrossing would therefore be to reduce the average from 100 to 99. Simultaneously, however, with this reducing process, other variations would be surviving below 99, in greater numbers than above 99; consequently the average would next become reduced to 98. There would thus be 'two operations going on side by side—the one ever destroying the symmetry of distribution' round the average, 'and the other ever tending to restore it.' It is evident, however, that the more the average is reduced by this process of indiscriminate variation the less chance there remains for its further reduction. When, for instance, it falls to 90, there are numerically (though not actually, because of inheritance) 89 to 9 in favour of diminution; but when it falls to 80 there are only 79 to 19 in such favour. Thus, theoretically, the average would continue to diminish at a slower and slower rate, until it comes to 50, where, the chances in favour of increase and of diminution being equal, it would remain stationary.

"Having thus, for the sake of clearness, considered this principle apart, let us now observe the effect of superadding to it the influence of the economy of growth—a principle with which its action must always be associated. Briefly, as this

[1] "Ueber den Rückschritt in der Natur" (Freiburg, 1886).

[1] As stated in the text, the leading idea in Mr. Darwin's suggestion was that impoverished conditions of life would accentuate the principle of Economy of Nutrition, and so assist in the reduction of useless structures by free intercrossing. Now, in this idea that of the cessation of selection was really implied; but neither in his own article nor in a subsequent letter by Mr. George Darwin on the same subject (NATURE, October 16, 1873), was it exhibited as an independent principle. It was inarticulately wrapped up with the much less significant principle of impoverished conditions. Afterwards, however, Mr. Darwin expressed himself as fully persuaded of the independent character of the more important principle, which he was really the first to perceive, although not clearly to express. Moreover, he then thought it was probably a principle of universal application, not only as regards rudimentary organs, but also as regards degenerated structures in general.

influence would be that of continually favouring the variations on the side of diminution, the effect of its presence would be that of continuously preventing the average from becoming fixed at 50, 40, 30, &c. In other words, the 'hard line of selection' which was originally placed at 100, would now become progressively lowered through 90, 80, 70, &c.; always allowing indiscriminate variation below the barrier, but never above it.[1]

"It will be understood that by 'cessation of selection from changed conditions of life,' I mean a change of any kind which renders the affected organ superfluous. Take, for example, the exact converse of Mr. George Darwin's illustration, by supposing a herd of cattle to migrate from a small tract of poor pasture to a large tract of rich. Segregation would ensue, the law of battle would become less severe, while variation would be promoted in a cumulative manner by the increase of food. The young males with shorter horns would thus have as good a chance of leaving progeny as 'their longer-horned brothers,' and the average length would gradually diminish as in the other case. Of course, as the predisposing cause of impoverished nutrition would now be absent, the reducing process would take place at a slower rate. Moreover, it is to be remarked that this principle differs in an important particular from that enunciated by Mr. Darwin, in that it could never reduce an organ much below the point at which the economy of growth, together with disuse, ceases to act. For, returning to our numerical illustration, suppose this point to be 6, the average would eventually become fixed at 3.

"That the principle thus explained has a real existence we may safely conclude, theoretical considerations apart, from the analogy afforded by our domestic races; for nothing is more certain to breeders than the fact that neglect causes degeneration, even though the strain be kept isolated."

Evidence of the wide-reaching operation of this principle under Nature must be sought for in cases where it is impossible that disuse can have had any part in the reducing process—seeing that we cannot all agree with Prof. Weismann in dismissing the agency of disuse on *a priori* grounds of deduction from his theory of germ-plasma. Now, although it is not at all an easy thing to find cases where the influence of the cessation of selection admits of being demonstrably dissociated from the possible influence of disuse, the following appear to meet the requirements of the proof:—

(1) The whole multitude of instances where recapitulative phases are absent from the developmental history of an embryo may stand for so many proofs of reduction without the agency of disuse. For, inasmuch as none of the structures represented in those phases elsewhere can ever have been of any *use* to the embryo from which they have disappeared, it is sufficiently evident that their obliteration can never have been due to *disuse*. And, forasmuch as such structures persist in the embryos of *allied* species, it appears equally evident that their reduction cannot be ascribed to natural selection acting through the economy of nutrition; for, were this the case, natural selection ought to have effected the reduction in the embryos of all the species.

(2) Even in adult organisms we meet with many structures which, although of obvious *use* in the sense of affording protection, yet cannot be said ever to be *used* in the sense of being actively employed, or of being employed in any way that could possibly lead to their structure being modified by their function. Of such, for example, are the hard coverings of animals and of parts of plants. It is impossible that the thickness of shells, for instance, can ever have been increased by their "use" as protective coverings, seeing that the use is here wholly passive—is not of the active kind which determines a greater flow of nutrition to the part. Hence, we can only attribute the formation of such structures to the unaided influence of selection. But, if so, we can only attribute to the cessation of selection their subsequent degeneration in all cases—such as that of male cirripedes, hinder parts of hermit crabs, &c.—where changed conditions of life have rendered these parts no longer needful in the struggle for existence. Here, indeed, economy of growth may have assisted in the reduction; but, whether or not, disuse can scarcely have done so, and this is the point with which we are at present concerned.

(3) In many species of social Hymenoptera the neuter insects have lost their wings. Now, as these neuter insects never have progeny, it is evident that the reduction of their wings cannot possibly have been due to the inherited effects of disuse. We must, therefore, set it down to the cessation of selection, joined, perhaps, with the economy of growth. This is a particularly cogent line of proof, seeing that in some species the head, jaws, and other parts of the neuters have been *enlarged*, in order the better to fit them for heavy work where strength or fighting is required. Had such an enlargement been met with in the case of an animal which leaves progeny, the fact might well have been attributed to the inherited effects of increased *use*. But, as the matter stands, these neuter insects are available as a demonstrative and a double proof of the possibility both of the development and the degeneration of important structures without the aid either of use or of disuse.

(4) In his essay on "Degeneration," Prof. Lankester names three distinct sets of conditions as those under which the process has taken place, and all these are conditions under which the cessation of selection must have taken place. First, "Any new set of conditions occurring to an animal which render its food and safety very easily attained, seem to lead as a rule to degeneration. . . . The habit of parasitism clearly acts upon animal organisation in this way. Let the parasitic life once be secured, and away go legs, jaws, eyes, and ears." In other words, so soon as these organs, which were originally built up by natural selection for the purpose of securing "food and safety," are rendered superfluous by food and safety being otherwise secured, all selective premium on their efficiency is withdrawn, and so they are allowed to degenerate by indiscriminate variation. Second, "Let us suppose a race of animals fitted and accustomed to catch their food, and having a variety of organs to help them in the chase—suppose such animals suddenly to acquire the power of feeding on the carbonic acid dissolved in the water around them just as green plants do. This would tend to degeneration; they would cease to hunt their food, and would bask in the sunlight, taking food in by the whole surface, as plants do by their leaves. . . . These vegetating animals . . . . show how a degeneration of animal forms may be caused by *vegetative nutrition.*" Now, to "cease to hunt their food" is here equivalent to their ceasing to be under the influence of natural selection with respect to their food-hunting organs, just as in the previous case. Third, "Another possible cause of degeneration appears to be the indirect one of minute size. . . . The needs of a very minute creature are limited as compared with those of a large one, and thus we may find heart and blood-vessels, gills and kidneys, besides legs and muscles, lost by the diminutive degenerate descendants of a larger race." But, if "the *needs* of a very minute creature are limited as compared with those of a large one," this is the same as to say that in the "diminutive descendants of a larger race" natural selection will no longer operate for the maintenance of structures which have become needless. In fact, in this passage Prof. Lankester comes very near an express statement of the principle of the cessation of selection.

The sundry instances given in the above paragraphs may, I hope, be held sufficient firmly to establish this principle, and to show that it is one of universal application, wherever an organ or a structure has ceased to be of service to the species presenting it.[1] Now, quite apart from the reference in which we have

[1] It is desirable to remark that this numerical mode of representing the principle is adopted only for the purposes of exposition. The exact point at which equilibrium would be reached *in actual fact* we have no means of ascertaining, since such would depend in any given case upon the original force of inheritance, or the persistence with which heredity would assert itself when left entirely to itself—and of this we have no means of judging. Therefore, I adopt the numerical mode of representing the progressive decline of a structure under the cessation of selection merely to show that at *whatever* point we may suppose equilibrium to be reached—or a state of balance between heredity and indiscriminate variation to be attained—this point must become progressively lowered by the superadded influence of the economy of growth. It may, however, be remarked that the initial stages of reduction would probably take place more rapidly than subsequent stages, seeing that the maximum efficiency of a structure is maintained, not only by heredity, but also by the continued influence of selection. Therefore, when the influence of selection is withdrawn, indiscriminate variation would rapidly degrade the structure through the initial stages of its reduction.

[1] Or, if these instances are not held sufficient for this purpose, I may refer to Prof. Weismann's essay, where further instances are given, and also supplement them with the following passage from my old articles in NATURE:—

"If it be supposed that disuse is the chief cause of atrophy in wild species, then it has not produced so much effect in tame species as we should antecedently expect. . . . For, supposing the cessation of selection to be here the *only* cause at work, what degree of atrophy should we expect to find? Before I turned to the valuable measurements given in the 'Variation of Plants and Animals under Domestication,' I concluded (cf. NATURE, vol. ix. p. 441) that from 20 to 25 per cent. is the maximum of reduction we should expect this unassisted principle to accomplish, in the case of natural as distinguished from artificially-bred organs. Now on calculating the average afforded by each of Mr. Darwin's tables, and then reducing the averages to parts of 100, I find that the highest average decrease is 16 per cent., and the lowest 5; the average of the averages being rather less than 12. Only four hitherto been considering this principle—or with reference to use and disuse—we have here a consideration of great importance in regard to the subject of Prof. Lankester's essay above quoted. Apparently without having either heard or thought of the principle of cessation, Dr. Dohrn was led to attribute an important part in the drama of evolution to the effects of cessation, as these are witnessed in the phenomena of degeneration.[1] About the *facts* of degeneration there can be no doubt, and to this naturalist belongs the credit of having first perceived the wide range of their importance. But, on account of having missed the principle of cessation, both Dr. Dohrn and his English expositor, Prof. Lankester,[2] fell into an omission of *interpretation.* For they both attributed the facts of degeneration to a *reversal* of natural selection; they represented that degeneration could only take place under a change in the conditions of life such that organs or structures previously useful become, not merely useless, but deleterious. Degeneration was thus regarded as always the result of what may be termed active hostility on the part of natural selection; not as the result of a merely passive disregard. Hence the sphere within which the phenomena of degeneration might be expected —or admitted of being satisfactorily explained—was needlessly limited. For instance, Prof. Lankester writes: "It is clearly enough possible for a set of forces such as we sum up in the term 'natural selection' to so act on the structure of an organism as to . . . . . diminish the complexity of its structure." But in order "to diminish the complexity" of any useless structure, it is not necessary that natural selection should "act on the structure": the complexity, like the size, of the structure would necessarily diminish under the mere withdrawal of selection. And hence the phenomena of degeneration do not require, either that the organism presenting them should ever have found its useless organs actively deleterious, or that there should ever have been any "Functions-wechsels" in the case.[3]

The case of degenerated *complexity* proves that the cessation of selection may effect degradation without assistance from the economy of nutrition. I am therefore more disposed to think that the *size* of any useless structure may be reduced to a greater extent by the mere cessation of selection (apart from economy),

individual cases fall below 25 per cent., and of these two should be omitted (cf. 'Variation,' p. 272). Thus, out of eighty-three examples, only two fall below the lowest average expected (*i.e.* on the supposition that disuse has not had anything to do with the reduction). Moreover, we should scarcely expect disuse alone to affect in so similar a degree such widely different tissues as are brain and muscle. The deformity of the sternum in fowls also points to the cessation of selection rather than to disuse. Further, the fact that several of our domestic animals have not varied at all is inexplicable upon the one supposition, while it affords no difficulty to the other. We have seen that disuse can only act by causing variations; and so we can see no reason why, if it acts upon a duck, it should not also act upon a goose. But the cessation of selection depends upon variations being supplied to it; and so, if from any reason a specific type does not vary, this principle cannot act. Why one type should vary, and another not, is a distinct question, the difficulty of which is embodied by the one supposition, and excluded by the other. For, to say that disuse has not acted upon type A, because of its inflexible constitution, while it has acted on a closely allied type B, because of its flexible constitution, is merely to insinuate that disuse, having proved itself inadequate to cause reduction in the one case, may not have been the efficient cause of reduction in the other. But the counter-supposition altogether excludes the idea of a causal connection, and so rests upon the more ultimate fact of differential variability, as not requiring to be explained. Lastly, it is remarkable that those animals which have not suffered reduction in any part of their bodies are likewise the animals which have not varied in any other way, and conversely; for as there can be no causal connection between these two peculiarities, the fact of the intimate association between them tends to show that special reduction depends upon general variability, rather than that special variability depends upon special reducing causes."

[1] "Der Ursprung der Wirbelthiere und der Princip des Functions-wechsels" (Leipzig, 1875).

[2] "Degeneration: a Chapter in Darwinism" (London, 1880).

[3] The same considerations apply to the size of an organism as a whole. If for any reason it ceases to be an advantage to be kept up to the ancestral standard of size, the cessation of selection as regards size would result in a gradual diminution of size, even though the ancestral standard of size were not actually deleterious. Yet, in the last of the passages above quoted from Prof. Lankester—and the passage in his essay where he most nearly approaches the principle of selection as withdrawn—the context shows that he only has in view the principle of selection as reversed. For he says:—"It cannot be doubted that natural selection has frequently acted on a race of animals so as to reduce the size of the individuals. The smallness of size has been favourable to their survival in the struggle for existence." Of course "it cannot be doubted" that this has been so in many cases; but as little can it be doubted that it has not been so in all. In any given case of diminution, it is not necessary to suppose that "the smallness of size has been *favourable* in the struggle for existence": it is enough if the previous largeness of size has *ceased* to be so, or that smallness of size is no longer *deleterious.* Moreover, the same considerations apply to instincts. For example, it can scarcely ever have been a *fatal disadvantage* to the slave-making ants that they should be able to eat their own food; therefore the loss of their original instincts, which now renders them dependent on their slaves for being fed, can only have been brought about by the *cessation* of selection—not by its *reversal.*

than I thought when writing the articles above quoted. Here, however, we must remember that the hold which heredity has upon complexity is much less than that which it has upon size. This is evident, not only from obvious considerations of an *a priori* kind, but also from such cases as those of the blind crabs of Kentucky. Here the disused eyes have been lost, while the foot-stalks which originally supported them have been retained. Now, we can well understand why the eyes should have been the first to disappear under the cessation of selection, seeing that they were structures so highly organised that the continuous influence of selection must have been required to preserve them in a state of efficiency before the animals began to inhabit the dark caves; and, therefore, that when the animals did begin to inhabit these caves, such refined and complex structures would rapidly degenerate through the mere withdrawal of selection. But if we were to attribute any large share in this process of rapid degeneration to the economy of nutrition, we should be unable to explain the persistence of the foot-stalks. Therefore, the cessation of selection, when acting alone, is thus proved capable of reducing a complex structure more quickly than it can reduce a larger but less complex structure, in the same species and under the same conditions.

It is true that in a passage above quoted, and which was published two years before Dr. Dohrn's essay, I myself attributed the phenomena of degeneration to a "reversal of natural selection."[1] But I alluded to such reversal only in so far as it arose from the economy of nutrition (*i.e.* I did not suppose that degeneration can only occur when useless parts become actively deleterious, and therefore require the active agency of selection to remove them); and the effect of reading the subsequently published literature on the subject of degeneration has been to make me attribute more importance to the cessation of selection, and less importance to the economy of nutrition. Nevertheless, I still believe that these principles are inadequate to explain the final and total obliteration of organs which by their combined action they have rendered rudimentary.

And these remarks lead me to indicate the points wherein my hypothesis of the cessation of selection differs from that which has recently been published by Prof. Weismann. Briefly, he does not mention the assistance which this principle derives from that of the economy of nutrition, and he believes that it is in itself sufficient to explain the final and total obliteration of useless parts. Having already given my reasons for holding different views with regard to both these points, it will now suffice merely to re-state the principles which I suggested in the NATURE articles as having been most probably concerned in this final and total obliteration of useless parts. These principles are two in number, and are both quite independent of those which we have hitherto been considering. The first of them is inheritance at earlier periods of life, which progressively pushes back the development of a useless rudiment to a more and more embryonic stage of growth; and the second is the eventual failure of the principle of inheritance itself. For, "whether or not we believe in Pangenesis, we cannot but deem it in the highest degree improbable that the influence of heredity is of unlimited duration."[2] This view of the matter renders it abundantly intelligible why it is that, when once the cessation of selection—co-operating with the economy of nutrition—has with comparative rapidity reduced any useless organ to a rudiment, the latter should then persist for so enormous a length of time that in the result, as Mr. Darwin observes, "rudimentary organs are so extremely common that scarcely one species can be named which is wholly free from a blemish of this nature."

We have seen that in the cessation of selection we must recognise one of the principal causes of atrophy in species; in whatever measure we hold the presence of selection explanatory of evolution, in a corresponding measure must we hold the withdrawal of selection accountable for degeneration. But from this it does not necessarily follow that no other causes either of evolution or of degeneration are to be found. Those naturalists who adopt the light and easy method of out-Darwining Darwin, or close their eyes to every other "factor" save that of natural selection, may indeed rest satisfied with these two complementary principles as in themselves adequate to explain all

[1] Prof. Weismann christens the principle which I have called Cessation of Selection, *Kehrseite der Naturzüchtung;* but, for reasons above given, I do not think that this is so good a name as that which he elsewhere uses incidentally, and which, indeed, is an unconscious translation of my own term —namely, *Nachlass der Naturzüchtung.*

[2] NATURE, *loc. cit.*, where see for a fuller discussion of the causes leading to eventual and total suppression.

the facts both of progress and of regress. But, unless we are satisfied to walk upon the high *priori* road to the exclusion of every other, we must not too readily assume that the presence and the absence of selection have been the only factors at work. In particular, we have now to consider whether use and disuse have co-operated with the presence and the absence of selection in bringing about the existing state of matters in organic Nature as a whole.

Now, the only way in which this inquiry can be conducted is by the method of difference. We must search through organic Nature in order to ascertain whether there are any cases either of evolution or of degeneration where it is manifestly impossible that either the presence or the absence of selection can have had anything to do with the process. If we can find any such cases, we shall not merely save Darwin from his friends by justifying his acceptance of the Lamarckian assumption: we shall prove that presumably in *all* cases where the presence or the absence of selection has been concerned in either building up or breaking down organic structures, these principles have been largely *assisted* in their operations by the inherited effects of use and disuse. For if it can be proved that these effects are inherited in cases where it is impossible that the principle of selection—or its cessation—can have obtained, it would be irrational to deny that they are also inherited in other cases where these principles do obtain.

Seeing that so accomplished a naturalist and so philosophic a thinker as Prof. Weismann has declared that there is no one case to be found such as those of which we are in search, we must be prepared to expect some difficulty in meeting with examples of the uncompounded influence of use and disuse—even supposing use and disuse to be the true causes of specific modification that they were taken to be by Darwin. In order to show the kind of difficulty that here besets inquiry, I will quote a passage from Mr. Spencer's recently-published essay upon the subject.

"When discussing the question more than twenty years ago ('Principles of Biology,' § 166), I instanced the decreased size of the jaws in the civilised races of mankind as a change not accounted for by the natural selection of favourable variations; since no one of the decrements by which, in thousands of years, this reduction has been effected would have given to an individual in which it occurred such advantage as would cause his survival, either through diminished cost of local nutrition or diminished weight to be carried. . . . Reconsideration of the facts does not show me the invalidity of the conclusion drawn, that this decrease in the size of the jaw can have had no other cause than continued inheritance of those diminutions consequent on diminutions of function, implied by the use of selected and well-prepared food. Here, however, my chief purpose is to add an instance showing, even more clearly, the connection between change of function and change of structure. This instance, allied in nature to the other, is presented by those varieties—or, rather, sub-varieties—of dogs, which, having been household pets, and habitually fed on soft food, have not been called upon to use their jaws in tearing and crunching, and have been but rarely allowed to use them in catching prey and in fighting."

There follows an account of a somewhat laborious examination of dogs' skulls in the Museum of Natural History, the result of which was to show that "we have two, if not three, kinds of dog, which, similarly leading protected and pampered lives, show that in the course of generations the parts concerned in clenching the jaws have dwindled;" after which the passage proceeds as follows:—

"To what cause must this decrease be ascribed? Certainly not to artificial selection; for most of the modifications named make no appreciable external signs: the width across the zygomata could alone be perceived. Neither can natural selection have had anything to do with it; for even were there any struggle for existence among such dogs, it cannot be contended that any advantage in the struggle could be gained by an individual in which a decrease took place. Economy of nutrition, too, is excluded. Abundantly fed as such dogs are, the constitutional tendency is to find places where excess of absorbed nutriment may be conveniently deposited, rather than to find places where the cutting down of the supplies is practicable. Nor, again, can there be alleged a possible correlation between these diminutions and that shortening of the jaws which has probably resulted from selection; for in the bull-dog, which has also relatively short jaws, the structures concerned in closing them are unusually large. Thus, there remains as the only conceivable cause, the diminution of size which results from diminished use."

Evidently Mr. Spencer has never heard or thought of the cessation of selection, either as explained thirteen years ago by myself, or as republished within the last few months by Prof. Weismann. For it is evident that, far from his having excluded all conceivable causes of the diminution save that of diminished use, it would be difficult to find a case more favourable to the influence of the cessation of selection. The dogs in question have been "habitually fed on soft food, have not been called on to use their jaws in tearing and crunching, and have been but rarely allowed to use them in catching prey and in fighting." In other words, for at least a hundred generations these dogs have been "leading protected and pampered lives," wholly shielded from the struggle for existence and survival of the fittest. Never having had to use their jaws either in "tearing, crunching, catching prey, or fighting," they, more than any other dogs—even of domesticated breeds—have not been "called on" to use their jaws for any life-serving purpose. Clearly, therefore, if the cessation of selection ever acts at all as a reducing cause in species, here is a case where it is positively bound to act. And, of course, the same remark applies to the analogous case of the diminished size of the jaws in civilised man.

Be it observed, I am not disputing that disuse may in both these cases have co-operated with the cessation of selection in bringing about the observed result. Indeed, I am rather disposed to allow that the large amount of reduction described in the case of the dogs as having taken place in so comparatively short a time, is strongly suggestive of disuse having co-operated with the cessation of selection. But at present I am merely pointing out that Mr. Spencer's investigations have here failed to exhibit the crucial proof of disuse as a reducing cause which he assigns to them: it is not true that in these cases disuse "remains as the only conceivable cause."

Far more successful, however, is his second line of argument. Indeed, to me it has always appeared, since I first encountered it fifteen years ago in the "Principles of Biology," as little short of demonstrative proof of the Lamarckian assumption. Therefore, if, as a result of reading the passage above quoted, one feels disposed to regret that before publishing it Mr. Spencer did not have his attention called to Prof. Weismann's essay on the cessation of selection, still more must one regret that before publishing that essay Prof. Weismann should have failed to remember the "Principles of Biology." For, had he done so, it seems impossible that he could ever have committed himself to the statement that there is no evidence of functionally-produced modifications being inherited, and thus he might have been led to pause before announcing—at least in its present shape—his theory of germ-plasma.

The argument whereby in my opinion Mr. Spencer succeeds in virtually proving the truth of the Lamarckian assumption is expanded in his recently-published essay, from which, therefore, I will quote.

"If, then, in cases where we can test it, we find no concomitant variation in co-operative parts that are near together—if we do not find it in parts which, though belonging to different tissues, are so closely united as teeth and jaws—if we do not find it even when the co-operative parts are not only closely united, but are formed out of the same tissue, like the crab's eye and its peduncle; what shall we say of co-operative parts which, besides being composed of different tissues, are remote from one another? Not only are we forbidden to assume that they vary together, but we are warranted in asserting that they can have no tendency to vary together. And what are the implications in cases where increase of a structure can be of no service unless there is concomitant increase in many distant structures, which have to join it in performing the action for which it is useful?

"As far back as 1864 ('Principles of Biology,' § 166) I named in illustration an animal carrying heavy horns—the extinct Irish elk; and indicated the many changes in bones, muscles, blood-vessels, nerves, composing the fore-part of the body, which would be required to make an increment of size in such horns advantageous. Here let me take another instance—that of the giraffe: an instance which I take partly because, in the sixth [last] edition of the 'Origin of Species,' issued in 1872, Mr. Darwin has referred to this animal when effectually disposing of certain arguments urged against his hypothesis. He there says:—

"'In order that an animal should acquire some structure specially and largely developed, it is almost indispensable that several other parts should be modified and co-adapted. Although every part of the body varies slightly, it does not follow that the necessary parts should always vary in the right direction and to the right degree' (p. 179).

"And in the summary of the chapter, he remarks concerning the adjustments in the same quadruped, that 'the prolonged use of all the parts, together with inheritance, will have aided in an important manner in this co-adaptation' (p. 199): a remark probably having reference to the increased massiveness of the lower part of the neck; the increased size and strength of the thorax required to bear the additional burden; and the increased strength of the fore-legs required to carry the greater weight of both. But now I think that further consideration suggests the belief that the entailed modifications are much more numerous and remote than at first appears; and that the greater part of these are such as cannot be ascribed in any degree to the selection of favourable variations, but must be ascribed exclusively to the inherited effects of changed functions."

The passage then proceeds to trace these modifications of structure in detail; showing that the changes in the fore-quarters entail corresponding changes in the hind-quarters, which when running "perform actions differing in one or another way and degree from all the actions performed by the homologous bones and muscles in a mammal of ordinary proportions, and from those of the ancestral mammal which gave birth to the giraffe." Thus it is shown that bones, muscles, blood-vessels, nerves, and indeed nearly all the constituent structures of the body, have everywhere been more or less modified as to relative size and function, in order to adapt the giraffe as a whole to the unusual development of its neck: this unusual development has entailed changes, and changes, and counter-changes, which have eventually spread throughout the whole organisation of the animal.

Now, it appears to me that we have in this a most cogent argument in favour of the inherited effects of use and disuse. For, seeing how immense must be the sum of the organic changes required to produce this mutual co-adaptation of many structures, the chances against their all happening to occur together by way of fortuitous variation must be, as Mr. Spencer observes, infinity to one. Yet unless they all did occur together in the same organism—and this repeatedly—the co-adaptations in question cannot have been due to natural selection.

With more or less success Mr. Spencer develops several other lines of argument; but as they cannot well be reproduced without occupying more space than can here be allowed, I will conclude by adding to his material yet another consideration which appears to me to be entitled to great weight. When we search through the animal kingdom, we meet with certain instincts which cannot reasonably be supposed to subserve any such life-preserving function as that which has led to the survival, through natural selection, of instincts in general. Now the existence of instincts which are thus not of vital importance to the species presenting them can only be explained by the hereditary effects of function. For instance, it is difficult to suppose that the instinct, which is still inherited by our domesticated dogs, of turning round and round to trample down a comfortable bed before lying down, can ever have been of so life-preserving a character as to have been developed by survival of the fittest. Or, if this instance be held doubtful, what shall we say to the courting instincts in general, and to the play-instincts of the bower-bird in particular, which are surely quite without meaning from any utilitarian point of view? And these instincts naturally lead on to the æsthetic faculties of mankind, few of which can be possibly ascribed to natural selection, as Mr. Spencer very conclusively shows.

And here it becomes needful again to say a few words on Prof. Weismann's essay, by way of criticism. For he, too, has there considered the case of instincts, but this in a manner which can scarcely be termed fortunate. For example, he particularly instances the case of hereditary fear of enemies as one which supports his argument against the inheritance of functionally-produced modifications. Now, this happens to be one of the instincts which I have elsewhere specially chosen as yielding particularly good proof of the hereditary transmission of individual experience, apart from natural selection. And the proof consists merely in showing, from abundant testimony, that "the original tameness of animals in islands unfrequented by man gradually passes into an hereditary instinct of wildness as the special experiences of man's proclivities accumulate; and that such instinctive adaptation to newly developing conditions may take place without much aid from selection is shown by the short time, or the small number of generations, which is sufficient to allow for the change."[1] But although I think that Prof. Weismann's selection of this instinct is a particularly unfortunate one for the purpose of showing that its *acquisition* can *only* be due to natural selection, I quite agree with him in holding that its degeneration in our domesticated animals is due to the withdrawal of natural selection—at least in considerable part.

Again, he argues that if acquired mental proclivities are ever inherited we should expect the human infant, without any individual instruction, to converse. For, he argues, ever since man became human he has been a talking animal, and therefore, if there were any truth in the view that knowledge acquired by individuals tends to be transmitted to their progeny, here is a case where the fact ought to admit of abundant proof: yet every child requires to be taught its mother-tongue by its own individual experience.

Now, without waiting to show the manifest unfairness of this example—seeing how enormously complex a system of cerebral relations the speaking of even the simplest language implies—it is enough for our present purposes to observe that language has been itself the product of an immensely prolonged and highly elaborate evolution. Although it is true that man has always been a talking animal, it is very far from true that he *has always talked the same language*. As a matter of fact, he has talked in thousands of different languages, and if the genetic history of any one of them could now be traced back to its original birth, the probability is that it would be found to have passed through some hundreds of phases, no one of which would have been fully intelligible to the generations which spoke the others. Consequently, even if we were to adopt the impossible supposition that any length of time could be sufficient to enable heredity to elaborate so huge an amount of instinctive acquisition as would be required to render the knowledge of any language intuitive, there would still remain this answer to Prof. Weismann—namely, that if a child *could* talk by instinct, it would require to astonish its parents by addressing them in at least a hundred unknown tongues, before arriving at the one which alone they could understand.

So much, then, by way of answer to Prof. Weismann's supposed difficulty. But the matter does not end even here; for if he had searched the whole range of human faculties he could scarcely have found a worse example to quote in support of his argument, seeing that it admits of being turned against that argument with the most overwhelming effect. This argument is that the fact of speech not being instinctive is proof that acquired knowledge is not transmitted. Now, we have just seen it to be manifestly impossible that so elaborate, as well as so recent, a body of acquired knowledge should be transmitted—even though it were true that many instincts had been evolved in this way. Nevertheless, it might still be reasonably objected—as, indeed, Weismann says—that the simpler features which serve to characterise all spoken languages alike, and which, therefore, have always constituted the common elements of language as such—it might reasonably be urged that these simpler elements which are thus common to all languages might well be expected by this time to have become instinctive, if there is any truth at all in the Lamarckian doctrine of the inherited effects of continuous function. *But this is exactly what we find.* The only elements that are common to all languages are the simplest elements of *articulation*; and it is now established beyond doubt that the human infant is endowed with the instinct of making articulate sounds. Long before the powers of understanding are sufficiently advanced to admit of the child making any rational use of language, he begins to babble meaningless syllabic utterances. And although these utterances are extremely simple when contrasted with the enormous complexity which they are soon destined to attain in intelligible speech, yet, regarded in themselves, or as merely hereditary endowments, the evolution of mechanism which they represent is by no means contemptible. For they necessitate highly peculiar as well as highly co-ordinated movements of the larynx, tongue, lips, and respiratory muscles, not to speak of the special innervation which all this requires, or the yet more special cerebral conformation which it betokens. In short, the illustration of spoken language, far from making against the doctrine of Lamarck, is one of the best illustrations that can be adduced in its favour; for surely it is in itself a most significant fact that the young of the only talking animal should alone present the instinct of making articulate sounds—just as it also alone presents the instinct of alternately placing one leg before the other, in a manner suited to walking in an erect position.

Upon the whole, then, I conclude that the effects of use and disuse are certainly inherited; that the reducing influences of the latter are largely assisted by the cessation of selection; that the cessation of selection is itself assisted by the economy of growth,

[1] "Mental Evolution in Animals," p. 197, where see for evidence.

which constantly depresses the average size of any useless structure; and that in a comparatively few cases, where changed conditions of life have rendered a previously useful organ actively injurious, the influence of selection may not only be withdrawn, but reversed. And if in justification of these views I were required to adduce any single tests as crucial, I should point on the one hand to the neuter ants, and on the other hand to the bower-birds. For the neuter ants prove to demonstration the fact of developing such important structures as enlarged and strengthened jaws through the agency of selection, and of totally losing such important structures as wings through the cessation of selection—in both cases under circumstances which effectually preclude the possibility of any inherited effects of use and disuse. On the other hand, the bower-birds no less decisively prove the fact of developing highly elaborate and most remarkable instincts, which are entirely without reference to any life-preserving function, and therefore can be ascribed only to the inherited effects of functionally acquired peculiarities.

If this paper has been at all successful in its objects, it must have brought into prominence one point which I am particularly anxious to make clear—namely, that it is a precarious thing to differ, in any point of biological doctrine, from the matured judgment of Charles Darwin. The more deeply his work is studied, the more profoundly is the conviction impressed, that even though he did not always give it, he always had a reason for the faith that was in him. Therefore, before his followers venture to question a doctrine which was sanctioned by him, common prudence should dictate a careful pondering of the matter. Some of the readers of NATURE may have been led to suppose that as to this I am myself living in a glass house. For my recent suggestion of an additional "factor of organic evolution" has had the effect of bringing many stones about my head with regard to this very point. But these have mostly been thrown by men who have not taken the trouble to acquaint themselves with the exact nature of Mr. Darwin's final judgment upon the points in question. As a matter of fact, there is only one point upon which I have deviated at all from the latest editions of Mr. Darwin's works—namely, as to the degree in which free intercrossing is inimical to natural selection—and, curiously enough, this is just the point which my critics for the most part disregard. I am blamed for my arrogance in disputing the universally adaptive character of specific distinctions, in affirming the generality of some degree of sterility between species, and so forth; but all these criticisms only serve to exemplify the truth of what I am now saying—namely, that before anyone ventures to write about Darwinism he should take the trouble to ascertain exactly what it was that Darwin thought.

GEORGE J. ROMANES.

THE AUGUST METEORS OF 1887.

THE circumstances attending the recurrence of this celebrated meteoric display were by no means favourable in the present year. On August 10 and 11 the moon rose before 11 p.m., so that during later hours the smaller and more numerous class of meteors, many of which would have been visible on a dark sky-ground, were obliterated. Apart from this, the night of the 11th was much overcast, and comparatively few observations could be secured. But, making every allowance for hindrances of this character, the recent shower has proved itself decidedly inferior to many of the conspicuous returns recorded in previous years.

But if this notable stream has been deficient in numerical strength, it has exhibited some features which, though previously observed, have never been capable of being so definitely and satisfactorily traced in their development as during the present year. I refer to the displacement of the apparent radiant point amongst the stars, and to the visible duration of the shower, both of which form important elements in determining the physical nature of the system and in theoretical investigations as to the perturbations which our earth may have exercised upon it during the frequent *rencontres* with its materials in past ages.

The very clear weather recently experienced enabled the progress of the display to be watched on fourteen nights between July 19 and August 14, and the radiant point on each one was determined separately, as by combining the results of several nights the changes in its position would have been rendered more difficult of detection. I first pointed out this change in the radiant in NATURE, vol. xvi. p. 362, and subsequently further details were published in the *Monthly Notices* for December 1884, pp. 97-98. In NATURE, vol. xxxiv. p. 373, will also be found the observations of this peculiarity made here in 1886, but they were not so complete as during the present year, when the radiant centres were successively derived as under.

Great Perseid Radiant Point 1887.

Night.	Radiant. α	δ	Meteors.	Night.	Radiant. α	δ	Meteors.
July 19	19°	+ 51°	4	August 1	35°	+ 56°	4
22	25	+ 52	5	6	42	+ 55	5
23	25	+ 52	4	7	43	+ 56	5
27	29	+ 54	5	8	43	+ 56	6
28	30	+ 55	10	10	42½	+ 57½	22
29	31	+ 51½	10	11	45	+ 57½	16
31	35	+ 54	11	14	53	+ 57	8

It will be noticed that these figures do not show a perfectly regular progression of the radiant in the direction of east-north-east. This is, however, entirely owing to observational errors which cannot be wholly eliminated from such determinations. Thus the radiant given above for August 6 is no doubt slightly east, and the one for August 10 slightly west, of the true positions. But these trivial discordances in individual positions do not affect the general result, which shows in the clearest manner possible that there is a rapid advance of the radiant from night to night. From all my observations since 1867, which include several thousands of Perseids, I believe this shower extends over a duration of at least forty days, from July 13 to August 22. The earliest visible meteors of the stream emanate from a point between Cassiopeia and Andromeda, while the latest ones diverge from the space separating Auriga and Camelopardus.

From its first oncoming to the epoch of culmination on the night of August 10 it does not gradually intensify but reaches a somewhat sudden maximum. I have sometimes found these meteors rather scarce on August 6, 7, and 8, and not much exceeding their observed frequency at the end of July. But on August 9 there is a marked increase, and on the following night it is apparent the shower attains its most brilliant effect. As to the displacement of the radiant this seems to be accelerated during the declining stages of the display. In July I find the degrees of right ascension of the shower nearly correspond with the days of the month, the diurnal advance being equivalent to about 1° of R.A., whereas on nights succeeding the maximum the change amounts to 2° of R.A. or even more. This difference in place is so striking that any observer may determine it for himself by watching the region of Perseus at the right epoch and charting, with the utmost accuracy, the directions of such meteors as presumably originate from the Perseid stream. These meteors generally leave streaks which furnish a ready means of fixing the paths with a degree of precision that could not be otherwise attained.

In NATURE for August 4, p. 318, I described my observations up to July 29 last. On July 31 I recorded 42 meteors in a watch of 3¼ hours, but the moonlight interfered considerably with the work, as it also did on following nights. The Perseids formed one-fourth of the visible meteors on July 31. I saw 25 meteors on August 1 in 3¼ hours, but the Perseid display was only just recognizable. At 12h. 18m. I observed a splendid fireball passing somewhat slowly from 338° + 43° to 164° + 70°. It left a bright streak or thick train in the latter part of its course, and it was evidently a member of the July Aquariads. At first it was scarcely brighter than a third magnitude star, but when near Polaris it became very brilliant, and afterwards lit up the northern sky with a flash much stronger than the moonlight. I saw 7 other Aquariads on the same night.

On August 6 observations were continued, and 28 meteors were seen in 4¼ hours. Besides the usual shower of Perseids I was much interested in finding a companion radiant at 31° + 49°, which was very sharply defined. I observed a shower on August 11-13, 1880, from 30° + 46° which may be the same; and there is a great probability that this system is connected with Comet I. 1870, which passed near the earth's orbit and would give a radiant near that of the meteor shower and at the same epoch.

On August 7, 23 meteors were seen in 2¾ hours. Only 5 Perseids were recorded. On August 8, 14 meteors were seen in 2¼ hours during moonlight, and of these one appearing at 10h. 34m. was as bright as Jupiter. Its course was from 6° + 67½° to 302° + 60½°, and it left a bright streak. At 11h. 28m. a fireball was seen moving rather swiftly from 349° + 15° to 9° + 14½°, so that its path was one of 20° just above γ Pegasi. At its end point the meteor burst out with a great accession to its brilliancy, and there was a vivid flash, though the moon was near. The radiant of this fine meteor was probably near Delphinus at $304^\circ + 11^\circ$.

On August 10, before midnight, the Perseids were by no means numerous. Only 22 were seen during $1\frac{1}{4}$h., and after the moon rose the display was not critically watched, as observations made during moonlight are not comparable with those obtained under more favourable conditions. There were fine meteors now and then, but the phenomenon never developed into an imposing shower. On August 11 the sky was much overcast, and not many shooting-stars were discerned. In 1 hour before 11h. 30m., when the firmament was fairly clear, I counted 21 meteors, of which 16 were Perseids. On August 14 the weather became very fine, and I enumerated 45 meteors in a $4\frac{1}{2}$ hours' watch. There were only 8 Perseids, and amongst the meteors I registered were about 5 Aquariads from the same radiant as at the end of July. I also noticed the Aquariad shower at the middle of August in 1877, and in 1879 on August 21, 14 meteors were traced from $339^\circ - 10^\circ$, so that it would appear this system is prolonged until the end of the third week in August, and without any apparent displacement of the radiant point. The members of the latter stream are widely dissimilar in their visible aspect to the Perseids, and move slowly, often covering considerable arcs before extinction. In its chief richness the shower belongs to the July meteoric epoch, though sometimes, as in the present year, remaining conspicuous until the middle of August or even later than that, as in 1879.

Bristol. W. F. DENNING.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 16.—M. Janssen in the chair.—Note on the work recently carried out at the Observatory of Meudon, by M. J. Janssen. Special reference is made to the many successful solar photographs already obtained, representing the history of the solar disk for the last ten years. The processes are now so perfected that on the same plate the details are taken both of the brighter and less luminous parts, such as the edge of the disk and the penumbræ of the spots. Photographs ten times enlarged were exhibited of the extremely interesting spots taken on June 22, 1885, and last June. The striæ of the penumbra and the faculæ surrounding the former consist of granulations, in form and size resembling those constituting the entire solar surface. The same phenomenon reappears on the large round spot photographed last July, so that it seems all but demonstrated that the whole solar disk has a uniform constitution, and that the so-called granulations are in fact the constituent elements of every part of the surface of the sun.—Fresh researches on the relations existing between the chemical and mechanical work of the muscular tissue (continued), by M. A. Chauveau, with the co-operation of M. Kaufmann. Here a determination is made of the coefficient of the quantity of mechanical work produced by the muscles performing useful work in the physiological conditions of the normal state. By translating into absolute measurements the indications furnished by the dynamograph already referred to, it is shown that the muscular work performed may be estimated at about 31 to 35 millionths of calorie.—Some further remarks on the radicular nature of the stolons in Nephrolepis, by M. A. Trécul. In reply to M. Lachmann's recent note, the author again shows that these stolons are not stems or stalks, but true roots. No matter what their length, they never produce leaves, have always the structure of roots, and as they alone represent the primary roots of Nephrolepis, the expression "radicular stolons," applied to them by the author, is fully justified.—New fluorescences with well-defined spectral rays (continued), by M. Lecoq de Boisbaudran. The author here treats fully the combination of alumina and the earth $Z\beta_2O_3$, which, without being pure, is very rich in $Z\beta$ and poor in $Z\alpha$. Alumina with 1/50 of this earth heated with sulphuric acid and moderately calcined shows a somewhat yellowish-green fluorescence, much more vivid than that of alumina containing the same quantity of $Z\alpha_2O_3$ impure. The fluorescences have also been examined of calcined alumina containing the oxides of Ce, La, Er, Tu, Dy, Yb, Gd, Yt, and U. During these researches several rays were noticed apparently belonging to none of the already determined elementary bodies. Some of these rays may perhaps correspond to the substances announced by Mr. Crookes; but each case will have to be determined for itself.—Determination of the longitude of the Observatory of Tacubaya, Mexico, by MM. Anguiano and Pritchett. Continuous observations spread over six months show a definite longitude of 6h. 36m. 46·56s. west of Greenwich, which will require a correction of close upon 5s. for the accepted longitude of the capital of Mexico.—Electric excitement of the liver, by MM. Gréhant and Mislawsky. The question is discussed, whether the excitement of the liver by electricity increases the quantity of urea contained in the blood. In opposition to the views of M. Stolnikow the experiments here described show that variations in quantity occur only in the arterial blood, and that the blood of the supra-hepatic veins presents no change in the weight of the urea after electric excitement of the liver.—Dissemination of the Bacillus of tuberculosis by flies, by MM. Spilmann and Haushalter. Observations recently made in consumptive-hospitals seem to show that the virus (Koch's Bacillus) may easily be disseminated by the house-fly.—Note on Hæmatocytes, by M. Fokker. The author recently showed that the protoplasm taken from a healthy animal and protected from microbes survives and may produce fermentations. Here he continues his researches, showing that this protoplasm is capable of generating a vegetable form different from that under which it existed in the animal organism. But the Hæmatocytes thus produced do not multiply themselves in a cultivating medium, and their development should perhaps be described as a case of heterogeny.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Dijmphna—Togtets Zoologisck—Botaniske Udbytte: Dr. Chr. Fr. Lütken (Kjobenhavn).—Seven, the Sacred Number: R. Samuell (K. Paul).—University College, Dundee, Calendar 1887-88 (Leng, Dundee).—Qualitative Chemical Analysis: Dr. C. R. Fresinius, 10th edition, translated by C. E. Groves (Churchill).—Notes to accompany a Geological Map of the Northern Portion of the Dominion of Canada: G. M. Dawson (Montreal).—Die Geoiddeformationen der Eiszeit: E. von Drygalski (Berlin).—Proceedings of the Linnean Society of New South Wales, 2nd series, vol. ii. Part 1 (Cunninghame, Sydney).—Verhandlungen der Naturhistorischen Vereines, Fünfte Folge, 4 Jahrgang, Erste Hälfte (Max Cohen, Bonn).

CONTENTS.

THURSDAY, SEPTEMBER 1, 1887.

HIGHER ALGEBRA.

Higher Algebra: a Sequel to Elementary Algebra for Schools. By H. S. Hall, M.A., and S. R. Knight, B.A. (London: Macmillan, 1887.)

ANYONE who imagined from the shortened title of "Higher Algebra," which appears on the back of this volume, that the work extended to that higher region of algebra to which Salmon's well-known "Lessons" are "introductory," would be surprised to find that it contains little beyond what may fairly be regarded as "elementary algebra." Indeed it appears to us that much that is contained in the earlier chapters would have found its place more appropriately in the same authors' "Elementary Algebra for Schools," using their own device of an asterisk to indicate those articles which might, in the case of the ordinary school-boy, be omitted or reserved for a second reading; and thus the awkwardness of breaking up such subjects as ratio, proportion, and progressions into separate parts, by an arbitrary division into lower and higher, would have been avoided.

Apart from this defect of plan, the work before us has great merits as a text-book adapted to the wants of the ordinary student of algebra and to the exigencies of examinations. It is a development and improvement upon "Todhunter," as "Todhunter" was a development and improvement upon "Wood." The main framework is the same: many of the proofs of algebraical theorems have been replaced by better proofs, and new matter has been introduced. Still it remains essentially an artificial framework and has no claim to be regarded as an organic growth from a few central principles, with a corresponding natural relation and affiliation of parts. Thus we find the fundamental laws of algebra for the first time gathered together and discussed in the thirty-fourth chapter (p. 429) of this volume, a chapter of "Miscellaneous Theorems and Examples" for which apparently no fitting place could be found in the framework. It also includes such a fundamental theorem as that known as the "remainder theorem"—that $f(a)$ is the remainder when the rational integral function $f(x)$ is divided by $x - a$—some of its applications, as well as some discussion of symmetrical expressions and identities.

An elementary algebra, written by a master of modern algebraical science in the light of the higher views of the essential nature of algebra which modern investigations have established, and yet with such simplicity that it may be put into the hands of the school-boy, is a desideratum the advent of which is perhaps foreshadowed, though not fully realized, in respect of simplicity at any rate, in Prof. Chrystal's recent work. It would be obviously unfair to criticize the present work from this point of view: our remaining remarks on it, therefore, will be confined to some matters of detail in the order of the chapters of the book itself.

Perhaps the strongest part of the book is the examples, both those which are worked out, and those which are added to each chapter as exercises for the pupil. As far as we have been able to examine them, they are sufficiently numerous, well chosen, and instructive, and also well arranged in each exercise in the order of their difficulty. We are surprised to find in the chapter on "Miscellaneous Equations" that there is no hint or caution given that the root obtained may not satisfy the original equation unless the sign of one or more of the radicals involved in it is changed. In fact, in the example worked out on p. 99, the root $x = 6a$ gives by substitution in the equation $2a - 6a = 4a$! We hold that in all such cases the student should be required to show with what signs of the radicals in the equation each solution is consistent, and what combinations of their signs are impossible; otherwise more than half the instructiveness of the example is lost.

The chapters on ratio and proportion need no remark; but that on variation, as in most books of algebra, is in our opinion unsatisfactory, from the fact that the attention of the student is not called to the distinction between a magnitude and its numerical measure. If A stand for the distance and B for the time, when the speed is uniform, "A varies as B" is a statement true of the magnitudes themselves independent of any particular mode of measuring them; but when from this is deduced the equation $A = mB$, either A and B must be regarded as numerical measures of the distance and time with reference to some particular units, in which case m will have a value depending on the units selected; or else m is a multiplier which, besides altering the numerical value of B into that of A, converts a time into a distance, an extension of the notion of multiplication which, if admitted, ought to be very carefully noted and explained.

After chapters on progressions, we come to one on scales of notation, though there is no reason, apart from the traditional place it has occupied in books on algebra, why such simple questions as are discussed in it, which, if arithmetic were rationally taught, would have been treated in connexion with the theory of decimal numeration and notation, should be regarded as forming a chapter of "Higher Algebra." The algebraical formulæ which encumber this chapter should only be introduced as summing up what has been previously proved in particular instances by direct reasoning from first principles, not in order to prove the propositions themselves.

It would have been well if the chapter on the theory of quadratic *equations* had been made one on that of quadratic *expressions*. By not doing this the opportunity is lost of exemplifying the notion of *continuity* in the changes of such expressions with the change of the variable both in magnitude and sign and their maximum and minimum values, as well as the introduction of the graph (as Prof. Chrystal has done), to illustrate these changes.

The authors state in their preface that the part of algebra which is concerned with permutations and combinations "is made far more intelligible to the beginner by a system of common-sense reasoning from first principles than by the proofs usually found in algebraical text-books," a proposition with which we heartily agree, only that we see no reason why it should be confined to this particular part of algebra.

When we turn, however, to the chapter on permutations and combinations, except that there is a greater variety of proofs, we fail to find any further appeal to "common-sense reasoning from first principles" than in other text-books. In fact, in some of the proofs the crucial point of the proposition, instead of being elaborated, is so condensed as to make it very difficult to understand, though it is certainly put in a form which may be easily carried into an examination to the perplexity of the examiner, who may well be in doubt whether the examinee who reproduces the words really sees the point of the proof. We hold that the true way of appealing to "common sense" is to take particular cases first, and when these are grasped, the general proof becomes easy. Thus, to find the number of permutations of 4 things (a, b, c, d) taken 3 together, it is plain that the arrangement

$$\overbrace{\overbrace{b}^{}\quad c \quad d}^{a}$$

$$\underbrace{\;a\;}_{\overbrace{b}^{\overbrace{c\ d}} \quad \overbrace{c}^{\overbrace{d\ b}} \quad \overbrace{d}^{\overbrace{b\ c}}}$$

repeated for each of the 4 letters in the top line will give all possible permutations, and that the number is therefore $4 \times 3 \times 2$, and further that the principle of arrangement may be extended to any number of things. This is the essence of the proof given on page 116. It may be said that such exemplification is in the province of the teacher rather than in that of the text-book, but we fear there are many teachers who fail to make things clear in this way to their pupils.

The proof, or rather proofs, of the binomial theorem for positive integral indices are distinct improvements on the cumbrous proof given in Todhunter, the theorem being shown, as it should be, to be a direct consequence of the multiplication of n binomial factors. Euler's proof for any index is carefully stated, and its crucial point emphasized by a preliminary discussion.

Following the binomial theorem comes a chapter on logarithms, which in our opinion would have better followed the chapter on indices in the "Elementary Algebra," as that on interest and annuities might have followed those on progressions. The exponential and logarithmic series would then have followed naturally as a development of the binomial theorem.

The authors have given a chapter on the convergency and divergency of series, in which this important subject is treated with unusual care. We may perhaps demur to the sweeping character of the statement (p. 249) that "the use of divergent series in *mathematical reasoning* leads to erroneous results," but the student cannot be too early or too emphatically warned that a result obtained by the use of divergent series should be verified by other means.

The chapters which follow treat of intermediate coefficients, partial fractions, recurring series, continued fractions, indeterminate equations of the first degree, recurring continued fractions, and indeterminate equations of the second degree, summation of series, the usual elementary theorems of the theory of numbers, the general theory of continued fractions, and probability. All these subjects appear to us to be judiciously and adequately treated, though we should have been glad to see a little more of "common-sense reasoning from first principles" in the elementary chapter on continued fractions, by which it might easily and with advantage have been made to take its place among the chapters of the "Elementary Algebra." In the chapter on summation of series, the authors, as they tell us in the preface, have laid much stress on the "method of differences." As they have gone so far, we think it is a pity that they have not introduced the notation and the elementary propositions of the calculus of differences, which seem to us very naturally to fall within the limits of algebra. In any case, in their use of the symbol Σ they should not have deviated from its proper meaning by making Σn, for instance, include the term n instead of denoting by it the series ending with $n-1$.

Here the ordinary treatises on algebra end. Our authors have, however, very wisely added a chapter on determinants, containing a satisfactory and sufficient discussion of determinants of the second and third orders, with a useful series of examples of their application, and an indication of the general properties of determinants of any order. The study of this chapter will enable the student to read, without difficulty, treatises on analytical geometry, and afford a good introduction to special works on determinants in general.

Following this is the chapter on miscellaneous theorems and examples, of which we have before spoken, containing a short discussion of the fundamental laws of algebra, then the "remainder theorem," and synthetic division, symmetrical and alternating functions, and elimination.

While the end of ordinary algebra and its various direct applications is undoubtedly a suitable place for a rediscussion of its fundamental laws, as preliminary to the interpretations of double algebra and to the various higher algebras with different fundamental laws, it is strange that our authors have not found the desirability, indeed the necessity, of introducing the other subjects of this chapter, with the exception perhaps of elimination, at a much earlier stage, and as part of a regular sequence in the development of algebraic operations.

The book concludes with a chapter containing the elementary parts of the theory of equations—on the whole judiciously selected. We note it, however, as a defect in this, as in all other treatises we have met with, that Horner's process for approximating to the roots of numerical equations is barely mentioned. We hold that the simplicity and generality of this process is such that it ought to be taught, as a rule (without proof), for finding the roots of numbers, in all treatises on arithmetic, to the exclusion of the cumbrous, uninstructive, and utterly useless method of finding cube roots only, which is usually given; while the proof of the process, which may be made quite easy and intelligible, and its general application to numerical equations, ought to occupy a prominent and early position in every treatise on algebra. Everyone who has made himself expert in the use of Horner's method will, we are sure, agree with us that it gives a power in discussing an algebraical expression with numerical coefficients, which can be obtained in no other way.

R. B. H.

OUR BOOK SHELF.

Outlines of Quantitative Analysis. By A. H. Sexton. (Charles Griffin and Co., 1887.)

IT is perhaps as great an evil to err on the side of trying too much as to do too little where more might be done. In this book, intended, as the author tells us, to be put into the hands of students who have but little time to spare and may not intend to become professional chemists, a very wide analytical field is got over; indeed a little too much is attempted in the space, and sacrifices have in nearly all cases to be made where "shortness and simplicity" is the combined ruling idea.

We fully agree with what the author says as to the educational value of quantitative analysis. It is indeed high time that our more elementary students should have the long courses of qualitative analysis shortened, and some more *exact* exercises substituted.

In the course of the 127 pages of this book, including six for tables, we are introduced to the balance, and it is much to be regretted that more has not been said about it. What is said is purely practical—how to turn up the handle and put on the weights.

The first exercises are the determination of water in a carbonate and the ash in several substances, after which a couple of specific gravity methods are given, and then we pass to "simple gravimetric analysis," iron, silver, barium, lead, &c. In the silver exercise the factor 0·75276 is introduced to get the actual silver from the weight of chloride found, and this "factor" is given in all other analyses. It is not of much use any way, and for beginners it is not advisable, as it binds them down to the book, and no appreciable time is saved for ordinary analysis calculations.

The directions for volumetric analysis are very good, and the exercises are well arranged in order of difficulty. The separation exercises and miscellaneous examples will need some attention from the teacher.

In the description of organic analysis—combustion of carbon compounds—the closed-tube process is well described, and a student might be able to do a combustion from the description only; but we are not informed, when the open tube is spoken of, whether the same length, viz. 18 inches, will be sufficient or not. By inference it will. We venture to say that a very doubtful analysis, especially of a volatile body, would result from the use of an open tube only 18 inches long. The description here is much too slight to work by.

The tables at the end are sensible—only just those wanted in the course of the work in the book itself.

Qualitative Chemical Analysis. By Dr. C. Remigius Fresenius. Tenth Edition. Translated and edited by Charles E. Groves, F.R.S. (London: J. and A. Churchill, 1887.)

THE fifteenth German edition of this well-known book contains many emendations and additions, especially in the concluding portions devoted to the reactions of the alkaloids and the systematic methods of detecting them. Of this edition of the original work the present edition of the English translation is as nearly as possible an exact reproduction, and much credit is due to the translator and editor for the care with which he has accomplished a very difficult task. Various styles of type and other typographical improvements have been introduced, in the hope, as Mr. Groves explains, that the book may thereby be rendered more handy and useful to students.

Melting and Boiling Point Tables. Vol. II. By Thomas Carnelley, D.Sc., and Professor of Chemistry in University College, Dundee. (Harrison and Sons, 1887.)

THE issue of vol. ii. of this important work completes it. It is not too much to say that these two volumes will be found in every laboratory. Their compilation represents an amount of patient work from which most men would have recoiled; and the total result, which has cost ten years of effort, reflects the highest credit upon Prof. Carnelley.

Part II., dealing with organic compounds, brings the data down to 1885.

Part III. deals with vapour tensions and boiling points of simple substances, and freezing and melting points of cryohydrates, including facts recorded 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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Law of Error.

EVERYONE interested in the theory of statistics is aware how strongly Quetelet was under the conviction that there is only one law of error (or curve of facility, to use the corresponding expression for the graphical representation of the law) prevalent for the departure from the mean of a number of magnitudes or measurements of any natural phenomenon. I have done what I can to protest against this doctrine as a theoretic assumption; and recently Mr. F. Galton and Mr. F. Y. Edgeworth have shown in some very interesting and valuable papers in the *Philosophical Magazine* and elsewhere how untenable it is, and how great is the importance of studying the properties of other laws of error than the symmetrical binomial, and its limiting form the exponential.

I have been making some calculations recently, principally in the field of meteorology, and I should be extremely glad of the

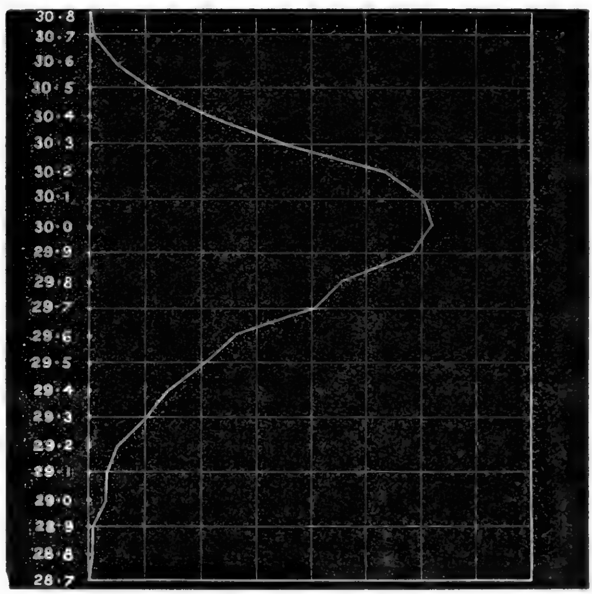


FIG. 1.

judgment and criticism of any of your readers who may be better versed in this science than myself. It must be carefully understood that the questions here raised are solely these:—(1) Do the magnitudes, when arranged in order of their departure from the mean, display a *symmetrical* arrangement? (2) If so, is this arrangement in accordance with the binomial or exponential law?

The first diagram represents the grouping, in respect of relative frequency, of 4857 successive barometric heights. They are from the observations of Mr. W. E. Pain, of Cambridge, and show the readings at 9 a.m. on successive mornings for about thirteen years from January 1, 1865. They are the results of the same instrument, which has required no correction or alteration during that period. They are given to the first decimal place.

The second diagram refers to a similar set of 4380 thermometric observations (1) of the maximum, (2) of the minimum temperature on successive days[1] from January 1, 1873.

In regard to the first diagram the asymmetry is obvious. I have tested the conclusion in the usual way. For instance, the total of 4857 observations was composed of seven batches of a little less than two years each. Precisely the same asymmetry, in varying degrees, is displayed by each of these batches. The asymmetry is of course obvious to the eye in the diagram, but various numerical tests may be proposed. For instance, we may compare (1) the position of the mean value (in this case 29·91) between the extreme values, (2) the relative positions of the maximum ordinate and the mean ordinate, (3) the comparative magnitudes of the "mean errors" to the right and the left of the mean ordinate. They all yield a result in the same direction.

I should be very glad if any of your readers could confirm (or correct) these results by those of more extended observations, or by results taken from other districts. That something of this kind should be displayed where, as here, we are dealing with a one-ended phenomenon—*i.e.* with one in which unlimited variation was conceivable in one direction but not in the other—seems to me in itself reasonable. But I was certainly surprised to find it so marked, considering how small is the fluctuation in relation to the actual magnitude of the variable phenomenon.

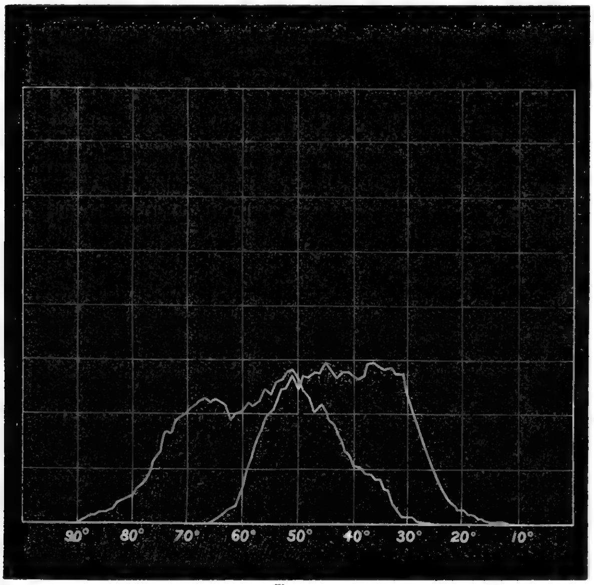


FIG. 2.

It seems to suggest that the common theoretic assumption of a sort of fixed mean or type which is swayed about by a large number of equal and opposite independent disturbing causes, does not hold good in this case.

As regards the second diagram, the two curves are (especially that of the minima) tolerably symmetrical, but they depart widely from anything approaching to Quetelet's supposed fixed type.

Anyone looking at the curve of maxima would say at once that it mingled the results of two distinct means (in Quetelet's phrase), as if we were to group together the observed statures of a great many Scotchmen and Frenchmen. That we are mingling results of distinct means seems true enough, but not of *two* such, and I cannot account for the two peaks in the curve. What I should have expected would have been something of this kind: Each day has its own appropriate mean maximum (subject to the usual fluctuation), and these mean maxima are themselves grouped about *their* mean, hence the true mean of all ought to be decidedly the commonest result, *i.e.* the curve should have a single vertex.

The facts are quite otherwise. The depression towards the centre is far too deep to be accidental, and the final mean (*i.e.* about 57°) is very far from being the commonest value.

Somewhat similar remarks may be made about the curve of minima. There is some evidence (though not conclusive) of a depression towards the centre in this case also, and the curve is very fairly symmetrical. But the true mean of all the minima cannot claim any numerical preponderance over any other value between 32° and 52°.

I am far too deeply conscious of the numerous pitfalls which lurk about the statistician's path to offer these results with any great confidence. But considering how large is the number of observations included, it certainly seems to me that they call for some explanation. There may of course be some blunder in the calculations, but I have done my best to guard against this. What I trust is that these results may be the means of calling forth some discussion by practised experts in this branch of statistical inquiry, which may serve to confirm or correct my results, and in the former case to offer some explanation of the causes of the phenomena. Very likely this practical inquiry has been already undertaken elsewhere, but the statistics of meteorology are so vastly extensive that it is impossible for any but a professional student of the subject to be acquainted with what goes on in it. J. VENN.

Cambridge.

[1] In this case, as the lengths of the successive ordinates from the original data were very irregular, I have smoothed the curve out by taking the mean of three successive heights. For instance, to take the actual figures, the number of occasions on which the maxima were 58°, 59°, and 60°, were respectively 108, 99, and 124; I have assigned the number 110 to 59°, and so on.

The Sense of Smell in Dogs.

WILL Mr. Russell (whose letter in NATURE of August 4 I have just read) be so good as to make another experiment with his pug bitch? He says that she had been "taught to hunt" for biscuit; probably she was also enjoined to "*find it*," or something similar, when she came into the room. Can he manage to try her powers without awakening her expectation?

I ask it because it seems to me that in this case (and many others) we have something different to observe than mere quickness or keenness of sense, and something well worthy of observation; namely, exclusive direction of the attention of a sense—if I may so term it.

We may note this mysterious power in ourselves to a certain extent. In the case of a dog or bird, or any other in which there is little brain work going on to cause distraction, it may be much greater, and account for many wonderful things. It may be said that this is trying to explain the unknown by the even less known; nevertheless, by gathering together many and varied instances of the action of any power some light must be thrown upon it. The mesmerizer seems to deal with this one when he closes all avenues to the senses of his subject except the one he wishes to keep open.

The sense of hearing in some birds seems as wonderful and discriminating as that of smell in dogs. I have watched with astonishment a thrush listening for worms—as their manner is—and very evidently hearing them too, within two yards of a noisy lawn-mower on the other side of a small hedge of roses. Probably the worms came nearer to the surface in consequence of the vibration caused by the machine—they are said to do so—but that the thrush *heard* and did not *see* them was evident. Robins appear to be able to distinguish the voices of their own offspring and parents from a number of others, and at a great distance. I say *appear*, for in such a case one cannot be quite sure, still less can one give all the small details of long-continued observation that make up the evidence in favour of it.

All these cases have a common and mysterious element. It is as if a window were opened in one direction and all others closed; or a chord set vibrating that answers, as a struck glass answers, only to one note; or as if all the available energy were directed along one narrow path. At any rate there is something more than mere keenness of sense. J. M. H.

Sidmouth.

Electricity of Contact of Gases with Liquids.

WILL you allow me to ask Mr. Enright (NATURE, p. 365) how he proved that the "charge of the escaping hydrogen was positive" or negative, as the case may be? That the escaping *spray* was electrified by friction, after the manner of the steam spray in Armstrong's old hydro-electric machine, is a natural explanation of these capricious effects; but that *gas* should be thus electrified, and that this electrification should have any relation whatever to the subject of "atomic charge," are propositions which strike one as improbable.

OLIVER J. LODGE.

The Lunar Eclipse of August 3.

I OBSERVE the account given by "H. H." (p. 367) of the eclipse of the moon as seen at Hamburg on August 3. Here the appearance was certainly unusual; at least I never saw anything like it. The shadow cast on the moon (with a perfectly cloudless sky) was irregular and jagged. I at first thought it was a cloud, but, on looking repeatedly at intervals, I continued to observe the same appearance; allowance being made for the progress of the eclipse. I was prevented by circumstances from watching continuously, but observed it at a little before 9, and again repeatedly between 9 and 10. M. C.

La Tour de Peilz, August 22.

As seen from Killin, on Loch Tay, the shadow on the moon had no form similar to that given by "H. H.," in your issue of August 18 (p. 367); the sky was clear, and it seems possible that the clouds caused the straight lines shown in the diagram.

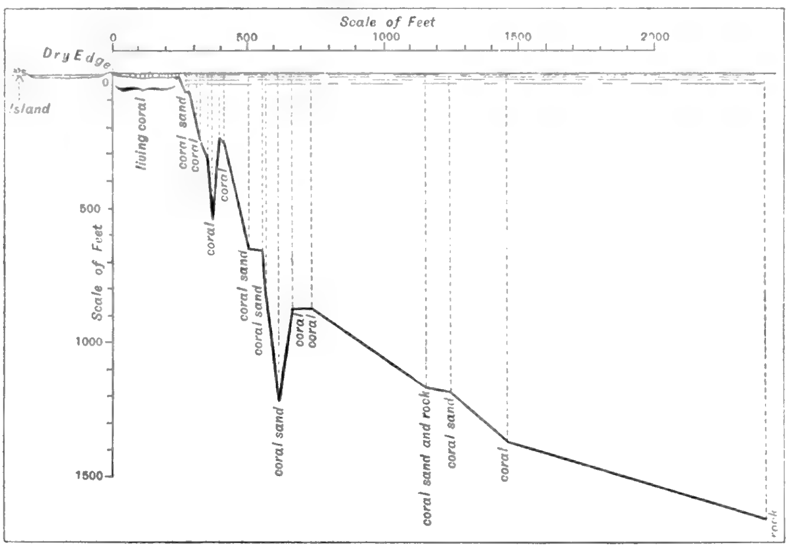
H. P. MALET.

MASÁMARHU ISLAND.

CAPTAIN MACLEAR, commanding H.M.S. *Flying Fish*, obtained, during his voyage home in April last, two sections of the slope of the coral reef surrounding the small island of Masámarhu, situated in the Red Sea in lat. 18° 49′ N., long. 38° 45′ E.

As accurate sections of reefs standing in deep water are comparatively rare, I have thought that a permanent record of them in the pages of NATURE will render them

SECTIONS OF CORAL REEF OF MASÁMARHU, RED SEA.



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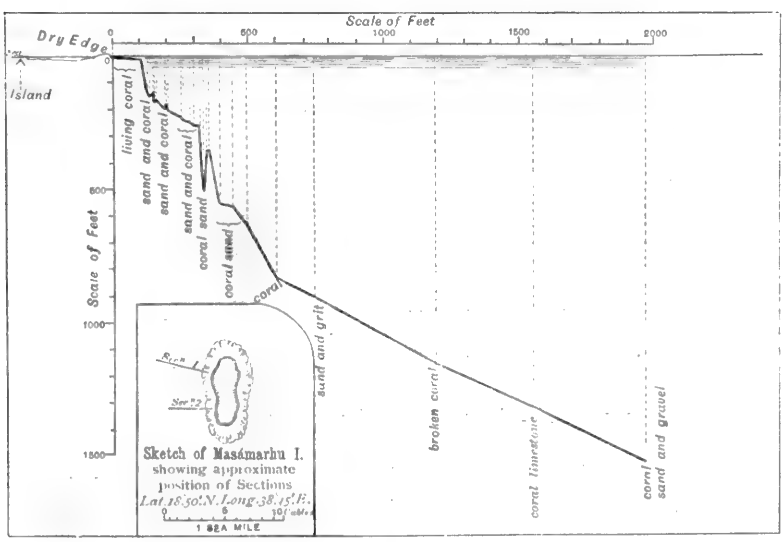


No. 2.

more available to those interested. The reduced copies of these sections, appended, show most of the more important features. They are drawn on equal scales vertical and horizontal, showing the true slope. The dotted vertical lines show where the soundings were obtained. Specimens of coral sand brought home were not from depths sufficient to show the changes of the life on the coral slopes. Mr. John Murray, who has examined them, reports as follows:—

"The fragments of coral belong to *Stylophora palmata*, Blain., a common Red Sea species; and the others to the genera Stylophora and Echinopora, but too fragmentary for specific determination.

"The beach sand has a mottled red and white appearance. The particles are nearly all rounded, and have an average diameter of 3 or 4 millimetres. They consist of corals, Echinoderms, calcareous Algæ, Gasteropod and Lamellibranch shells, and many Foraminifera. Among the latter the following could be recognized: *Peneroplis pertusus*, Forsk.; *Orbitolites complanata*, Lam.; *Rotalia calcar*, d'Orb.; *Amphistegina lessonii*, d'Orb.

"The hardened rock, 'from high-water line near section 2, solid and firm in the sand and similar to the slabs of the south-east shore,' is made up of precisely the same particles as the sand above described, cemented by the infiltration of carbonate of lime among the particles. No mineral particles other than those of organic origin were observed in the sand or hardened slabs."

W. J. L. WHARTON.

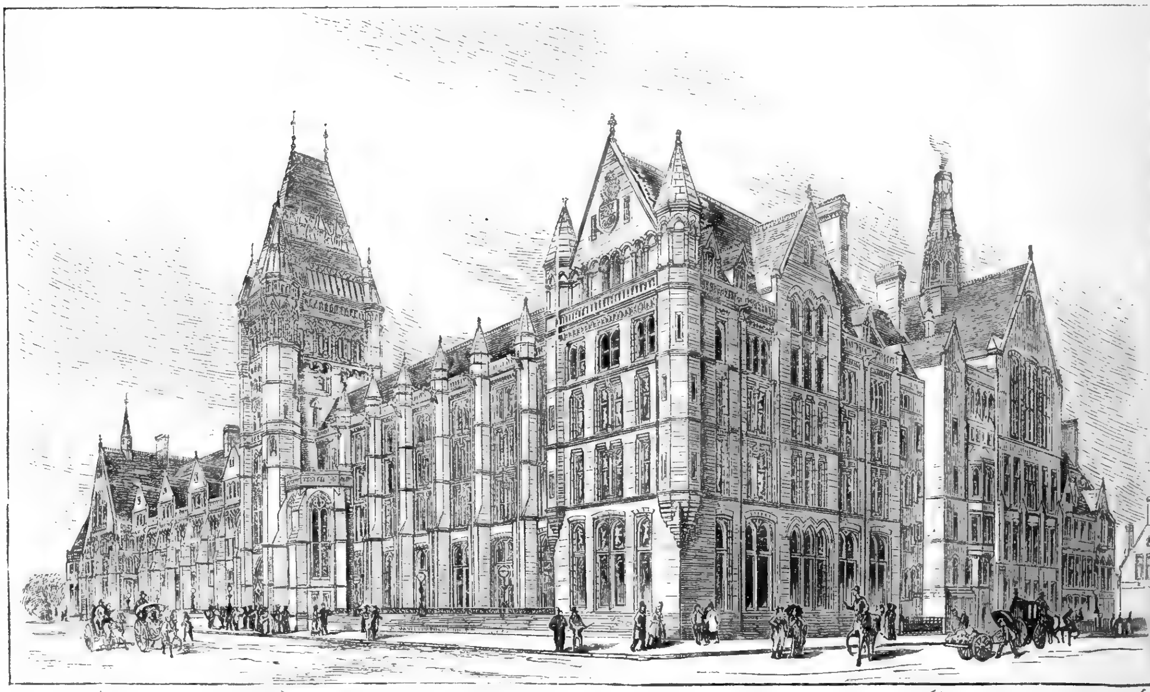
THE OWENS COLLEGE NATURAL HISTORY BUILDINGS.

THE recently completed Natural History Museums and Laboratories form an important addition not merely to the Owens College itself but to the teaching appliances of the country at large.

The buildings, which, like the older part of the College, are from the plans of Mr. Alfred Waterhouse, R.A., extend along the north and east sides of the College quadrangle, the main frontage being towards the Oxford Road. They include a lofty central tower and entrance gateway, large and convenient museums for the various departments of natural history, and a very extensive and well-equipped series of laboratories for zoology, botany, geology, and mineralogy, with lecture-theatres, class-rooms, and private rooms for the professors and demonstrators. The total cost, including fittings, will not be less than £80,000.

The general appearance of the new buildings from the north-east is shown in the illustration.

The Museum block extends along the eastern or Oxford Road frontage, and is approached from the main entrance beneath the central tower; it is also in free communication with the several laboratories. It consists of two main stories, the upper of which has its floor area almost tripled by two very wide galleries, in addition to



Future Extension for Library and Examination Hall. — Museum Block. — Laboratory Block.

VIEW OF THE NEW BUILDINGS FROM THE OXFORD ROAD.

which there is very extensive storage space in the roof. The ground floor is divided into geological and mineralogical museums, measuring respectively 90 feet by 50 feet, and 65 feet by 26 feet, the former extending along the Oxford Road, the latter facing north, towards Coupland Street. These are lighted from the sides, and will be divided into bays by the main cases, which are placed at right angles to the walls, extending from them to the pillars supporting the roof. In the centre of each bay there will be a large table case, and a smaller one under the window. This arrangement gives at once a maximum of light and a maximum of what is practically wall space; while the division into bays greatly facilitates the classification of the collections, and the different forms of case in each bay enable objects of all kinds to be displayed to advantage.

The upper museum, which is approached by a very handsome staircase in the tower, is similarly divided into zoological and botanical portions. It is lighted both from the sides and above, and the general arrangement of the cases will be the same as in the lower museum, with the addition of long rows of table cases round the edges of the galleries. Two large rooms, for use as articulating and preparation rooms, open directly on to the floor of the museum.

Owens College already possesses very important natural history collections, though owing to want of space it has been impossible up to the present time to arrange or utilize them in a proper manner. The nucleus is formed by the collections previously in the possession of the Manchester Natural History and Geological Societies, which were transferred to the College in 1867 and 1869 respectively: to these, very valuable additions have since been made by gift, bequest, or purchase. The general Geological collection is a very good one; the Tertiary collections, including those made by Prof. Boyd Dawkins and by Mr. Waters, being of exceptional importance, and the Coal Measure series being one of the best in existence. In Mineralogy the David Forbes Collection, which was purchased by the College in 1877, is well known. In Zoology there is a good osteological series; and the collections of shells, including those presented by Mr. Cholmondeley and by Mr. Walton, and of insects are unusually complete, and in exceedingly good condition. The Botanical Museum contains a very fine British herbarium, and Prof. Williamson's unique collections illustrating the Carboniferous flora. The Museum will thus start very fairly equipped, and it may reasonably be hoped that the stimulus caused by the opening of the new buildings will lead to additional gifts and bequests, which will speedily render the collection one worthy in all respects of the College and of the city which has created it.

In the Laboratory block, which occupies the north side of the quadrangle, between the older buildings and the Museum, and is shown in the right-hand corner of the illustration, the ground floor contains on the inner side two lecture-theatres, seating 200 and 80 respectively, with convenient preparation and diagram rooms. On the other side, facing the street, are the mineralogical and petrological laboratories, geological laboratories, geological drawing room, a laboratory for applied geology, and private rooms for the professors and lecturers.

The Botanical Department is on the second floor, and comprises a large laboratory 42 feet by 28 feet, private rooms for the professor and for the demonstrator, and a dark room for physiological experiments. Provision is also made for a greenhouse 20 feet square, in direct connexion with the Laboratory.

The Zoological Laboratories occupy the third and part of the second floor.

The Junior and Senior Laboratories, which are in free communication with each other, measure 42 feet by 37 feet and 42 feet by 16 feet respectively; they are 29 feet high, and are extremely well lighted and equipped. In the Junior Laboratory the tables run north and south; each student has his own locker and drawer at his side, and gas- and water-supply in front of him; larger sinks with hot-water spirals are in the corners of the rooms; a large demonstration-table, with drawers and cupboards beneath, occupies the centre of the room; and a lecture-table and black-board are placed against the north wall. In the Senior Laboratory the tables face north. A gallery runs along the east and west walls of the laboratories, but has not yet been fitted up.

Besides these laboratories there are a Zoological Research Laboratory 42 feet by 16 feet; private rooms for the professor and for the demonstrators; a very convenient tank-room; and large storage space.

The building has concrete floors throughout; the heating is by hot water, and there is a very efficient system of ventilation. At each floor there is free communication between the Laboratory and Museum blocks, and the lift is placed midway between these two.

The Zoological and Botanical Laboratories have been in use since Christmas; the Museum will not be fitted up till October. An excellent opportunity for seeing the buildings is afforded by the meeting of the British Association. The ground-floor museum is being used for the reception-room and post-office, and the upper museum for reading- and writing-room, ladies' room, smoking-room, &c.; while the quadrangle is occupied by temporary luncheon-rooms and lavatories. The Section Rooms are partly in the College and partly in its immediate neighbourhood.

THE BRITISH ASSOCIATION.

MANCHESTER, *Tuesday Evening*.

UP to the present the third Manchester meeting of the British Association promises to be as successful as everyone expected it would be. Probably no Local Committee has ever made more strenuous exertions to command success than that which for many months past has been busying itself with preparations for the present meeting. It would be difficult to suggest any improvements on the local preparations. The Reception Room in Owens College is spacious and is entirely confined to business. The Reading Rooms, Ladies' Rooms, Smoking Rooms, and Exhibition Galleries are all upstairs away from the crowd and noise. The Luncheon Rooms can accommodate hundreds, and the Sectional Rooms have had the special care of the Committee, several of whom know well the practical requirements of Sectional work. It is perhaps unfortunate that the rooms for D, E, F, and G are a long way from the Reception Room; but that has been unavoidable. The exhibition in the galleries of the Reading and Writing Room is of special interest. The anthropological collections contributed by Dr. Fritsch, Mr. Coutts Trotter, and others, are extensive and varied and highly instructive. Besides these there are collections of physical instruments by Sir William Thomson and Mr. W. H. Gee, and a fine series of models and apparatus for teaching practical physics in schools and colleges, exhibited by the Owens College Physical Department. In Section C, Prof. Boyd Dawkins exhibits several museum appliances, and Mr. J. H. Teall a series of specimens illustrating his paper on "The Origin of Certain Banded Gneisses." Other exhibits come under Sections D, G, and H, and the whole collection is likely to attract many visitors.

It is not expected that in numbers the present meeting will exceed the Newcastle meeting of 1863, when 3335 persons were present, or even the last meeting in this city in 1861, when the number reached 3138. But of course at present it is impossible to say. Some weeks ago the number who had taken tickets exceeded 2000, and to-day and to-morrow it is probable that at least another 1000 will be added. Whatever may be the number, it is certain that few past meetings of the Association will have surpassed the present in quality and weight. The marked feature is the number of foreign men of science who have promised to attend. The names of most of them have already appeared in NATURE. Their presence is entirely due to the exertions of the Local Committee, and especially, we believe, of Dr. Schuster. Nearly every man of any eminence in science abroad had a cordial letter of invitation to come to Manchester as a guest of the Local Committee, and the result is that over 100 have accepted. Among those who have arrived in Manchester to-day are Prof. Riley, of Washington; Prof. Rowlands, of Baltimore; Prof. Langley, of Michigan; Prof. Dewalque, from Belgium; and Prof. Fittica, of Marburg. Among others who are expected to-morrow I need only mention such names as those of Cleveland Abbe, Neumayer, A. C. Young, Asa Gray, Mendeléeff, Pringsheim, G. Wiedemann, Wislicenus, F. Zirkel, De Bary, Cohn, His, and the two Saportas.

Several important discussions have been arranged for. One between Sections C and D on the arrangement of natural history museums, will be led off by Dr. Woodward on Friday morning. There will be then other discussions in Section D on questions of the greatest scientific interest, while electrolysis will come up again, I believe, in Sections A and B. A joint discussion on gold and silver has been arranged between Sections C and F. As these discussions will be real, and as several eminent foreigners are expected to take part in them, the meeting on the whole promises to be lively.

The social distractions—*conversaziones*, receptions, dinners, and excursions—are perplexingly numerous. The hand-books for the excursions have been got up with much care and thoughtfulness. There is, indeed, a separate little hand-book for each excursion, the whole set being done up in a case. Another hand-book of about one hundred pages gives an epitome of the history, antiquities, meteorology, physiography, flora and fauna of Manchester and the district.

Thus, so far as the officials are concerned, everything has been done to make the Manchester meeting a success. At the present moment the weather is not quite what could be wished; it is raining hard, and the weather is oppressively sultry. We can only hope it will improve before active operations begin.

Inaugural Address by Sir Henry E. Roscoe, M.P., D.C.L., LL.D., Ph.D., F.R.S., V.P.C.S., President.

Manchester, distinguished as the birthplace of two of the greatest discoveries of modern science, heartily welcomes to-day, for the third time, the members and friends of the British Association for the Advancement of Science.

On the occasion of our first meeting in this city in the year 1842, the President, Lord Francis Egerton, commenced his address with a touching allusion to the veteran of science, John Dalton, the great chemist, the discoverer of the laws of chemical combination, the framer of the atomic theory upon which the modern science of chemistry may truly be said to be based. Lord Francis Egerton said:—"Manchester is still the residence of one whose name is uttered with respect wherever science is cultivated, who is here to-night to enjoy the honours due to a long career of persevering devotion to knowledge, and to receive from myself, if he will condescend to do so, the expression of my own deep personal regret that increase of years, which to him up to this hour has been but increase of wisdom, should have rendered him, in respect of mere bodily strength, unable to fill on this occasion an office which in his case would have received more honour than it could confer. I do regret that any cause should have prevented the present meeting in his native town from being associated with the name"—and here I must ask you to allow me to exchange the name of Dalton in 1842 for that of Joule in 1887, and to add, again in the words of the President of the former year, that I would gladly have served as a doorkeeper in any house where Joule, the father of science in Manchester, was enjoying his just pre-eminence.

For it is indeed true that the mantle of John Dalton has fallen on the shoulders of one well worthy to wear it, one to whom science owes a debt of gratitude not less than that which it willingly pays to the memory of the originator of the atomic theory. James Prescott Joule it was who, in his determination of the mechanical equivalent of heat, about the very year of our first Manchester meeting, gave to the world of science the results of experiments which placed beyond reach of doubt or cavil the greatest and most far-reaching scientific principle of modern times; namely, that of the conservation of energy. This, to use the words of Tyndall, is indeed a generalization of conspicuous grandeur fit to take rank with the principle of gravitation; more momentous, if that be possible, combining as it does the energies of the material universe into an organic whole, and enabling the eye of science to follow the flying shuttles of the universal power as it weaves what the Erdgeist in "Faust" calls "the living garment of God."

It is well, therefore, for us to remember, in the midst of the turmoil of our active industrial and commercial life, that Manchester not only well represents the energy of England in these practical directions, but that it possesses even higher claims to our regard and respect as being the seat of discoveries of which the value not only to pure science is momentous, but which also lie at the foundation of all our material progress and all our industrial success. For without a knowledge of the laws of chemical combination all the marvellous results with which modern industrial chemistry has astonished the world could not have been achieved, whilst the knowledge of the quantitative relations existing between the several forms of energy, and the possibility of expressing their amount in terms of ordinary mechanics, are matters which now constitute the life-breath of every branch of applied science. For example, before Dalton's discovery every manufacturer of oil of vitriol—a substance now made each week in thousands of tons within a few miles of this spot—every manufacturer had his own notions of the quantity of sulphur which he ought to burn in order to make a certain weight of sulphuric acid, but he had no idea that only a given weight of sulphur can unite with a certain quantity of oxygen and of water to form the acid, and that an excess of any one of the component parts was not only useless but harmful. Thus, and in tens of thousands of other instances, Dalton replaced rule of thumb by scientific principle. In like manner the applications of Joule's determination of the mechanical equivalent of heat are even more general; the increase and measurement of the efficiency of our steam-engines and the power of our dynamos are only two of the numerous examples which might be adduced of the practical value of Joule's work.

If the place calls up these thoughts, the time of our meeting also awakens memories of no less interest, in the recollection that we this year celebrate the Jubilee of Her Most Gracious Majesty's accession to the throne. It is right that the members of the British Association for the Advancement of Science should do so with heart and voice, for, although science requires and demands no royal patronage, we thereby express the feeling which must be uppermost in the hearts of all men of science, the feeling of thankfulness that we have lived in an age which has witnessed an advance in our knowledge of Nature, and a consequent improvement in the physical, and let us trust also in the moral and intellectual, well-being of the people hitherto unknown; an age with which the name of Victoria will ever be associated.

To give even a sketch of this progress, to trace even in the merest outline the salient points of the general history of science during the fifty momentous years of Her Majesty's reign, is a task far beyond my limited powers. It must suffice for me to point out to you, to the best of my ability, some few of the steps of that progress as evidenced in the one branch of science with which I am most familiar, and with which I am more closely concerned, the science of chemistry.

In the year 1837 chemistry was a very different science from that existing at the present moment. Priestley, it is true, had discovered oxygen, Lavoisier had placed the phenomena of combustion on their true basis, Davy had decomposed the alkalies, Faraday had liquefied many of the gases, Dalton had enunciated the laws of chemical combination by weight, and Gay-Lussac had pointed out the fact that a simple volumetric relation governs the combination of the gases. But we then possessed no knowledge of chemical dynamics, we were then altogether unable to explain the meaning of the heat given off in the act of chemical combination. The atomic theory was indeed accepted, but we were as ignorant of the mode of action of the atoms and as incapable of explaining their mutual relationship as were the ancient Greek philosophers. Fifty years ago, too, the connexion existing between the laws of life, vegetable and animal, and the phenomena of inorganic chemistry, was ill understood. The idea that the functions of living beings are controlled by the same forces, chemical and physical, which regulate the changes occurring in the inanimate world, was then one held by only a very few of the foremost thinkers of the time. Vital force was a term in everyone's mouth, an expression useful, as Goethe says, to disguise our ignorance, for

> "Wo die Begriffe fehlen,
> Da tritt ein Wort zur rechten Zeit sich ein."

Indeed the pioneer of the chemistry of life, Liebig himself, cannot quite shake himself free from the bonds of orthodox opinion, and he who first placed the phenomena of life on a true basis cannot trust his chemical principles to conduct the affairs of the body, but makes an appeal to vital force to help him out of his difficulties; as when in the body politic an unruly mob requires the presence and action of physical force to restrain it and to bring its members under the saving influence of law and order, so too, according to Liebig's views, in the body corporeal a continual conflict between the chemical forces and the vital power occurs throughout life, in which the latter, when it prevails, insures health and a continuance of life, but of which defeat insures disease or death. The picture presented to the student of to-day is a very different one. We now believe that no such conflict is possible, but that life is governed by chemical and physical forces, even though we cannot in every case explain its phenomena in terms of these forces; that whether these tend to continue or to end existence depends upon their nature and amount, and that disease and death are as much a consequence of the operation of chemical and physical laws as are health and life.

Looking back again to our point of departure fifty years ago, let us for a moment glance at Dalton's labours, and compare his views and those of his contemporaries with the ideas which now prevail. In the first place it is well to remember that the keystone of his atomic theory lies not so much in the idea of the existence and the indivisible nature of the particles of matter—though this idea was so firmly implanted in his mind that, being questioned on one occasion on the subject, he said to his friend the late Mr. Ransome, "Thou knowst it must be so, for no man can split an atom"—as in the assumption that the weights of these particles are different. Thus whilst each of the ultimate particles of oxygen has the same weight as every other particle of oxygen, and each atom of hydrogen, for example, has the same weight as every other particle of hydrogen, the oxygen atom is sixteen times heavier than that of hydrogen, and so on for the atoms of every chemical element, each having its own special weight. It was this discovery of Dalton, together with the further one that the elements combine in the proportions indicated by the relative weights of their atoms or in multiples of these proportions, which at once changed chemistry from a qualitative to a quantitative science, making the old invocation prophetic, "Thou hast ordered all things in measure and number and weight."

The researches of chemists and physicists during the last fifty years have not only strengthened but broadened the foundations of the great Manchester philosopher's discoveries. It is true that his original numbers, obtained by crude and inaccurate methods, have been replaced by more exact figures, but his laws of combination and his atomic explanation of those laws stand as the great bulwarks of our science.

On the present occasion it is interesting to remember that within a stone's-throw of this place is the small room belonging to our Literary and Philosophical Society which served Dalton as his laboratory. Here, with the simplest of all possible apparatus—a few cups, penny ink-bottles, rough balances, and self-made thermometers and barometers—Dalton accomplished his great results. Here he patiently worked, marshalling facts to support his great theory, for as an explanation of his laborious experimental investigations the wise old man says: "Having been in my progress so often misled by taking for granted the results of others, I have determined to write as little as possible but what I can attest by my own experience." Nor ought we when here assembled to forget that the last three of Dalton's experimental essays—one of which, on a new method of measuring water of crystallization, contained more than the germ of a great discovery—were communicated to our Chemical Section in 1842, and that this was the last memorable act of his scientific life. In this last of his contributions to science, as in his first, his method of procedure was that which has been marked out as the most fruitful by almost all the great searchers after Nature's secrets; namely, the assumption of a certain view as a working hypothesis, and the subsequent institution of experiment to bring this hypothesis to a test of reality upon which a legitimate theory is afterwards to be based. "Dalton," as Henry well says, "valued detailed facts mainly, if not solely, as the stepping-stones to comprehensive generalizations."

Next let us ask what light the research of the last fifty years has thrown on the subject of the Daltonian atoms: first, as regards their size; secondly, in respect to their indivisibility and mutual relationships; and thirdly, as regards their motions.

As regards the size and shape of the atoms, Dalton offered no opinion, for he had no experimental grounds on which to form it, believing that they were inconceivably small and altogether beyond the grasp of our senses aided by the most powerful appliances of art. He was in the habit of representing his atoms and their combinations diagrammatically as round disks or spheres made of wood, by means of which he was fond of illustrating his theory. But such mechanical illustrations are not without their danger, for I well remember the answer given by a pupil to a question on the atomic theory: "Atoms are round balls of wood invented by Dr. Dalton." So determinedly indeed did he adhere to his mechanical method of representing the chemical atoms and their combinations that he could not be prevailed upon to adopt the system of chemical formulæ introduced by Berzelius and now universally employed. In a letter addressed to Graham in April 1837, he writes: "Berzelius's symbols are horrifying. A young student in chemistry might as soon learn Hebrew as make himself acquainted with them." And again: "They appear to me equally to perplex the adepts in science, to discourage the learner, as well as to cloud the beauty and simplicity of the atomic theory."

But modern research has accomplished, as regards the size of the atom, at any rate to a certain extent, what Dalton regarded as impossible. Thus in 1865, Loschmidt, of Vienna, by a train of reasoning which I cannot now stop to explain, came to the conclusion that the diameter of an atom of oxygen or nitrogen was 1/10,000,000 of a centimetre. With the highest known magnifying power we can distinguish the 1/40,000 part of a centimetre; if now we imagine a cubic box each of whose sides has the above length, such a box when filled with air will contain from 60 to 100 millions of atoms of oxygen and nitrogen. A few years later William Thomson extended the methods of atomic measurement, and came to the conclusion that the distance between the centres of contiguous molecules is less than 1/5,000,000 and greater than 1/1,000,000,000 of a centimetre; or, to put it in language more suited to the ordinary mind, Thomson asks us to imagine a drop of water magnified up to the size of the earth, and then tells us that the coarseness of the graining of such a mass would be something between a heap of small shot and a heap of cricket balls. Or again, to take Clifford's illustration, you know that our best microscopes magnify from 6000 to 8000 times; a microscope which would magnify that result as much again would show the molecular structure of water. Or again, to put it in another form, if we suppose that the minutest organism we can now see were provided with equally powerful microscopes, these beings would be able to see the atoms.

Next, as to the indivisibility of the atom, involving also the question as to the relationships between the atomic weights and properties of the several elementary bodies.

Taking Dalton's aphorism, "Thou knowst no man can split an atom," as expressing the view of the enunciator of the atomic theory, let us see how far this idea is borne out by subsequent work. In the first place, Thomas Thomson, the first exponent of Dalton's generalization, was torn by conflicting beliefs until he found peace in the hypothesis of Prout, that the atomic weights of all the so-called elements are multiples of a common unit, which doctrine he sought to establish, as Thorpe remarks, by some of the very worst quantitative determinations to be found in chemical literature, though here I may add that they were not so incorrect as Dalton's original numbers.

Coming down to a somewhat later date, Graham, whose life was devoted to finding what the motion of an atom was, freed himself from the bondage of the Daltonian aphorism, and defined the atom not as a thing which cannot be divided, but as one which has not been divided. With him, as with Lucretius, as Angus Smith remarks, the original atom may be far down.

But speculative ideas respecting the constitution of matter have been the scientific relaxation of many minds from olden time to the present. In the mind of the early Greek the action of the atom as one substance taking various forms by unlimited combinations was sufficient to account for all the phenomena of the world. And Dalton himself, though upholding the indivisibility of his ultimate particles, says: "We do not know that any of the bodies denominated elementary are absolutely indecomposable." Again, Boyle, treating of the origin of form and quality, says: "There is one universal matter common to all bodies—an extended divisible and impenetrable substance." Then Graham in another place expresses a similar thought when he writes: "It is conceivable that the various kinds of matter now recognized as different elementary substances may possess one and the same ultimate or atomic molecules existing in different conditions of movement. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies."

What experimental evidence is now before us bearing upon these interesting speculations? In the first place, then, the space of fifty years has completely changed the face of the inquiry. Not only has the number of distinct well-established elementary bodies increased from fifty-three in 1837 to seventy in 1887 (not including the *twenty* or more new elements recently said to have been discovered by Krüss and Nilson in certain rare Scandinavian minerals), but the properties of these elements have been studied, and are now known to us with a degree of precision then undreamt of. So that relationships existing between these bodies which fifty years ago were undiscernible are now clearly manifest, and it is to these relationships that I would for a moment ask your attention. I have already stated that Dalton measured the relative weights of the ultimate particles by assuming hydrogen as the unit, and that Prout believed that on this basis the atomic weights of all the other elements would be found to be multiples of the atomic weight of hydrogen, thus indicating that an intimate constitutional relation exists between hydrogen and all the other elements.

Since the days of Dalton and Prout the truth or otherwise of Prout's law has been keenly contested by the most eminent chemists of all countries. The inquiry is a purely experimental one, and only those who have a special knowledge of the difficulties which surround such inquiries can form an idea of the amount of labour and self-sacrifice borne by such men as Dumas, Stas, and Marignac in carrying out delicate researches on the atomic weights of the elements. What is, then, the result of these most laborious experiments? It is that, whilst the atomic weights of the elements are not exactly either multiples of the unit or of half the unit, many of the numbers expressing most accurately the weight of the atom approximate so closely to a multiple of that of hydrogen, that we are constrained to admit that these approximations cannot be a mere matter of chance, but that some reason must exist for them. What that reason is, and why a close approximation and yet something short of absolute identity exists, is as yet hidden behind the veil; but who is there that doubts that when this Association celebrates its centenary, this veil will have been lifted, and this occult but fundamental question of atomic philosophy shall have been brought into the clear light of day?

But these are by no means all the relationships which modern science has discovered with respect to the atoms of our chemical elements. So long ago as 1829 Döbereiner pointed out that certain groups of elements exist presenting in all their properties strongly marked family characteristics, and this was afterwards extended and insisted upon by Dumas. We find, for example, in the well-known group of chlorine, bromine, and iodine, these resemblances well developed, accompanied moreover by a proportional graduation in their chemical and physical properties. Thus, to take the most important of all their characters, the atomic weight of the middle term is the mean of the atomic weights of the two extremes. But these groups of triads appeared to be unconnected in any way with one another, nor did they seem to bear any relation to the far larger number of the elements not exhibiting these peculiarities.

Things remained in this condition until 1863, when Newlands threw fresh light upon the subject, showing a far-reaching series of relationships. For the first time we thus obtained a glance into the mode in which the elements are connected together, but, like so many new discoveries, this did not meet with the recognition which we now see it deserves. But whilst England thus had the honour of first opening up this new path, it is to Germany and to Russia that we must look for the consummation of the idea. Germany, in the person of Lothar Meyer, keeps, as it is wont to do, strictly within the limits of known facts. Russia, in the person of Mendelejeff, being of a somewhat more imaginative nature, not only seizes the facts which are proved, but ventures upon prophecy. These chemists, amongst whom Carnelley must be named, agree in placing all the elementary bodies in a certain regular sequence, thus bringing to light a periodic recurrence of analogous chemical and physical properties, on account of which the arrangement is termed the periodic system of the elements.

In order to endeavour to render this somewhat complicated matter clear to you, I may perhaps be allowed to employ a simile. Let us, if you please, imagine a series of human families: a French one, represented by Dumas; an English one, by name Newlands; a German one, the family of Lothar Meyer; and lastly, a Russian one, that of Mendelejeff. Let us next imagine the names of these chemists placed in a horizontal line in the order I have mentioned. Then let us write under each the name of his father, and again, in the next lower line, that of his grandfather, followed by that of his great-grandfather, and so on. Let us next write against each of these names the number of years which has elapsed since the birth of the individual. We shall then find that these numbers regularly increase by a definite amount, *i.e.* by the average age of a generation, which will be approximately the same in all the four families. Comparing the ages of the chemists themselves we shall observe certain differences, but these are small in comparison with the period which has elapsed since the birth of any of their ancestors. Now each individual in this series of family trees represents a chemical element; and just as each family is distinguished by certain idiosyncrasies, so each group of the elementary bodies thus arranged shows distinct signs of consanguinity.

But more than this, it not unfrequently happens that the history and peculiarities of some member of a family may have been lost, even if the memory of a more remote and more famous ancestor may be preserved, although it is clear that such an individual must have had an existence. In such a case Francis Galton would not hesitate from the characteristics of the other members to reproduce the physical and even the mental peculiarities of the missing member; and should genealogical research bring to light the true personal appearance and mental qualities of the man, these would be found to coincide with Galton's estimate.

Such predictions and such verifications have been made in the case of no less than three of our chemical elements. Thus, Mendelejeff pointed out that if, in the future, certain lacunæ in his table were to be filled, they must be filled by elements possessing chemical and physical properties which he accurately specified. Since that time these gaps have actually been stopped by the discovery of gallium by Lecoq de Boisbaudron, of scandium by Nilson, and of germanium by Winkler, and their properties, both physical and chemical, as determined by their discoverers, agree absolutely with those predicted by the Russian chemist. Nay, more than this, we not unfrequently have had to deal with chemical foundlings, clements whose parentage is quite unknown to us. A careful examination of the personality of such waifs has enabled us to restore them to the family from which they have been separated by an unkind fate, and to give them that position in chemical society to which they are entitled.

These remarkable results, though they by no means furnish a proof of the supposition already referred to, viz. that the elements are derived from a common source, clearly point in this direction, and lend some degree of colour to the speculations of those whose scientific imagination, wearying of dry facts, revels in picturing to itself an elemental Bathybius, and in applying to the inanimate, laws of evolution similar to those which rule the animate world. Nor is there wanting other evidence regarding this inquiry, for here heat, the great analyzer, is brought into court. The main portion of the evidence consists in the fact that distinct chemical individuals capable of existence at low temperatures are incapable of existence at high ones, but split up into new materials possessing a less complicated structure than the original. And here it may be well to emphasize the distinction which the chemist draws between the atom and the molecule, the latter being a more or less complicated aggregation of atoms, and especially to point out the fundamental difference between the question of separating the atoms in the molecule and that of splitting up the atom itself. The decompositions above referred to are, in fact, not confined to compound bodies, for Victor Meyer has proved in the case of iodine that the molecule at high temperatures is broken to atoms, and J. J. Thomson has added to our knowledge by showing that this breaking up of the molecule may be effected not only by heat vibrations, but likewise by the electrical discharge at a comparatively low temperature.

How far, now, has this process of simplification been carried? Have the atoms of our present elements been made to yield? To this a negative answer must undoubtedly be given, for even the highest of terrestrial temperatures, that of the electric spark, has failed to shake any one of these atoms in two. That this is the case has been shown by the results with which spectrum analysis, that new and fascinating branch of science, has enriched our knowledge, for that spectrum analysis does give us most valuable aid in determining the varying molecular conditions of matter is admitted by all. Let us see how this bears on the question of the decomposition of the elements, and let us suppose for a moment that certain of our present elements, instead of being distinct substances, were made up of common ingredients, and that these compound elements, if I may be allowed to use so incongruous a term, are split up at the temperature of the electric spark into less complicated molecules. Then the spectroscopic examination of such a body must indicate the existence of these common ingredients by the appearance in the spark-spectra of these elements of identical bright lines. Coincidences of this kind have indeed been observed, but on careful examination these have been shown to be due either to the presence of some one of the other elements as an impurity, or to insufficient observational power. This absence of coincident lines admits, however, of two explanations—either that the elements are not decomposed at the temperature of the elec'ric spark, or, what appears to me a much more improbable supposition, each one of the numbers of bright lines exhibited by every element indicates the existence of a separate constituent, no two of this enormous number being identical.

Terrestrial analysis having thus failed to furnish favourable evidence, we are compelled to see if any information is forthcoming from the chemistry of the sun and stars. And here I would remark that it is not my purpose now to dilate on the wonders which this branch of modern science has revealed. It s sufficient to remind you that chemists thus have the means placed at their disposal of ascertaining with certainty the presence of elements well known on this earth in fixed stars so far distant that we are now receiving the light which emanated from them perhaps even thousands of years ago.

Since Bunsen and Kirchhoff's original discovery in 1859, the labours of many men of science of all countries have largely increased our knowledge of the chemical constitution of the sun and stars, and to no one does science owe more in this direction than to Lockyer and Huggins in this country, and to Young in the New England beyond the seas. Lockyer has of late years devoted his attention chiefly to the varying nature of the bright lines seen under different conditions of time and place on the solar surface, and from these observations he has drawn the inference that the matching observed by Kirchhoff between, for instance, the iron lines as seen in our laboratories and those visible in the sun, has fallen to the ground. He further explains this want of uniformity by the fact that at the higher transcendental temperatures of the sun the substance which we know here as iron is resolved into separate components. Other experimentalists, however, while accepting Lockyer's facts as to the variations in the solar spectrum, do not admit his conclusions, and would rather explain the phenomena by the well-known differences which occur in the spectra of all the elements when their molecules are subject to change of temperature or change of position.

Further, arguments in favour of this idea of the evolution of the elements have been adduced from the phenomena presented by the spectra of the fixed stars. It is well known that some of these shine with a white, others with a red, and others again with a blue light; and the spectroscope, especially under the hands of Huggins, has shown that the chemical constitution of these stars is different. The white stars, of which Sirius may be taken as a type, exhibit a much less complicated spectrum than the orange and the red stars; the spectra of the latter remind us more of those of the metalloids and of chemical compounds than of the metals. Hence it has been argued that in the white, presumably the hottest, stars a celestial dissociation of our terrestrial elements may have taken place, whilst in the cooler stars, probably the red, combination even may occur. But even in the white stars we have no *direct* evidence that a decomposition of any terrestrial atom has taken place; indeed we learn that the hydrogen atom, as we know it here, can endure unscathed the inconceivably fierce temperature of stars presumably many times more fervent than our sun, as Sirius and Vega.

Taking all these matters into consideration, we need not be surprised if the earth-bound chemist should, in the absence of celestial evidence which is incontestable, continue, for the present at least, and until fresh evidence is forthcoming, to regard the elements as the unalterable foundation-stones upon which his science is based.

Pursuing another line of inquiry on this subject, Crookes has added a remarkable contribution to the question of the possibility of decomposing the elements. With his well-known experimental prowess, he has discovered a new and beautiful series of phenomena, and has shown that the phosphorescent lights emitted by certain chemical compounds, especially the rare earths, under an electric discharge in a high vacuum exhibit peculiar and characteristic lines. For the purpose of obtaining his material Crookes started from a substance believed by chemists to be homogeneous, such, for example, as the rare earth yttria, and succeeded by a long series of fractional precipitations in obtaining products which yield different phosphorescent spectra, although when tested by the ordinary methods of what we may term high temperature spectroscopy, they appear to be the one substance employed at the starting-point. The other touchstone by which the identity, or otherwise, of these various products might be ascertained, viz. the determination of their atomic weights, has not, as yet, engaged Crookes's attention. In explanation of these singular phenomena, the discoverer suggests two possibilities. First, that the bodies yielding the different phosphorescent spectra are different elementary constituents of the substance which we call yttria Or, if this be objected to because they all yield the same spark-spectrum, he adopts the very reasonable view that the Daltonian atom is probably, as we have seen, a system of chemical complexity; and adds to this the idea that these complex atoms are not all of exactly the same constitution and weight, the differences, however, being so slight that their detection has hitherto eluded our most delicate tests, with the exception of this one of phosphorescence in a vacuum. To these two explanations, Marignac, in a discussion of Crookes's results, adds a third. It having been shown by Crookes himself that the presence of the minutest traces of foreign bodies produce remarkable alterations in the phosphorescent spectra, Marignac suggests that in the course of the thousands of separations which must be made before these differences become manifest, traces of foreign bodies may have been accidently introduced, or, being present in the original material, may have accumulated to a different extent in the various fractions, their presence being indicated by the only test by which they can now be detected. Which of these three explanations is the true one must be left to future experiment to decide.

We must now pass from the statics to the dynamics of chemistry; that is, from the consideration of the atoms at rest to that of the atoms in motion. Here, again, we are indebted to John Dalton for the first step in this direction, for he showed that the particles of a gas are constantly flying about in all directions; that is, that gases diffuse into one another, as an escape of coal gas from a burner, for example, soon makes itself perceptible throughout the room. Dalton, whose mind was constantly engaged in studying the molecular condition of gases, first showed that a light gas cannot rest upon a heavier gas as oil upon water, but that an interpenetration of each gas by the other takes place. It is, however, to Graham's experiments, made rather more than half a century ago, that we are indebted for the discovery of the law regulating these molecular motions of gases, proving that their relative rates of diffusion are inversely proportional to the square roots of their densities, so that oxygen being 16 times heavier than hydrogen, their relative rates of diffusion are 1 and 4.

But whilst Dalton and Graham indicated that the atoms are in a continual state of motion, it is to Joule that we owe the first accurate determination of the rate of that motion. At the Swansea meeting, in 1848, Joule read a paper before Section A on the "Mechanical Equivalent of Heat and on the Constitution of Elastic Fluids." In this paper Joule remarks that whether we conceive the particles to be revolving round one another according to the hypothesis of Davy, or flying about in every direction according to Herapath's view, the pressure of the gas will be in proportion to the *vis viva* of its particles. "Thus it may be shown that the particles of hydrogen at the barometrical pressure of 30 inches at a temperature of 60° must move with a velocity of 6225·54 feet per second in order to produce a pressure of 14·714 lbs. on the square inch," or, to put it in other words, a molecular cannonade or hailstorm of particles, at the above rate—a rate, we must remember, far exceeding that of a cannon ball—is maintained against the bounding surface.

We can, however, go a step further and calculate with Clerk Maxwell the number of times in which this hydrogen molecule, moving at the rate of 70 miles per minute, strikes against others of the vibrating swarm, and we learn that in one second of time it must knock against others no less than 18 thousand million times.

And here we may pause and dwell for a moment on the reflection that in Nature there is no such thing as great or small, and that the structure of the smallest particle, invisible even to our most searching vision, may be as complicated as that of any one of the heavenly bodies which circle round our sun.

But how does this wonderful atomic motion affect our chemistry? Can chemical science or chemical phenomena throw light upon this motion, or can this motion explain any of the known phenomena of our science? I have already said that Lavoisier left untouched the dynamics of combustion. He could not explain why a fixed and unalterable amount of heat is in most cases emitted, but in some cases absorbed, when chemical combination takes place. What Lavoisier left unexplained Joule has made clear. On August 25, 1843, Joule read a short communication, I am glad to remember, before the Chemical Section of our Association, meeting that year at Cork, containing an announcement of a discovery which was to revolutionize modern science. This consisted in the determination of the mechanical equivalent of heat, in proving by accurate experiment that by the expenditure of energy equal to that developed by the weight of 772 pounds falling through 1 foot at Manchester, the temperature of 1 pound of water can be raised 1° F. In other words, every change in the arrangement of the particles is accompanied by a definite evolution or an absorption of heat. In all such cases the molecular energy leaves the potential to assume the kinetic form, or *vice versâ*. Heat is evolved by the clashing of the atoms, and this amount is fixed and definite.

Thus it is to Joule we owe the foundation of chemical dynamics and the basis of thermal chemistry. As the conservation of mass or the principle of the indestructibility of matter forms the basis of chemical statics, so the principle of the conservation of energy[1] constitutes the foundation of chemical dynamics. Change in the form of matter and change in the form of energy are the universal accompaniments of every chemical operation. Here again it is to Joule we owe the proof of the truth of this principle in another direction, viz. that when electrical energy is developed by chemical change a corresponding quantity of chemical energy disappears. Energy, as defined by Maxwell, is the power of doing work, and work is the act of producing a configuration in a system in opposition to a force which resists that change. Chemical action produces such a change of configuration in the molecules. Hence, as Maxwell says, 'A complete knowledge of the mode in which the potential energy of a system varies with the configuration would enable us to predict every possible motion of the system under the action of given external forces, provided we were able to overcome the purely mathematical difficulties of the calculation." The object of thermal chemistry is to measure these changes of energy by thermal methods, and to connect these with chemical changes, to estimate the attractions of the atoms and molecules to which the name of chemical affinity has been applied, and thus to solve the most fundamental problem of chemical science. How far has modern research approached the solution of this most difficult problem? How far can we answer the question, What is the amount of the forces at work in these chemical changes? What laws govern these forces? Well, even in spite of the results with which recent researches, especially the remarkable ones of the Danish philosopher Thomsen have enriched us, we must acknowledge that we are yet scarcely in sight of Maxwell's position of successful prediction. Thermal chemistry, we must acknowledge, is even yet in its infancy; it is, however, an infant of sturdy growth, likely to do good work in the world, and to be a credit to him who is its acknowledged father, as well as to those who have so carefully tended it in its early years.

But recent investigation in another direction bids fair even to eclipse the results which have been obtained by the examination of thermal phenomena. And this lies in the direction of electrical chemistry. Faraday's work relating to conductivity of chemical substances has been already referred to, and this has been since substantiated and extended to pure substances by Kohlrausch. It has been shown, for example, that the resistance of absolutely pure water is almost an infinite quantity. But a small quantity of an acid, such as acetic or butyric acid, greatly increases the conductivity; but more than this, it is possible by determination of the conductivity of a mixture of water with these two acids to arrive at a conclusion as to the partition of the molecules of the water between the acids. Such a partition, however, implies a change of position, and therefore we are furnished with a means of recognizing the motion of the molecules in a liquid, and of determining its amount. Thus it has been found that the hindrance to molecular motion is more affected by the chemical character of the liquid than by physical characters such as viscosity. We have seen that chemical change is always accompanied by molecular motion, and further evidence of the truth of this is gained from the extraordinary chemical inactivity of pure unmixed substances. Thus pure anhydrous hydrochloric acid does not act upon lime, whereas the addition of even a trace of moisture sets up a most active chemical change, and hundreds of other examples of a similar kind might be stated. Bearing in mind that these pure anhydrous compounds do not conduct, we are led to the conclusion that an intimate relation exists between chemical activity and conductivity. And we need not stop here; for a method is indicated indeed by which it will be possible to arrive at a measure of chemical affinity from determination of conductivity. It has indeed been already shown that the rate of change in the saponification of acetic ether is directly proportional to the conductivity of the liquid employed.

Such wide-reaching inquiries into new and fertile fields, in which we seem to come into nearer touch with the molecular state of matter, and within a measurable distance of accurate mathematical expression, leads to confident hope that Lord Rayleigh's pregnant words at Montreal may ere long be realized: "It is from the further study of electrolysis that we may expect to gain improved views as to the nature of chemical reactions, and of the forces concerned in bringing them about; and I cannot help thinking that the next great advance, of which we already have some foreshadowing, will come on this side."

There is, perhaps, no branch of our science in which the doctrine of the Daltonian atom plays a more conspicuous part than in organic chemistry or the chemistry of the carbon compounds, as there is certainly none in which such wonderful progress has been made during the last fifty years. One of the most striking and perplexing discoveries made rather more than half a century ago was that chemical compounds could exist which, whilst possessing an identical chemical composition, that is containing the same percentage quantity of their constituents, are essentially distinct chemical substances exhibiting different properties. Dalton was the first to point out the existence of such substances, and to suggest that the difference was to be ascribed to a different or to a multiple arrangement of the constituent atoms. Faraday soon afterwards proved that this supposition was correct, and the research of Liebig and Wöhler on the identity of composition of the salts of fulminic and cyanic acid gave further confirmation to the conclusion, leading Faraday to remark that "now we are taught to look for bodies composed of the same elements in the same proportion, but differing in their qualities, they may probably multiply upon us." How true this prophecy has become we may gather from the fact that we now know of thousands of cases of this kind, and that we are able not only to explain the reason of their difference by virtue of the varying position of the atoms within the molecule, but even to predict the number of distinct variations in which any given chemical compound can possibly exist. How large this number may become may be understood from the fact that, for example, one chemical compound, a hydrocarbon containing thirteen atoms of carbon combined with twenty-eight atoms of hydrogen, can be shown to be capable of existing in no less than 802 distinct forms.

Experiment in every case in which it has been applied has proved the truth of such a prediction, so that the chemist has no need to apply the cogent argument sometimes said to be used by experimentalists enamoured of pet theories, "When facts do not agree with theory, so much the worse for the facts"! This power of successful prediction constitutes a high-water mark in science, for it indicates that the theory upon which such a power is based is a true one.

But if the Daltonian atom forms the foundation of this theory, it is upon a knowledge of the mode of arrangement of these atoms and on a recognition of their distinctive properties that the superstructure of modern organic chemistry rests. Certainly it does appear almost to verge on the miraculous that chemists should now be able to ascertain with certainty the relative position of atoms in a molecule so minute that millions upon millions, like the angels in the schoolmen's discussion, can stand on a needle's point. And yet this process of orientation is one which is accomplished every day in our laboratories, and one which more than any other has led to results of a startling character. Still, this sword to open the oyster of science would have been wanting to us if we had not taken a step farther than Dalton did, in the recognition of the distinctive nature of the elemental atoms. We now assume on good grounds that the atom of each element possesses distinct capabilities of combination: some a single capability, others a double, others a triple, and others again a fourfold combining capacity. The germs of this theory of valency, one of the most fruitful of modern chemical ideas, were enunciated by Frankland in 1852, but the definite explanation of the linking of atoms, of the tetrad nature of the carbon atoms, their power of combination, and of the difference in structure between the fatty and aromatic series of compounds,

[1] "The total energy of any material system is a quantity which can neither be increased nor diminished by any action between the parts of the system, though it may be transformed into any of the forms of which energy is susceptible."—MAXWELL.

was first pointed out by Kekulé in 1857; though we must not forget that this great principle was foreshadowed so long ago as 1833 from a physical point of view by Faraday in his well-known laws of electrolysis, and that it is to Helmholtz, in his celebrated Faraday Lecture, that we owe the complete elucidation of the subject; for, whilst Faraday has shown that the number of the atoms electrolytically deposited is in the inverse ratio of their valencies, Helmholtz has explained this by the fact that the quantity of electricity with which each atom is associated is directly proportional to its valency.

Amongst the tetrad class of elements, carbon, the distinctive element of organic compounds, finds its place; and the remarkable fact that the number of carbon compounds far exceeds that of all the other elements put together receives its explanation. For these carbon atoms not only possess four means of grasping other atoms, but these four-handed carbon atoms have a strong partiality for each other's company, and readily attach themselves hand in hand to form open chains or closed rings, to which the atoms of other elements join to grasp the unoccupied carbon hand, and thus to yield a dancing company in which all hands are locked together. Such a group, each individual occupying a given position with reference to the others, constitutes the organic molecule. When, in such a company, the individual members change hands, a new combination is formed. And as in such an assembly the eye can follow the changing positions of the individual members, so the chemist can recognize in his molecule the position of the several atoms, and explain by this the fact that each arrangement constitutes a new chemical compound possessing different properties, and account in this way for the decompositions which each differently constituted molecule is found to undergo.

Chemists are, however, not content with representing the arrangement of the atoms in one plane, as on a sheet of paper, but attempt to express the position of the atoms in space. In this way it is possible to explain certain observed differences in isomeric bodies, which otherwise baffled our efforts. To Van t'Hoff, in the first instance, and more recently to Wislicenus, chemistry is indebted for work in this direction, which throws light on hitherto obscure phenomena, and points the way to still further and more important advances.

It is this knowledge of the mode in which the atoms in the molecule are arranged, this power of determining the nature of this arrangement, which has given to organic chemistry the impetus which has overcome so many experimental obstacles, and given rise to such unlooked-for results. Organic chemistry has now become synthetic. In 1837 we were able to build up but very few and very simple organic compounds from their elements; indeed the views of chemists were much divided as to the possibility of such a thing. Both Gmelin and Berzelius argued that organic compounds, unlike inorganic bodies, cannot be built up from their elements. Organic compounds were generally believed to be special products of the so-called vital force, and it was only intuitive minds like those of Liebig and Wöhler who foresaw what was coming, and wrote in 1837 strongly against this view, asserting that the artificial production in our laboratories of all organic substances, so far as they do not constitute a living organism, is not only probable but certain. Indeed, they went a step farther, and predicted that sugar, morphia, salicine, will all thus be prepared; a prophecy which, I need scarcely remind you, has been after fifty years fulfilled, for at the present time we can prepare an artificial sweetening principle, an artificial alkaloid, and salicine.

In spite of these predictions, and in spite of Wöhler's memorable discovery in 1828 of the artificial production of urea, which did in reality break down for ever the barrier of essential chemical difference between the products of the inanimate and the animate world, still, even up to a much later date, contrary opinions were held, and the synthesis of urea was looked upon as the exception which proves the rule. So it came to pass that for many years the artificial production of any of the more complicated organic substances was believed to be impossible. Now the belief in a special vital force has disappeared like the *ignis fatuus*, and no longer lures us in the wrong direction. We know now that the same laws regulate the formation of chemical compounds in both animate and inanimate nature, and the chemist only asks for a knowledge of the constitution of any definite chemical compound found in the organic world in order to be able to promise to prepare it artificially.

But the progress of synthetic organic chemistry, which has of late been so rapid, was made in the early days of the half-century only by feeble steps and slow. Seventeen long years elapsed between Wöhler's discovery and the next real synthesis. This was accomplished by Kolbe, who in 1845 prepared acetic acid from its elements. But then a splendid harvest of results gathered in by chemists of all nations quickly followed, a harvest so rich and so varied that we are apt to be overpowered by its wealth, and amidst so much that is alluring and striking we may well find it difficult to choose the most appropriate examples for illustrating the power and the extent of modern chemical synthesis.

Next, as a contrast to our picture, let us for a moment glance back again to the state of things fifty years ago, and then notice the chief steps by which we have arrived at our present position. In 1837 organic chemistry possessed no scientific basis, and therefore no classification of a character worthy of the name. Writing to Berzelius in that year, Wöhler describes the condition of organic chemistry as one enough to drive a man mad. "It seems to me," says he, "like the tropical forest primæval, full of the strangest growths, an endless and pathless thicket in which a man may well dread to wander." Still clearances had already been made in this wilderness of facts. Berzelius in 1832 welcomed the results of Liebig and Wöhler's research on benzoic acid as the dawn of a new era; and such it really was, inasmuch as it introduced a novel and fruitful idea—namely, the possibility of a group of atoms acting like an element by pointing out the existence of organic radicals. This theory was strengthened and confirmed by Bunsen's classical researches on the cacodyl compounds, in which he showed that a common group of elements which acts exactly as a metal can exist in the free state, and this was followed soon afterwards by isolation of the so-called alcohol radicals by Frankland and Kolbe. It is, however, to Schorlemmer that we owe our knowledge of the true constitution of these bodies, a matter which proved to be of vital importance for the further development of the science.

Turning our glance in another direction we find that Dumas in 1834 by this law of substitution threw light upon a whole series of singular and unexplained phenomena by showing that an exchange can take place between the constituent atoms in a molecule. Laurent indeed went farther, and assumed that a chlorine atom, for example, took up the position vacated by an atom of hydrogen and played the part of its displaced rival, so that the chemical and physical properties of the substitution-product were thought to remain substantially the same as those of the original body. A singular story is connected with this discovery. At a *soirée* in the Tuileries in the time of Charles X. the guests were almost suffocated by acrid vapours which were evidently emitted by the burning wax candles, and the great chemist Dumas was called in to examine into the cause of the annoyance. He found that the wax of which the candles were made had been bleached by chlorine, that a replacement of some of the hydrogen atoms of the wax by chlorine had occurred, and that the suffocating vapours consisted of hydrochloric acid given off during the combustion. The wax was as white and as odourless as before, and the fact of the substitution of chlorine for hydrogen could only be recognized when the candles were destroyed by burning. This incident induced Dumas to investigate more closely this class of phenomena, and the results of this investigation are embodied in his law of substitution. So far indeed did the interest of the French school of chemists lead them that some assumed that not only the hydrogen but also the carbon of organic bodies could be replaced by substitution. Against this idea Liebig protested, and in a satirical vein he informs the chemical public, writing from Paris under the *nom de plume* of S. Windler, that he has succeeded in substituting not only the hydrogen but the oxygen and carbon in cotton cloth by chlorine, and he adds that the London shops are now selling nightcaps and other articles of apparel made entirely of chlorine, goods which meet with much favour, especially for hospital use!

But the debt which chemistry, both inorganic and organic, thus owes to Dumas' law of substitution is serious enough, for it proved to be the germ of Williamson's classical researches on etherification, as well as of those of Wurtz and Hofmann on the compound ammonias, investigations which lie at the base of the structure of modern chemistry. Its influence has been, however, still more far-reaching, inasmuch as upon it depends in great measure the astounding progress made in the wide field of organic synthesis.

It may here be permitted to me to sketch in rough outline the principles upon which all organic syntheses have been effected. We have already seen that as soon as the chemical structure of a body has been ascertained its artificial preparation may be certainly anticipated, so that the first step to be taken is the study of the structure of the naturally occurring substance which it is desired to prepare artificially by resolving it into simpler constituents, the constitution of which is already known. In this way, for example, Hofmann discovered that the alkaloid coniine, the poisonous principle of hemlock, may be decomposed into a simpler substance well known to chemists under the name of pyridine. This fact having been established by Hofmann, and the grouping of the atoms approximately determined, it was then necessary to reverse the process, and, starting with pyridine, to build up a compound of the required constitution and properties, a result recently achieved by Ladenburg in a series of brilliant researches. The well-known synthesis of the colouring matter of madder by Graebe and Liebermann, preceded by the important researches of Schunck, and that of indigo by Baeyer, are other striking examples in which this method has been successfully followed.

Not only has this intimate acquaintance with the changes which occur within the molecules of organic compounds been utilized, as we have seen, in the synthesis of naturally occurring substances, but it has also led to the discovery of many new ones. Of these perhaps the most remarkable instance is the production of an artificial sweetening agent termed saccharin, 250 times sweeter than sugar, prepared by a complicated series of reactions from coal-tar. Nor must we imagine that these discoveries are of scientific interest only, for they have given rise to the industry of the coal-tar colours, the value of which is measured by millions sterling annually, an industry which Englishmen may be proud to remember was founded by our countryman Perkin.

Another interesting application of synthetic chemistry to the needs of every-day life is the discovery of a series of valuable febrifuges, amongst which I may mention antipyrin as the most useful. An important aspect in connexion with the study of these bodies is the physiological value which has been found to attach to the introduction of certain organic radicals, so that an indication is given of the possibility of preparing a compound which will possess certain desired physiological properties, or even to foretell the kind of action which such bodies may exert on the animal economy.

But it is not only the physiological properties of chemical compounds which stand in intimate relation with their constitution, for we find that this is the case with all their physical properties. It is true that at the beginning of our period any such relation was almost unsuspected, whilst at the present time the number of instances in which this connexion has been ascertained is almost infinite. Amongst these perhaps the most striking is the relationship which has been pointed out between the optical properties and chemical composition. This was in the first place recognized by Pasteur in his classical researches on racemic and tartaric acids in 1848; but the first to indicate a quantitative relationship and a connexion between chemical structure and optical properties was Gladstone in 1863. Great instrumental precision has been brought to bear on this question, and consequently most important practical applications have resulted. I need only refer to the well-known accurate methods now in every-day use for the determination of sugar by the polariscope, equally valuable to the physician and to the manufacturer.

But now the question may well be put, is any limit set to this synthetic power of the chemist? Although the danger of dogmatizing as to the progress of science has already been shown in too many instances, yet one cannot help feeling that the barrier which exists between the organized and unorganized worlds is one which the chemist at present sees no chance of breaking down.

It is true that there are those who profess to foresee that the day will arrive when the chemist, by a succession of constructive efforts, may pass beyond albumen, and gather the elements of lifeless matter into a living structure. Whatever may be said regarding this from other standpoints, the chemist can only say that at present no such problem lies within his province. Protoplasm, with which the simplest manifestations of life are associated, is not a compound, but a structure built up of compounds. The chemist may successfully synthetize any of its component molecules, but he has no more reason to look forward to the synthetic production of the structure than to imagine that the synthesis of gallic acid leads to the artificial production of gall-nuts.

Although there is thus no prospect of our effecting a synthesis of organized material, yet the progress made in our knowledge of the chemistry of life during the last fifty years has been very great, and so much so indeed that the sciences of physiological and of pathological chemistry may be said to have entirely arisen within this period.

In the introductory portion of this address I have already referred to the relations supposed to exist fifty years ago between vital phenomena and those of the inorganic world. Let me now briefly trace a few of the more important steps which have marked the progress of this branch of science during this period. Certainly no portion of our science is of greater interest, nor, I may add, of greater complexity, than that which, bearing on the vital functions both of plants and of animals, endeavours to unravel the tangled skein of the chemistry of life, and to explain the principles according to which our bodies live, and move, and have their being. If, therefore, in the less complicated problems with which other portions of our science have to deal, we find ourselves, as we have seen, often far from possessing satisfactory solutions, we cannot be surprised to learn that with regard to the chemistry of the living body—whether vegetable or animal—in health or disease we are still farther from a complete knowledge of phenomena, even those of fundamental importance.

It is of interest here to recall the fact that nearly fifty years ago Liebig presented to the Chemical Section of this Association a communication in which, for the first time, an attempt was made to explain the phenomena of life on chemical and physical lines, for in this paper he admits the applicability of the great principle of the conservation of energy to the functions of animals, pointing out that the animal cannot generate more heat than is produced by the combustion of the carbon and hydrogen of his food.

"The source of animal heat," says Liebig, "has previously been ascribed to nervous action or to the contraction of the muscles, or even to the mechanical motions of the body, as if these motions could exist without an expenditure of force [equal to that] consumed in producing them." Again he compares the living body to a laboratory furnace in which a complicated series of changes occur in the fuel, but in which the end-products are carbonic acid and water, the amount of heat evolved being dependent, not upon the intermediate, but upon the final products. Liebig asked himself the question, Does every kind of food go to the production of heat; or can we distinguish, on the one hand, between the kind of food which goes to create warmth, and, on the other, that by the oxidation of which the motions and mechanical energy of the body are kept up? He thought that he was able to do this, and he divided food into two categories. The starchy or carbohydrate food is that, said he, which by its combustion provides the warmth necessary for the existence and life of the body. The albuminous or nitrogenous constituents of our food, the flesh meat, the gluten, the casein out of which our muscles are built up, are not available for the purposes of creating warmth, but it is by the waste of those muscles that the mechanical energy, the activity, the motions of the animal are supplied. We see, said Liebig, that the Esquimaux feeds on fat and tallow, and this burning in his body keeps out the cold. The Gaucho, riding on the pampas, lives entirely on dried meat, and the rowing man and pugilist, trained on beefsteaks and porter, require little food to keep up the temperature of their bodies, but much to enable them to meet the demand for fresh muscular tissue, and for this purpose they need to live on a strongly nitrogenous diet.

Thus far Liebig. Now let us turn to the present state of our knowledge. The question of the source of muscular power is one of the greatest interest, for, as Frankland observes, it is the corner-stone of the physiological edifice and the key to the nutrition of animals.

Let us examine by the light of modern science the truth of Liebig's view—even now not uncommonly held—as to the functions of the two kinds of food, and as to the cause of muscular exercise being the oxidation of the muscular tissue. Soon after the promulgation of these views, J. R. Mayer, whose name as the first expositor of the idea of the conservation of energy is so well known, warmly attacked them, throwing out the hypothesis that all muscular action is due to the combustion of food, and not to the destruction of muscle, proving his case by showing that if the muscles of the heart be destroyed in doing mechanical work the heart would be burnt up in eight days! What does modern research say to this question? Can it be brought to the crucial test of experiment? It can; but how? Well, in the first place we can ascertain the work done by a man or any other animal; we can measure this work in terms of our mechanical standard, in kilogramme-metres or foot-pounds. We can next determine what is the destruction of nitrogenous tissue at rest and under exercise by the amount of nitrogenous material thrown off by the body. And here we must remember that these tissues are never completely burnt, so that free nitrogen is never eliminated. If now we know the heat-value of the burnt muscle, it is easy to convert this into its mechanical equivalent, and thus measure the energy generated. What is the result? Is the weight of muscle destroyed by ascending the Faulhorn or by working on the treadmill sufficient to produce on combustion heat enough when transformed into mechanical exercise to lift the body up to the summit of the Faulhorn or to do the work on the treadmill? Careful experiment has shown that this is so far from being the case that the actual energy developed is twice as great as that which could possibly be produced by the oxidation of the nitrogenous constituents eliminated from the body during twenty-four hours. That is to say, taking the amount of nitrogenous substance cast off from the body, not only whilst the work was being done but during twenty-four hours, the mechanical effect capable of being produced by the muscular tissue from which this cast-off material is derived would only raise the body half-way up the Faulhorn, or enable the prisoner to work half his time on the treadmill.

Hence it is clear that Liebig's proposition is not true. The nitrogenous constituents of the food do doubtless go to repair the waste of muscle, which, like every other portion of the body, needs renewal, whilst the function of the non-nitrogenous food is not only to supply the animal heat, but also to furnish, by its oxidation, the muscular energy of the body.

We thus come to the conclusion that it is the potential energy of the food which furnishes the actual energy of the body, expressed in terms either of heat or of mechanical work.

But there is one other factor which comes into play in this question of mechanical energy, and must be taken into account; and this factor we are as yet unable to estimate in our usual terms. It concerns the action of the mind upon the body, and, although incapable of exact expression, exerts none the less an important influence on the physics and chemistry of the body, so that a connexion undoubtedly exists between intellectual activity or mental work and bodily nutrition. In proof that there is a marked difference between voluntary and involuntary work, we need only compare the mechanical action of the heart, which never causes fatigue, with that of the voluntary muscles, which become fatigued by continued exertion. So, too, we know well that an amount of drill which is fatiguing to the recruit is not felt by the old soldier, who goes through the evolutions automatically. What is the expenditure of mechanical energy which accompanies mental effort, is a question which science is probably far removed from answering. But that the body experiences exhaustion as the result of mental activity is a well-recognized fact. Indeed, whilst the second law of thermodynamics teaches that in none of the mechanical contrivances for the conversion of heat into actual energy can such a conversion be complete, it is perhaps possible, as Helmholtz has suggested, that such a complete conversion may take place in the subtle mechanism of the animal organism.

The phenomena of vegetation, no less than those of the animal world, have, however, during the last fifty years been placed by the chemist on an entirely new basis. Although before the publication of Liebig's celebrated report on chemistry and its application to agriculture, presented to the British Association in 1840, much had been done, many fundamental facts had been established, still Liebig's report marks an era in the progress of this branch of our science. He not only gathered up in a masterly fashion the results of previous workers, but put forward his own original views with a boldness and frequently with a sagacity which gave a vast stimulus and interest to the questions at issue. As a proof of this I may remind you of the attack which he made on, and the complete victory which he gained over, the humus theory. Although Saussure and others had already done much to destroy the basis of this theory, yet the fact remained that vegetable physiologists up to 1840 continued to hold to the opinion that humus, or decayed vegetable matter, was the only source of the carbon of vegetation. Liebig, giving due consideration to the labours of Saussure, came to the conclusion that it was absolutely impossible that the carbon deposited as vegetable tissue over a given area, as for instance over an area of forest land, could be derived from humus, which is itself the result of the decay of vegetable matter. He asserted that the whole of the carbon of vegetation is obtained from the atmospheric carbonic acid, which, though only present in the small relative proportion of 4 parts in 10,000 of air, is contained in such absolutely large quantity that if all the vegetation on the earth's surface were burnt, the proportion of carbonic acid which would thus be thrown into the air would not be sufficient to double the present amount.

That this conclusion of Liebig's is correct needed experimental proof, but such proof could only be given by long-continued and laborious experiment, and this serves to show that chemical research is not now confined to laboratory experiments lasting perhaps a few minutes, but that it has invaded the domain of agriculture as well as of physiology, and reckons the periods of her observations in the field not by minutes, but by years. It is to our English agricultural chemists Lawes and Gilbert that we owe the complete experimental proof required. And it is true that this experiment was a long and tedious one, for it has taken forty-four years to give the definite reply. At Rothamsted a plot was set apart for the growth of wheat. For forty-four successive years that field has grown wheat without addition of any carbonized manure; so that the only possible source from which the plant could obtain the carbon for its growth is the atmospheric carbonic acid. Now, the quantity of carbon which on an average was removed in the form of wheat and straw from a plot manured only with mineral matter was 1000 pounds, whilst on another plot, for which a nitrogenous manure was employed, 1500 pounds more carbon was annually removed; or 2500 pounds of carbon are removed by this crop annually without the addition of any carbonaceous manure. So that Liebig's prevision has received a complete experimental verification.

May I without wearying you with experimental details refer for a moment to Liebig's views as to the assimilation of nitrogen by plants—a much more complicated and difficult question than the one we have just considered—and compare these with the most modern results of agricultural chemistry? We find that in this case his views have not been substantiated. He imagined that the whole of the nitrogen required by the plant was derived from atmospheric ammonia; whereas Lawes and Gilbert have shown by experiments of a similar nature to those just described, and extending over a nearly equal length of time, that this source is wholly insufficient to account for the nitrogen removed in the crop, and have come to the conclusion that the nitrogen must have been obtained either from a store of nitrogenous material in the soil or by absorption of free nitrogen from the air. These two apparently contradictory alternatives may perhaps be reconciled by the recent observations of Warington and of Berthelot, which have thrown light upon the changes which the so-called nitrogenous capital of the soil undergoes, as well as upon its chemical nature, for the latter has shown that under certain conditions the soil has the power of absorbing the nitrogen of the air, forming compounds which can subsequently be assimilated by the plant.

Touching us as human beings even still more closely than the foregoing, is the influence which chemistry has exerted on the science of pathology, and in no direction has greater progress been made than in the study of micro-organisms in relation to health and disease. In the complicated chemical changes to which we give the names of fermentation and putrefaction, the views of Liebig, according to which these phenomena are of a purely chemical character, have given way under the searching investigations of Pasteur, who established the fundamental principle that these processes are inseparably connected with the life of certain low forms of organisms. Thus was founded the science of bacteriology, which in Lister's hands has yielded such splendid results in the treatment of surgical cases; and in those of Klebs, Koch, William Roberts, and others, has been the means of detecting the cause of many diseases both in man and animals; the latest and not the least important of which is the remarkable series of successful researches by Pasteur into the nature and mode of cure of that most dreadful of maladies, hydrophobia. And here I may be allowed to refer with satisfaction to the results of the labours on this subject of a Committee the formation of which I had the honour of moving for in the House of Commons. These results confirm in every respect Pasteur's assertions, and prove beyond a doubt that the adoption of his method has prevented the occurrence of hydrophobia in a large proportion of persons bitten by rabid animals, who, if they had not been subjected to this treatment would have died of that disease. The value of his discovery is, however, greater than can be estimated by its present utility, for it shows that it may be possible to avert other diseases besides hydrophobia by the adoption of a somewhat similar method of investigation and of treatment. This, though the last, is certainly not the least of the debts which humanity owes to the great French experimentalist. Here it might seem as if we had outstepped the boundaries of chemistry, and have to do with phenomena purely vital. But recent research indicates that this is not the case, and points to the conclusion that the microscopist must again give way to the chemist, and that it is by chemical rather than by biological investigation that the causes of diseases will be discovered, and the power of removing them obtained. For we learn that the symptoms of infective diseases are no more due to the microbes which constitute the infection than alcoholic intoxication is produced by the yeast-cell, but that these symptoms are due to the presence of definite chemical compounds, the result of the life of these microscopic organisms. So it is to the action of these poisonous substances formed during the life of the organism, rather than to that of the organism itself, that the special characteristics of the disease are to be traced; for it has been shown that the disease can be communicated by such poisons in entire absence of living organisms.

If I have thus far dwelt on the progress made in certain branches of pure science it is not because I undervalue the other methods by which the advancement of science is accomplished, viz. that of the application and of the diffusion of a knowledge of Nature, but rather because the British Association has always held, and wisely held, that original investigation lies at the root of all application, so that to foster its growth and encourage its development has for more than fifty years been our chief aim and wish.

Had time permitted I should have wished to have illustrated this dependence of industrial success upon original investigation, and to have pointed out the prodigious strides which chemical industry in this country has made during the fifty years of Her Majesty's reign. As it is I must be content to remind you how much our modern life, both in its artistic and useful aspects, owes to chemistry, and, therefore, how essential a knowledge of the principles of the science is to all who have the industrial progress of the country at heart.

This leads me to refer to what has been accomplished in this country of ours towards the diffusion of scientific knowledge amongst the people during the Victorian era. It is true that the English people do not possess, as yet, that appreciation of the value of science so characteristic of some other nations. Up to very recent years our educational system, handed down to us from the Middle Ages, has systematically ignored science, and we are only just beginning, thanks in a great degree to the prevision of the late Prince Consort, to give it a place, and that but an unimportant one, in our primary and secondary schools or in our Universities. The country is, however, now awakening to the necessity of placing its house in order in this respect, and is beginning to see that if she is to maintain her commercial and industrial supremacy the education of her people from top to bottom must be carried out on new lines. The question as to how this can be most safely and surely accomplished is one of transcendent national importance, and the statesman who solves this educational problem will earn the gratitude of generations yet to come.

In conclusion, may I be allowed to welcome the unprecedentedly large number of foreign men of science who have on this occasion honoured the British Association by their presence, and to express the hope that this meeting may be the commencement of an international scientific organization, the only means nowadays existing, to use the words of one of the most distinguished of our guests, of establishing that fraternity among nations from which politics appear to remove us further and further by absorbing human powers and human work, and directing them to purposes of destruction. It would indeed be well if Great Britain, which has hitherto taken the lead in so many things that are great and good, should now direct her attention to the furthering of international organizations of a scientific nature. A more appropriate occasion than the present meeting could perhaps hardly be found for the inauguration of such a movement.

But whether this hope be realized or not, we all unite in that one great object, the search after truth for its own sake, and we all, therefore, may join in re-echoing the words of Lessing "The worth of man lies not in the truth which he possesses believes that he possesses, but in the honest endeavour whic puts forth to secure that truth; for not by the possessio truth, but by the search after it are the faculties of enlarged, and in this alone consists his ever-growing perfect Possession fosters content, indolence, and pride. If God sh hold in His right hand all truth, and in His left hand the e active desire to seek truth, though with the condition of petual error, I would humbly ask for the contents of the hand, saying, 'Father, give me this; pure truth is only Thee.'"

SECTION A.

MATHEMATICAL AND PHYSICAL SCIENCE.

OPENING ADDRESS BY SIR ROBERT S. BALL, LL.D., F.R PRESIDENT OF THE SECTION.

A Dynamical Parable.

THE subject I have chosen for my address to you to-day been to me a favourite topic of meditation for many years. is that part of the science of theoretical mechanics which is usu known as the "Theory of Screws."

A good deal has been already written on this theory, b may say with some confidence that the aspect in which I s invite you now to look at it is a novel one. I propose to an account of the proceedings of a committee appointed to vestigate and experiment upon certain dynamical phenom It may appear to you that the experiments I shall describe l not as yet been made, that even the committee itself has as yet been called together. I have accordingly ventured to this address "A Dynamical Parable."

There was once a rigid body which lay peacefully at rest. committee of natural philosophers was appointed to make experimental and rational inquiry into the dynamics of body. The committee received special instructions. They to find out why the body remained at rest, notwithstanding certain forces were in action. They were to apply impul forces and observe how the body would begin to move. were also to investigate the small oscillations. These b settled, they were then to—— But here the chairman in posed; he considered that for the present, at least, there sufficient work in prospect. He pointed out how the quest already proposed just completed a natural group. "Let it su for us," he said, "to experiment upon the dynamics of this b so long as it remains in or near to the position it now occup We may leave to some more ambitious committee the of following the body in all conceivable gyrations through universe."

The committee was judiciously chosen. Mr. Anharm undertook the geometry. He was found to be of the ut value in the more delicate parts of the work, though his collea thought him rather prosy at times. He was much aided by two friends, Mr. One-to-One, who had charge of the ho graphic department, and Mr. Helix, whose labours will be to be of much importance. As a most respectable if rather fashioned member, Mr. Cartesian was added to the commit but his antiquated tactics were quite out-manœuvred by thos Helix and One-to-One. I need only mention two more na Mr. Commonsense was, of course, present as an *ex-o* member, and valuable service was even rendered by Querulous, who objected at first to serve on the committee at He said that the inquiry was all nonsense, because everybody k as much as they wished to know about the dynamics of a r body. The subject was as old as the hills, and had all b settled long ago. He was persuaded, however, to look occasionally. It will appear that a remarkable result of labours of the committee was the conversion of Mr. Queru himself.

The committee assembled in the presence of the rigid bod commence their memorable labours. There was the bod rest, a huge amorphous mass, with no regularity in its shape- uniformity in its texture. But what chiefly alarmed the c mittee was the bewildering nature of the constraints by w the movements of the body were hampered. They had l accustomed to nice mechanical problems, in which a sm body lay on a smooth table, or a wheel rotated on an axle, body rotated around a point. In all these cases the constra were of a simple character, and the possible movements of the body were obvious. But the constraints in the present case were of puzzling complexity. There were cords and links, moving axes, surfaces with which the body lay in contact, and many other geometrical constraints. Experience of ordinary problems in mechanics would be of little avail. In fact, the chairman truly appreciated the situation when he said that the *constraints were of a perfectly general type*.

In the dismay with which this announcement was received Mr. Commonsense advanced to the body and tried whether it could move at all. Yes, it was obvious that in some ways the body could be moved. Then said Commonsense, "Ought we not first to study carefully the nature of the freedom which the body possesses? Ought we not to make an inventory of every distinct movement of which the body is capable? Until this has been obtained I do not see how we can make any progress in the dynamical part of our business."

Mr. Querulous ridiculed this proposal. "How could you," he said, "make any geometrical theory of the mobility of a body without knowing all about the constraints? And yet you are attempting to do so with perfectly general constraints of which you know nothing. It must all be waste of time, for though I have read many books on mechanics, I never saw anything like it."

Here the gentle voice of Mr. Anharmonic was heard. "Let us try, let us simply experiment on the mobility of the body, and let us faithfully record what we find." In justification of this advice Mr. Anharmonic made a remark which was new to most members of the committee; he asserted that *though the constraints may be of endless variety and complexity there can be only a very limited variety in the types of possible mobility.*

It was therefore resolved to make a series of experiments with the simple object of seeing how the body could be moved. Mr. Cartesian, having a reputation for such work, was requested to undertake the inquiry and to report to the committee. Cartesian commenced operations in accordance with the well-known traditions of his craft. He erected a cumbrous apparatus which he called his three rectangular axes. He then attempted to push the body parallel to one of these axes, but it would not stir. He tried to move the body parallel to each of the other axes, but was again unsuccessful. He then attached the body to one of the axes and tried to effect a rotation around that axis. Again he failed, for the constraints were of too elaborate a type to accommodate themselves to Mr. Cartesian's crude notions.

We shall subsequently find that the movements of the body are necessarily of an exquisitely simple type, yet such was the clumsiness and the artificial character of Mr. Cartesian's machinery that he failed to perceive the simplicity. To him it appeared that the body could only move in a highly complex manner; he saw that it could accept a composite movement consisting of rotations about two or three of his axes and simultaneous translations also parallel to two or three axes. Cartesian was a very skilful calculator, and by a series of experiments even with his unsympathetic apparatus he obtained some knowledge of the subject, sufficient for purposes in which a vivid comprehension of the whole was not required. The inadequacy of Cartesian's geometry was painfully evident when he reported to the committee on the mobility of the rigid body. "I find," he said, "that the body can neither move parallel to x, nor to y, nor to z; neither can I make it rotate around x, nor y, nor z; but I could push it an inch parallel to x, provided that at the same time I pushed it a foot parallel to y and a yard backwards parallel to z, and that it was also turned a degree around x, half a degree the other way around y, and twenty-three minutes and nineteen seconds around z."

"Is that all?" asks the chairman. "Oh no," replied Mr. Cartesian, "there are other proportions in which the ingredients may be combined so as to produce a possible movement," and he was proceeding to state them when Mr. Commonsense interposed. "Stop! stop!" said he, "I can make nothing of all these figures. This jargon about x, y, and z may suffice for your calculations, but it fails to convey to my mind any clear or concise notion of the movements which the body is free to make."

Many of the committee sympathized with this view of Commonsense, and they came to the conclusion that there was nothing to be extracted from poor old Cartesian and his axes. They felt that there must be some better method, and their hopes of discovering it were raised when they saw Mr. Helix volunteer his services and advance to the rigid body. Helix brought with him no cumbrous rectangular axes, but commenced to try the mobility of the body in the simplest manner. He found it lying at rest in a position we may call A. Perceiving that it was in some ways mobile, he gave it a slight displacement to a neighbouring position, B. Contrast the procedure of Cartesian with the procedure of Helix. Cartesian tried to force the body to move along certain routes which he had arbitrarily chosen, but which the body had not chosen; in fact the body would not take any one of his routes separately, though it would take all of them together in the most embarrassing manner. But Helix had no preconceived scheme as to the nature of the movements to be expected. He simply found the body in a certain position, A, and then he coaxed the body to move, not in this particular way or in that particular way, but any way the body liked to any new position, B.

Let the constraints be what they may—let the position B lie anywhere in the close neighbourhood of A—Helix found that he could move the body from A to B by an extremely simple operation. With the aid of a skilful mechanic he prepared a screw with a suitable pitch, and adjusted this screw in a definite position. The rigid body was then attached by rigid bonds to a nut on this screw, and it was found that the movement of the body from A to B could be effected by simply turning the nut on the screw. A perfectly definite fact about the mobility of the body has thus been ascertained. It is able to twist to and fro on a certain screw.

Mr. Querulous could not see that there was any simplicity or geometrical clearness in the notion of a screwing movement; in fact he thought it was the reverse of simple. Did not the screwing movement mean a translation parallel to an axis *and* a rotation around that axis? Was it not better to think of the rotation and the translation separately than to jumble together two things so totally distinct into a composite notion?

But Querulous was instantly answered by One-to-One. "Lamentable, indeed," said he, "would be a divorce between the rotation and the translation. Together they form the unit of rigid movement. Nature herself has wedded them, and the fruits of their happy union are both abundant and beautiful."

The success of Helix encouraged him to proceed with the experiments, and speedily he found a second screw about which the body could also twist. He was about to continue when he was interrupted by Mr. Anharmonic, who said, "Tarry a moment, for geometry declares that a body free to twist about two screws is free to twist about a myriad of screws. These form the generators of a graceful ruled surface known as the cylindroid. There may be infinite variety in the conceivable constraints, but there can be no corresponding variety in the character of this surface. Cylindroids differ in size, they have no difference in shape. Let us then make a cylindroid of the right size, and so place it that two of its screws coincide with those you have discovered; then I promise you that the body can be twisted about every screw on the surface. In other words, if a body has two degrees of freedom the cylindroid is the natural and the perfect general method for giving an exact specification of its mobility."

A single step remained to complete the examination of the freedom of the body. Mr. Helix continued his experiments, and presently detected a third screw, about which the body can also twist in addition to those on the cylindroid. A flood of geometrical light then burst forth and illuminated the whole theory. It appeared that the body was free to twist about ranks upon ranks of screws all beautifully arranged by their pitches on a system of hyperboloids. After a brief conference with Anharmonic and One-to-One, Helix announced that sufficient experiments of this kind had now been made. By the single screw, the cylindroid, and the family of hyperboloids, every conceivable information about the mobility of the rigid body can be adequately conveyed. Let the body have any constraints, however elaborate, yet the definite geometrical conceptions just stated will be sufficient.

With perfect lucidity Mr. Helix expounded the matter to the committee. He exhibited to them an elegant fabric of screws, each with its appropriate pitch, and then he summarized his labours by saying, "About every one of these screws you can displace the body by twisting, and what is of no less importance it will not admit of any movement which is not such a twist." The committee expressed their satisfaction with this information. It was both clear and complete. Indeed, the chairman remarked with considerable force that *a more thorough method of specifying the freedom of the body was inconceivable.*

The discovery of the mobility of the body completed the first stage of the labours of the committee, and they were ready to commence the serious dynamical work. Force was now to be used, with the view of experimenting on the behaviour of the body under its influence. Elated by their previous success the committee declared that they would not rest satisfied until they had again obtained the most perfect solution of the most general problem.

"But what is force?" said one of the committee. "Send for Mr. Cartesian," said the chairman, "we will give him another trial." Mr. Cartesian was accordingly requested to devise an engine of the most ferocious description wherewith to attack the rigid body. He was promptly ready with a scheme, the weapons being drawn from his trusty but old-fashioned armoury. He would erect three rectangular axes, he would administer a tremendous blow parallel to each of these axes, and then he would simultaneously apply to the body a forcible couple around each of them; this was the utmost he could do.

"No doubt," said the chairman, "what you propose would be highly effective, but, Mr. Cartesian, do you not think that while you still retained the perfect generality of your attack, you might simplify your specification of it? I confess that these three blows all given at once at right angles to each other, and these three couples which you propose to impart at the same time, rather confuse me. There seems a want of unity somehow. In short, Mr. Cartesian, your scheme does not create a distinct geometrical image in my mind. We gladly acknowledge its suitability for numerical calculation, and we remember its famous achievements, but it is utterly inadequate to the aspirations of this committee. We must look elsewhere."

Again Mr. Helix stepped forward. He reminded the committee of the labours of Mathematician Poinsot, and then he approached the rigid body. Helix commenced by clearing away Cartesian's arbitrary scaffolding of rectangular axes. He showed how an attack of the most perfect generality could be delivered in a form that admitted of concise and elegant description. "I shall," he said, "administer a blow upon the rigid body from some unexpected direction, and at the same instant I shall apply a vigorous couple in a plane perpendicular to the line of the blow."

A happy inspiration here seized upon Mr. Anharmonic. He knew, of course, that the efficiency of a couple is measured by its moment—that is, by the product of a force and a linear magnitude. He proposed, therefore, to weld Poinsot's force and couple into the single conception of a *wrench* on a screw. The force would be directed along the screw while the moment of the couple would equal the product of the force and the pitch of the screw. "A screw," he said, "is to be regarded merely as a directed straight line with an associated linear magnitude called the pitch. The screw has for us a dual aspect of much significance. No small movement of the body is conceivable which does not consist of a twist about a screw. No set of forces could be applied to the body which were not equivalent to a wrench upon a screw. Every one remembers the two celebrated rules that forces are compounded like rotations and that couples are compounded like translations. These may now be replaced by the single but far more compendious rule which asserts that wrenches and twists are to be compounded by identical laws. Would you unite geometry with generality in your dynamics? It is by screws, and screws only, that you are enabled to do so."

These ideas were rather too abstract for Cartesian, who remarked that as D'Alembert's principle provided for everything in dynamics screws could not be needed. Mr. Querulous sought to confirm him by saying that he did not see how screws helped the study either of Foucault's Pendulum or of the Precession of the Equinoxes.

Such absurd observations kindled the intellectual wrath of One-to-One, who rose and said, "In the development of the natural philosopher two epochs may be noted. At the first he becomes aware that problems exist. At the second he discovers their solution. Querulous has not yet reached the first epoch, he cannot even conceive those problems which the 'Theory of Screws' proposes to solve. I may however inform him that the 'Theory of Screws' is not a general dynamical calculus. It is the discussion of a particular class of dynamical problems which do not admit of any other enunciation except that which the theory itself provides. Let us hope that ere our labours have ended Mr. Querulous may obtain some glimmering of the subject."

The chairman happily assuaged matters. "We must pardon he said, "the vigorous language of our friend Mr. One-to-On His faith in geometry is boundless. In fact he is said to belie that the only real existence in the universe is anharmonic rati It is also his opinion that if a man travelled sufficiently far alo a straight line in one direction he will ultimately arrive at th point from which he started. The committee would be glad see Mr. Querulous making the trial."

It was obvious that screws were indispensable alike f the application of the forces and for the observation of the mov ments. Special measuring instruments were devised by whi the positions and pitches of the various screws could be careful ascertained. All being ready the first experiment was co menced.

A screw was chosen quite at random, and a great impulsi wrench was administered thereon. In the infinite majority cases this would start the body into activity, and it would co mence to move in the only manner possible—*i.e.* it would beg to twist about some screw. It happened, however, that this fir experiment was unsuccessful; the impulsive wrench failed operate, or at all events the body did not stir. "I told you would not do," shouted Querulous, though he instantly subsid when One-to-One glanced at him.

Much may often be learned from an experiment which fail and the chairman sagaciously accounted for the failure, and doing so directed the attention of the committee to an importa branch of the subject. The mishap was due, he thought, some reaction of the constraints which had neutralized the effe of the wrench. He believed it would save time in their futu investigations if these reactions could be first studied and the number and position ascertained.

To this suggestion Mr. Cartesian demurred. He urged th it would involve an endless task. "Look," he said, "at th complexity of the constraints: how the body rests on these su faces here; how it is fastened by links to those points ther how there are a thousand-and-one ways in which reactions mig originate." Mr. Commonsense and other members of the co mittee were not so easily deterred, and they determined to wo out the subject thoroughly. At first they did not see their wa clearly, and much time was spent in misdirected attempts. A length they were rewarded by a curious and unexpected di covery, which suddenly rendered the obscure reactions perfect transparent.

A trial was being made upon a body which had only o degree of freedom; was, in fact, only able to twist about single screw, X. Another screw, Y, was speedily found, su that a wrench thereon failed to disturb the body. It no occurred to the committee to try the effect of interchanging th relation of these screws. They accordingly arranged that th body should be left only free to twist about Y, while a wrenc was applied on X. Again the body did not stir. The impor ance of this fact immediately arrested the attention of the mo intelligent observers, for it established the following gener law: If a wrench on X fails to move a body only free to twi about Y, then a wrench on Y must be unable to move a bo only free to twist about X. It was determined to speak of tw screws when related in this manner as *reciprocal.*

Some members of the committee did not at first realize th significance of this discovery. Their difficulty arose from th restricted character of the experiments by which the law of re procal screws had been suggested. They said, "You have show us that this law is observed in the case of a body only free twist about one screw at a time; but how does this teach an thing of the general case in which the body is free to twist abo whole shoals of screws?" Mr. Commonsense immediat showed that the discovery could be enunciated in a quite u objectionable form. "The law of reciprocal screws," he sa "does not depend upon the constraints or the limitations of th freedom. It may be expressed in this way: *Two screws a reciprocal when a small twist about either can do no work agai a wrench on the other.*"

This important step at once brought into view the wh geometry of the reactions. Let us suppose that the freedom the body was such that it could twist about all the screws of system which we shall call U. Let all the possible reactio form wrenches on the screws of another system, V. It th appeared that every screw upon U is reciprocal to every scr upon V. A body might therefore be free to twist about eve screw of V and still remain in equilibrium, notwithstanding th presence of a wrench on every screw of U. A body free twist about all the screws of V can therefore be only partially free. Hence V must be one of those few types of screw system already discussed. It was, accordingly, found that the single screw, the cylindroid, and the set of hyperboloids completely described every conceivable reaction from the constraints just as they described every conceivable kind of freedom. The committee derived much encouragement from these discoveries; they felt that they must be following the right path, and that the bounty of Nature had already bestowed on them some earnest of the rewards they were ultimately to receive.

It was with eager anticipation that they now approached the great dynamical question. They were to see what would happen if the impulsive wrench were not neutralized by the reactions of the constraints. The body would then commence to move—that is, to twist about some screw which it would be natural to call the instantaneous screw. To trace the connexion between the impulsive screw and the corresponding instantaneous screw was the question of the hour. Before the experiments were commenced, some shrewd member remarked that the issue had not yet been presented with the necessary precision. "I understand," he said, "that when you apply a certain impulsive wrench, the body will receive a definite twist velocity about a definite screw; but the converse problem is ambiguous. Unless the body be quite free, there are myriads of impulsive screws corresponding to but one instantaneous screw." The chairman perceived the difficulty, and not in vain did he appeal to the geometrical instinct of Mr. One-to-One, who at once explained the philosophy of the matter, dissipated the fog, and disclosed a fresh beauty in the theory.

"It is quite true," said Mr. One-to-One, "that there are myriads of impulsive screws, any one of which may be regarded as the correspondent to a given instantaneous screw, but it fortunately happens that among these myriads there is always one screw so specially circumstanced that we may select it as *the* correspondent, and then the ambiguity will have vanished."

As several members were not endowed with the geometrical insight possessed by One-to-One, they called on him to explain how this special screw was to be identified; accordingly he proceeded:—"We have already ascertained that the constraints permit the body to be twisted about any screw of the system, U. Out of the myriads of impulsive screws corresponding to a single instantaneous screw it always happens that one, but never more than one, lies on U. This is the special screw. No matter where the impulsive wrench may lie throughout all the realms of space, it may always be exchanged for a precisely equivalent wrench lying on U. Without the sacrifice of a particle of generality, we have neatly circumscribed the problem. For one impulsive screw there is one instantaneous screw, and for one instantaneous screw there is one impulsive screw."

The experiments were accordingly resumed. An impulsive screw was chosen, and its position and its pitch were both noted. An impulsive wrench was administered, the body commenced to twist, and the instantaneous screw was ascertained by the motion of marked points. The body was brought to rest. A new impulsive screw was then taken. The experiment was again and again repeated. The results were tabulated, so that for each impulsive screw the corresponding instantaneous screw was shown.

Although these investigations were restricted to screws belonging to the system which expressed the freedom of the body, yet the committee became uneasy when they reflected that the screws of that system were still infinite in number, and that consequently they had undertaken a task of infinite extent. Unless some compendious law should be discovered, which connected the impulsive screw with the instantaneous screw, their experiments would indeed be endless. Was it likely that such a law could be found—was it even likely that such a law existed? Mr. Querulous decidedly thought not. He pointed out how the body was of the most hopelessly irregular shape and mass, and how the constraints were notoriously of the most embarrassing description. It was therefore, he thought, idle to search for any geometrical law connecting the impulsive screw and the instantaneous screw. He moved that the whole inquiry be abandoned. These sentiments seemed to be shared by other members of the committee. Even the resolution of the chairman began to quail before a task of infinite magnitude. A crisis was imminent—when Mr. Anharmonic rose.

"Mr. Chairman," he said, "Geometry is ever ready to help even the most humble inquirer into the laws of Nature, but Geometry reserves her most gracious gifts for those who interrogate Nature in the noblest and most comprehensive spirit. That spirit has been ours during this research, and accordingly Geometry in this our emergency places her choicest treasures at our disposal. Foremost among these is the powerful theory of homographic systems. By a few bold extensions we create a comprehensive theory of homographic screws. All the impulsive screws form one system, and all the instantaneous screws form another system, and these two systems are homographic. Once you have realized this, you will find your present difficulty cleared away. You will only have to determine a few pairs of impulsive and instantaneous screws by experiment. The number of such pairs need never be more than seven. When these have been found, the homography is completely known. The instantaneous screw corresponding to every impulsive screw will then be completely determined by geometry both pure and beautiful." To the delight and amazement of the committee, Mr. Anharmonic demonstrated the truth of his theory by the supreme test of fulfilled prediction. When the observations had provided him with a number of pairs of screws, one more than the number of degrees of freedom of the body, he was able to predict with infallible accuracy the instantaneous screw corresponding to any impulsive screw. Chaos had gone. Sweet order had come.

A few days later the chairman summoned a special meeting in order to hear from Mr. Anharmonic an account of a discovery he had just made, which he believed to be of signal importance, and which he was anxious to demonstrate by actual experiment. Accordingly the committee assembled, and the geometer proceeded as follows:—

"You are aware that two homographic ranges on the same ray possess two double points, whereof each coincides with its correspondent; more generally when each point in space, regarded as belonging to one homographic system, has its correspondent belonging to another system; then there are four cases in which a point coincides with its correspondent. These are known as the four double points, and they possess much geometrical interest. Let us now create conceptions of an analogous character suitably enlarged for our present purpose. We have discovered that the impulsive screws and the corresponding instantaneous screws form two homographic systems. There will be a certain limited number (never more than six) of double screws common to these two systems. As the double points in the homography of point systems are fruitful in geometry, so the double screws in the homography of screw systems are fruitful in dynamics."

A question for experimental inquiry could now be distinctly stated. Does a double screw possess the property that an impulsive wrench delivered thereon will make the body commence to move by twisting about the same screw? This was immediately tested. Mr. Anharmonic, guided by the indications of homography, soon pointed out the few double screws. One of these was chosen; a vigorous impulsive wrench was imparted thereon. The observations were conducted as before: the anticipated result was triumphantly verified, for the body commenced to twist about the identical screw on which the wrench was imparted. The other double screws were similarly tried, and with a like result. In each case the instantaneous screw was identical both in pitch and in position with the impulsive screw.

"But surely," said Mr. Querulous, "there is nothing wonderful in this. Who is surprised to learn that the body twists about the same screw as that on which the wrench was administered? I am sure I could find many such screws. Indeed, the real wonder is not that the impulsive screw and the instantaneous screw are ever the same, but that they are ever different."

And Mr. Querulous proceeded to illustrate his views by experiments on the rigid body. He gave the body all sorts of impulses, but, in spite of all his endeavours, the body invariably commenced to twist about some screw which was *not* the impulsive screw. "You may try till Doomsday," said Mr. Anharmonic, "you will never find any besides the few I have indicated."

It was thought convenient to assign a name to these remarkable screws, and they were accordingly designated the *principal screws of inertia*. There are, for example, six principal screws of inertia when the body is perfectly free, and two when the body is free to twist about the screws of a cylindroid. The committee regarded the discovery of the principal screws of inertia as the most remarkable result they had yet obtained.

Mr. Cartesian was very unhappy. The generality of the subject was too great for his comprehension. He had an invincible attachment to the x, y, z, which he regarded as the *ne plus ultra* of dynamics. "Why will you burden the science," he sighs, "with all these additional names? Can you not express what you want without talking about cylindroids, and twists, and wrenches, and impulsive screws, and instantaneous screws, and all the rest of it?" "No," said Mr. One-to-One, "there can be no simpler way of stating the results than that natural method we have followed. You would not object to the language if your ideas of the natural phenomena had been sufficiently capacious. We are dealing with questions of perfect generality, and it would involve a sacrifice of generality were we to speak of the movement of a body except as a twist, or of a system of forces except as a wrench."

"But," said Mr. Commonsense, "can you not as a concession to our ignorance tell us something in ordinary language which will give an idea of what you mean when you talk of your 'principal screws of inertia?' Pray for once sacrifice this generality you prize so much and put the theory into some extreme shape that ordinary mortals can understand."

Mr. Anharmonic would not condescend to comply with this request, so the chairmrn called upon Mr. One-to-One, who somewhat ungraciously consented. "I feel," said he, "the request to be an irritating one. Extreme cases generally make bad illustrations of a general theory. That zero multiplied by infinity may be anything is surely not a felicitous exhibition of the perfections of the multiplication table. It is with reluctance that I divest the theory of its flowing geometrical habit, and present it only as a stiff conventional guy from which true grace has departed.

"Let us suppose that the rigid body, instead of being constrained as heretofore in a perfectly general manner, is subjected merely to a special type of constraint. Let it, in fact, be only free to rotate around a fixed point. The beautiful fabric of screws, which so elegantly expressed the latitude permitted to the body before, has now degenerated into a mere horde of lines all stuck through the point. Those varieties in the pitches of the screws which gave colour and richness to the fabric have also vanished, and the pencil of degenerate screws have a monotonous zero of pitch. Our general conceptions of mobility have thus been horribly mutilated and disfigured before they can be adapted to the old and respectable problem of the rotation of a rigid body about a fixed point. For the dynamics of this problem the wrenches assume an extreme and even monstrous type. Wrenches they still are, as wrenches they ever must be, but they are wrenches on screws of infinite pitch; they have ceased to possess definite screws as homes of their own. We often call them couples.

"Yet so comprehensive is the doctrine of the principal screws of inertia that even to this extreme problem the theory may be applied. The principal screws of inertia reduce in this special case to the three principal axes drawn through the point. In fact, we see that the famous property of the principal axes of a rigid body is merely a very special application of the general theory of the principal screws of inertia. Everyone who has a particle of mathematical taste lingers with fondness over the theory of the principal axes. Learn, therefore," says One-to-One in conclusion, "how great must be the beauty of a doctrine which comprehends the theory of principal axes as the merest outlying detail."

Another definite stage in the labours of the committee had now been reached, and accordingly the chairman summarized the results. He said that a geometrical solution had been obtained of every conceivable problem as to the effect of impulse on a rigid body. The impulsive screws and the corresponding instantaneous screws formed two homographic systems. Each screw in one system determined its corresponding screw in the other system, just as in two anharmonic ranges each point in one determines its correspondent in the other. The double screws of the two homographic systems are the principal screws of inertia. He remarked, in conclusion, that the geometrical theory of homography and the present dynamical theory mutually illustrated and interpreted each other.

There was still one more problem which had to be brought into shape by geometry, and submitted to the test of experiment.

The body is lying at rest though gravity and many other forces are acting upon it. These forces constitute a wrench which must lie upon a screw of the reciprocal system, inasmuch as it is neutralized by the reaction of the constraints. Let the body be displaced from its initial position by a small twist. The wrench will no longer be neutralized by the reaction of the constraints; accordingly when the body is released it will commence to move. So far as the present investigations are concerned these movements are small oscillations. Attention was therefore directed to these small oscillations. The usual observations were made, and Helix reported them to be of a very perplexing kind. "Surely," said the chairman, "you find the body twisting about some screw, do you not?" "Undoubtedly," said Helix; "the body can only move by twisting about some screw; *but*, unfortunately, this screw is not fixed, it is indeed moving about in such an embarrassing manner that I can give no intelligible account of the matter." The chairman appealed to the committee not to leave the interesting subject of small oscillations in such an unsatisfactory state. Success had hitherto guided their efforts. Let them not separate without throwing the light of geometry on this obscure subject.

Mr. Querulous here said he must be heard. He protested against further waste of time; there was nothing for them to do. Everybody knew how to investigate small oscillations; the equations were given in every book on mechanics. You had only to write down these equations, and scribble away till you got out something or other. But the more intelligent members of the committee took the same view as the chairman. They did not question the truth of the formulæ which to Querulous seemed all-sufficient, but they wished to see what geometry could do for the subject. Fortunately this view prevailed, and new experiments were commenced under the direction of Mr. Anharmonic. He first quelled the elaborate oscillations which had so puzzled the committee; he reduced the body to rest, and then introduced the subject as follows:—

"The body now lies at rest. I displace it a little, and I hold it in its new position. The wrench, which is the resultant of all the varied forces acting on the body, is no longer completely neutralized by the reactions of the constraints. Indeed, I can feel it in action. Our apparatus will enable us to measure the intensity of this wrench, and to determine the screw on which it acts."

A series of experiments was then made, in which the body was displaced by a twist about a screw, which was duly noted, while the corresponding evoked wrench was determined. The pairs of screws so related were carefully tabulated. When we remember the infinite complexity of the forces, of the constraints and of the constitution of the body, it might seem an endless task to determine the connexion between the two systems of screws. Here Mr. Anharmonic pointed out how exactly modern geometry was adapted to supply the wants of dynamics. The two screw systems were homographic, and when a number of pairs, one more than the degrees of freedom of the body, had been found, all was determined. This statement was put to the test. Again and again the body was displaced in some new fashion, but again and again did Mr. Anharmonic predict the precise wrench which would be required to maintain the body in its new position.

"But," said the chairman, "are not these purely statical results. How do they throw light on those elaborate oscillations which seem at present so inexplicable?"

"This I shall explain," said Anharmonic; "but I beg of you to give me your best attention, for I think the theory of small oscillations will be found worthy of it.

"Let us think of any screw, α, belonging to the system U, which expresses the freedom of the body. If α be an instantaneous screw, there will of course be a corresponding impulsive screw, θ, also on U. If the body be displaced from a position of equilibrium by a small twist about α, then the uncompensated forces produce a wrench, ϕ, which, without loss of generality, may also be supposed to lie on U. According as the screw α moves over U so will the two corresponding screws θ and ϕ also move over U. The system represented by α is homographic with both the systems of θ and of ϕ respectively. But two systems homographic with the same system are homographic with each other. Accordingly the θ system and the ϕ system are homographic. There will therefore be a certain number of double screws (not more than six) common to the systems θ and ϕ. Each of these double screws will of course have its correspondent in the α system, and we may call them α_1, α_2, &c., their number being equal to the degrees of freedom of the body. These screws are most curiously related to the small oscillations. We shall first demonstrate by experiment the remarkable property they possess."

The body was first brought to rest in its position of equilibrium. One of the special screws α having been carefully determined oth in position and in pitch, the body was displaced by a twist bout this screw and was then released. As the forces were ncompensated, the body of course commenced to move, but the scillations were of unparalleled simplicity. With the regularity f a pendulum the body twisted to and fro on this screw, just as it were actually constrained to this motion alone. The com- ittee were delighted to witness a vibration so graceful, and, emembering the complex nature of the ordinary oscillations, ey appealed to Mr. Anharmonic for an explanation. This he ladly gave, not by means of complex formulæ, but by a line of easoning that was highly commended by Mr. Commonsense, nd such that even Mr. Querulous could understand.

"This pretty movement," said Mr. Anharmonic, "is due to he nature of the screw α_1. Had I chosen any screw at random, he oscillations would, as we have seen, be of a very complex ype; for the displacement will always evoke an uncompensated rench, in consequence of which the body will commence to move y twisting about the instantaneous screw corresponding to that rench; and of course this instantaneous screw will usually be uite different from the screw about which the displacement was ade. But you will observe that α_1 has been chosen as a screw n the instantaneous system, corresponding to one of the double crews in the θ and ϕ systems. When the body is twisted about $_1$ a wrench is evoked on the double screw, but as α_1 is itself the nstantaneous screw, corresponding to the double screw, the only ffect of the wrench will be to make the body twist about α_1. hus we see that the body will twist to and fro on α_1 for ever. inally, we can show that the most elaborate oscillations the ody can possibly have may be produced by compounding the imple vibrations on these screws α_1, α_2, &c."

Great enlightenment was now diffused over the committee, nd even Mr. Querulous began to think there must be something n it. Cordial unanimity prevailed among the members, and it as appropriately suggested that the screws of simple vibra- on should be called *harmonic screws*. This view was adopted y the chairman, who said he thought he had seen a similar xpression in "Thomson and Tait."

The final meeting showed that real dynamical enthusiasm had een kindled in the committee. Vistas of great mathematical eories were opened out in many directions. One member owed how the theory of screws could be applied not merely to single rigid body but to any mechanical system whatever. He etched a geometrical conception of what he was pleased to call *screw-chain*, by which he said he could so bind even the most aborate system of rigid bodies that they would be compelled conform to the theory of screws. Nay, soaring still further to the empyrean, he showed that all the instantaneous motions every molecule in the universe were only a twist about one rew-chain while all the forces of the universe were but a wrench on another.

Mr. One-to-One expounded the "Ausdehnungslehre," and owed that the theory of screws was closely related to parts of rassman's great work; while Mr. Anharmonic told how lücker, in his celebrated "Neue Geometrie des Raumes," had lvanced some distance towards the theory of screws, but still d never touched it.

The climax of mathematical eloquence was attained in the eech of Mr. Querulous, who, with new-born enthusiasm, unched into appalling speculations. He had evidently been ading his "Cayley," and had become conscious of the poverty geometrical conception arising from our unfortunate residence a space of an arbitrary and unsymmetrical description.

"Three dimensions," he said, "may perhaps be enough for intelligent geometer. He may get on fairly well without a ur-dimensioned space, but he does most heartily remonstrate ainst a flat infinity. Think of infinity," he cries, "as it ould be, perhaps even as it is. Talk not of your scanty aight line at infinity and your miserable pair of circular points. ldly assert that infinity is an ample quadric, and not the mere ost of one; and then geometry will become what geometry ght to be. Then will every twist resolve itself into a right ctor and a left vector, as the genius of Clifford proved. Then ll the 'theory of screws' shed away some few adhering formities, and fully develop its shapely proportions. Then ll——" But here the chairman said he feared the dis- ssion was beginning to enter rather wide ground. For his rt he was content with the results of the experiments, en though they had been conducted in the vapid old space of clid. He reminded them that their labours were now com- ted, for they had ascertained everything relating to the rigid body which had been committed to them. He hoped they would agree with him that the inquiry had been an instructive one. They had been engaged in the study of Nature. They had approached the problems in the true philosophical spirit, and the rewards they had obtained proved that

"Nature never did betray
The heart that truly loved her."

NOTES.

At a public meeting held on Tuesday in Newcastle, under the presidency of the Mayor, Sir B. L. Brown, it was finally decided, on the motion of the Sheriff, Alderman W. H. Stephenson, seconded by Prof. Philipson, head of the medical staff at the Royal Infirmary, that a cordial invitation should be sent to the British Association to hold their annual meeting in Newcastle in 1889. It was stated that the necessary amount to cover expenses would be £4000, and of this £1700 had been already subscribed.

The New York meeting of the American Association for the Advancement of Science seems to have been very successful, although the attendance was not so large as had been expected. The next meeting will be held in Cleveland, O. An invitation from Toronto came just too late. The following are the officers for the next meeting:—President, J. W. Powell, of Washington; Vice-Presidents, Ormond Stone, of the University of Virginia, (Mathematics and Astronomy), A. A. Michelson, of Cleveland, (Physics), C. E. Munroe, of Newport, (Chemistry), Calvin M. Woodward, of St. Louis, (Mechanical Science), George H. Cook, of New Brunswick, (Geology and Geography), C. V. Riley, of Washington, (Biology), C. C. Abbot, of Trenton, (Anthropology), C. W. Smiley, of Washington, (Economic Science and Statistics); Permanent Secretary, F. W. Putnam, of Cambridge, (office, Salem, Mass.); General Secretary, J. C. Arthur, of La Fayette; Secretary of the Council, C. Leo Mees, of Athens; Secretaries of the Sections, C. L. Doolittle, of Bethlehem, (Mathematics and Astronomy), A. L. Kimball, of Baltimore, (Physics), William L. Dudley, of Nashville, (Chemistry), Arthur Beardsley, of Swarthmore, (Mechanical Science), George H. Williams, of Baltimore, (Geology and Geography), N. L. Britton, of New York, (Biology), Frank Baker, of Washington, (Anthropology), Charles S. Hill, of Washington, (Economic Science and Statistics).

The twenty-fourth annual meeting of the British Pharmaceutical Conference was opened on Tuesday in the Chemical Theatre of Owens College, Manchester. There was a large attendance of members of the Association. Mr. S. R. Atkins, of Salisbury, occupied the chair, and in his presidential address invited the attention of the Conference to "a brief review of the Victorian era as it more especially affected themselves as pharmacists."

The International Astronomical Congress met at Kiel on Monday, in the large hall of the University, under the presidency of Privy Councillor Dr. Auwers, of Berlin. There was a large assembly of astronomers, including delegates from Austria, France, Sweden and Norway, and America. The delegates were received on behalf of the Government by Herr Steinmann, Civil Governor of the province of Schleswig-Holstein, and on the part of the University by the Rector, Prof. Harsen. Dr. Auwers, in replying, thanked the Prussian Government for the interest which it had manifested in the Congress.

The Hygienic Congress, which will meet in Vienna next month, will be attended by over 1400 delegates from all countries. The programme includes excursions to the Kahlenberg, the Semmering, Buda-Pesth, and Abbazzia.

The Academy of Aërostation of France has presented a medal to M. Mendeleieff, in recognition of the pluck exhibited by him at Klin on August 19, when he went up alone in a balloon, although he had never been in one before. The Russian Ambassador in Paris has undertaken to transmit the medal to M. Mendeleieff.

THE *Ceylon Observer* of August 1 announces the death on July 31 of Mr. W. Ferguson at the age of sixty-seven. Mr. Ferguson arrived in Ceylon in December 1839, and at once entered upon the arduous duties of Surveyor to the Government, a post which he filled for many years. He finally relinquished it very much shattered in constitution from exposure to climate. He was an enthusiastic naturalist, and employed the opportunities his profession afforded him for observation with pleasure to himself and advantage to others. Botany especially profited by his knowledge and exertions. He contributed largely to Thwaites's "Enumeratio Plantarum Zeylaniæ," and also to other works relating to the vegetation of Ceylon; and his aid was warmly acknowledged by the various authors whom he assisted. He was of much service to the Eclipse Expedition of 1871.

THE death is announced of Dr. Vincenz Kosteletzky, formerly Professor at the University, and the Director of the Botanical Gardens, at Prague. He died on August 19 at the age of eighty-seven.

THE Council of the Institution of Civil Engineers has issued a list of subjects on which it invites original communications. For approved papers the Council has the power to award premiums, arising out of special funds bequeathed for the purpose.

THE *Times* of Tuesday printed some notes about the eclipse which had reached German papers from Siberia and various stations in the Russian eastern provinces. At Tomsk the astronomers were able to observe not only the total eclipse but the corona in a very satisfactory way. In most houses it was necessary to light candles or lamps. The eclipse began at 10.22 a.m., and ended at 11.46. The weather was very fine and the sky clear. At Krasnoyarsk, in the Government of Yeniseisk, the corona was very well photographed. At Irbit the period of absolute totality was at 8.44 a.m., and lasted 1½ minute. Prof. Stanoievich, from Belgrade, was very successful in his observations at Petrovsk; he saw and photographed the green line in the corona. Prof. Kononovich, of Odessa, was equally fortunate, obtaining photographs of the whole spectrum. At Ekaterinburg the eclipse began in a cloudless sky at 7.25 a.m., and lasted till 9.30. The temperature fell from 19° C. to 13° (about 55½° F.) at 8.37 a.m., and rose to 24° (over 75° F.) after the eclipse. At Novocherkask the sky was cloudless, but only about a quarter of the sun's surface was obscured, the appearance presented being a reaping-hook with the handle and point uppermost. Photographic sketches were taken every five minutes. At Savidovo the sky became suddenly clouded as the moment of the eclipse approached, and the sun was not visible till noon. The actual moment of the total eclipse could only be noted by the intense darkness which suddenly spread over the whole district. Here and there a yellowish or leaden-gray tint could be distinguished in the clouds, presenting a most weird appearance; and the strangeness of the scene was heightened by the profound disquiet and fear which seemed to have taken possession of the birds and the cattle in the fields.

PROF. YOUNG has returned from Russia, and is attending the Manchester meeting of the British Association.

THERE is a chance that, although the English Technical Education Bill has been abandoned, the corresponding Scotch measure may become law. The House of Commons went into Committee on the Bill on Monday night.

ACCORDING to the Meteorological Council, the telegrams received from the Ben Nevis Observatory have been of no service whatever as aids to the issue of storm warnings from the Meteorological Office. Mr. A. Buchan, in a letter to Mr. R. H. Scott, complains that the memorandum in which this judgment is pronounced is very misleading. "The finding of the memorandum," says Mr. Buchan, "is that the telegrams received from the Ben Nevis Observatory are absolutely useless to the Meteorological Office in issuing storm warnings. This statement is so incomplete that we do not think that in preparing the memorandum for your report the instructions have been kept in view which were sent to Mr. Omond, in accordance with your letter of December 3, 1883, a copy of which, so far as refers to this matter, is herewith sent. A copy of this letter was sent to Mr. Omond, with instructions to carry out your wishes to the best of his ability. Now, in these instructions to Mr. Omond no special mention is made of storms or storm warnings; and certainly neither the directors nor the staff at the Observatory have ever supposed that it was expected by the Meteorological Office that a telegram was to be sent for every storm that had actually broken out or appeared to be threatened. This, however, is the assumption of the memorandum. We therefore think that in these circumstances the finding of the memorandum will be misleading to those of the public who have little or no knowledge of meteorological matters, and of the nature of the information asked from Ben Nevis Observatory; and as regards others it may be considered as doubtful if the finding that weather telegrams from Ben Nevis Observatory are useless will be indorsed by them. As you are aware, the directors offered the Meteorological Office, in their letter of November 16, 1883, daily weather telegrams from both Ben Nevis Observatory and the low-level station at Fort William. This offer, however, the Meteorological Council did not see their way to accept, chiefly on the ground of the expense; but asked for telegrams whenever any very striking change of conditions or a special phenomenon of great interest was recorded. This has been done by Mr. Omond, and, so far as the directors are aware, no application has been made by the Meteorological Office for more frequent telegrams or for any other information. The directors, in view, then, of the limited nature of the information asked for, would have been surprised if any other result had been found than that stated in the memorandum."

IN Mr. Symons's "British Rainfall," which we briefly reviewed last week, it is shown that the total fall in 1886 was rather above the average, but not exceptionally so, the amount being for England and Wales 37·53 inches, for Scotland 37·31 inches, and for Ireland (four stations only) 41·61 inches. The mean of all stations was 37·59 inches, or about 7 per cent. above the average for a long series of years. Some one ought now to discuss the observations with the view of showing the *probability* of rainfall for each month, and also with the view of showing the seasonal rainfall for the whole period now available.

MR. EDWARD SANGER SHEPHERD has sent us a very fine photograph of lightning taken by him at Norfolk Terrace, Westbourne Grove, W., during the thunder-storm of Wednesday, August 17. While the storm lasted Mr. Shepherd exposed fifteen plates, seven of which were successful. The apparatus used was a half-plate square box camera and a portrait lens of 1¾ inches aperture; the plates used were "Ilford extra rapid."

IN a letter to the *Times* of Wednesday Prof. Tyndall calls attention to the very imperfect way in which lightning conductors are often set up. Some years ago a rock lighthouse on the coast of Ireland was struck and damaged by lightning; and when the facts were brought before Prof. Tyndall, as scientific adviser of the Trinity House and Board of Trade, he found that the lightning conductor had been carried down the lighthouse tower, its lower extremity being carefully embedded in a stone perforated to receive it. "If the object," says Prof. Tyndall, "had been to invite the lightning to strike the tower, a better arrangement could hardly have been adopted. I gave directions to have the ductor immediately prolonged, and to have added to it a large minal plate of copper, which was to be completely submerged the sea. The obvious convenience of a chain as a prolonga-n of the conductor caused the authorities in Ireland to propose but I was obliged to veto the adoption of the chain. The tact of link with link is never perfect. I had, moreover, be-e me a portion of a chain cable through which a lightning charge had passed, the electricity in passing from link to link countering a resistance sufficient to enable it to partially fuse chain. The abolition of resistance is absolutely necessary in necting a lightning conductor with the earth, and this is done closely embedding in the earth a plate of good conducting terial and of large area. The largeness of area makes atone-nt for the imperfect conductivity of earth. The plate, in t, constitutes a wide door through which the electricity passes ely into the earth, its disruptive and damaging effects being reby avoided." Prof. Tyndall understands that lightning ductors are frequently set up without any terminal plate atever. It is said that the Bishop of Winchester's palace at rnham is "protected" in this way. If this is true, the Bishop l be interested to hear that the "protection" is "a mockery, elusion, and a snare."

WE have received the twelfth Report of the Bradford ilosophical Society. This institution was revived two years , and we are glad to see from the Report that it has "a ght prospect of success." The Society is closely associated h a group of affiliated Societies in Bradford, and it has been nd that this plan works well. "The joint programme of the cieties," says the Report, "is one that reflects great credit on town, and members of the Philosophical Society would do l to avail themselves (as their membership allows) of the ous lectures and excursions of the united Societies. Members the Society may be assured of a hearty welcome." The iated Societies are the Historical and Antiquarian Society, Microscopical Society, the Naturalists' Society, the Scientific ociation, and the Browning Society.

IEUT. WISSMANN, the well-known African traveller, has ved at Mozambique. He intends to proceed to Zanzibar on way back to Europe.

HREE large packages containing rare plants and specimens n India have been received from Calcutta by the Keeper of Ethnographical Department of the British Museum.

SHOCK of earthquake was felt in Mexico at seven o'clock Monday morning. The houses were shaken and the inhabit- much terrified, but no damage was done. The direction of shock was from north to south. The shock was also felt at pancingo, where two arches of an arcade in the main square demolished, at Orizaba, Tlaltan, and Otumba.

LARGE proportion of the salmon fry hatched out by the rn Fishery Board at the new hatchery at Worcester this are being reared by Mr. William Burgess in his ponds at vern Wells, pending their transference to the open river. worthy of note that the fry may be seen rising continually e fly. Seeing that they inhabit the bottom of the river in wild state and do not rise, this is rather remarkable. Their of growth does not seem to be so fast as that of other fish, ugh their present position is well suited to their require-s.

IE additions to the Zoological Society's Gardens during the week include a Rhesus Monkey (*Macacus rhesus*) from , presented by Miss Austin; a —— Capuchin (*Cebus* ——) South America, presented by Mr. J. H. Williams; two ed Lizards (*Phrynosoma cornutum*) from North America, nted by Mr. Maxwell Blackie; two Common Boas (*Boa constrictor*) from Dominica, W.I., presented by Mr. A. Nicholls; Smooth Snake (*Coronella lævis*) from Hampshire, presented by Mr. Sidney G. Smith; a Lion Marmoset (*Midas rosalia*), a Peba Armadillo (*Tatusia peba*), two Blue-bearded Jays (*Cyanocorax cyanopogon*), an Ariel Toucan (*Ramphastos ariel*), three Bahama Ducks (*Dafila bahamensis*), a Laughing Gull (*Larus atricilla*) from Brazil, a Black-handed Spider Monkey (*Ateles melanochir* ♀) from Central America, eight Blanding's Terrapins (*Clemmys blandingi*) from Michigan, U.S.A., purchased; two Hybrid Australian Ibises (between *Ibis strictipennis* and *Ibis bernieri*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN

VARIABLE STAR IN THE RING NEBULA IN LYRA.—Herr Spitaler draws attention in the *Astronomische Nachricten*, No. 2800, to the apparent variability of the small star near the centre of this well-known nebula. He had made himself pretty well acquainted with the nebula in September 1885, when he had sketched it, but was induced to examine it again last autumn from the note on the "ring-formed nucleus" discovered by means of photography, which Herr E. von Gothard had published in the *Astronomische Nachricten*, No. 2749. The interior of the ring nebula appeared with a low power to be covered with a faint curtain of light, which a high power showed to be of varying intensity, so that the interior had a faint flocculent appearance; a bright speck of light was also easily recognized midway between the centre of the nebula and the inner edge of the ring on the south-west side. In the eastern portion three faint stars were seen several times, but a fourth star seen by Prof. Vogel, and shown on the photographs of the Bros. Henry, could not be made out. But on July 25 of the present year, during the visit of Prof. Young to the Vienna Observatory, on the telescope being again turned to the nebula a small star was seen at the first glance a very little north-west of the centre, just as it is shown in the Gothard photograph, but a little fainter. The following night it was seen again, but not so distinctly. The star would therefore appear to be variable, and well worth watching. The evidence of Herr von Gothard's photograph, which shows it, whilst a faint star in the neighbourhood is not represented, seems to indicate that it is particularly rich in actinic light.

NEW VARIABLE STAR.—Mr. Espin announces in Circular No. 17 of the Wolsingham Observatory that the star Birmingham 541 is variable from 6·6 ± to 8·0 ±. The star's place for 1887 is R.A. 20h. 9m. 17s; Decl. 38° 22'·0 N.

DISCOVERY OF A COMET.—Mr. W. R. Brooks, Red House Observatory, Phelps, New York, discovered a comet on August 24, 20h. 53m. G.M.T. Place of the comet, R.A. 8h. 33m., Decl. 29° 0' N. It seems probable that this object is the expected comet of Olbers.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 SEPTEMBER 4-10.

(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 4

Sun rises, 5h. 19m.; souths, 11h. 58m. 58·3s.; sets, 18h. 39m.; decl. on meridian, 7° 13' N.: Sidereal Time at Sunset, 17h. 33m.

Moon (at Last Quarter Sept. 10, 15h.) rises, 19h. 25m.*; souths, 1h. 9m.; sets, 7h. 4m.; decl. on meridian, 3° 39' S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° '
Mercury	4 38	11 39	18 40	11 8 N.
Venus	8 0	13 17	18 34	9 8 S.
Mars	1 46	9 39	17 32	19 59 N.
Jupiter	10 7	15 11	20 15	11 35 S.
Saturn	1 31	9 24	17 17	19 57 S.

* Indicates that the rising is that of the preceding evening.

Sept.	h.	
10	18	Mercury in superior conjunction with the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·3 ...	81 16 N. ...	Sept. 5,	19 6 *m*
λ Tauri...	3 54·4 ...	12 10 N. ...	,, 7,	4 14 *m*
V Geminorum ...	7 16·8 ...	13 19 N. ...	,, 9,	*M*
R Leonis Minoris.	9 38·8 ...	35 2 N. ...	,, 10,	*M*
δ Libræ	14 54·9 ...	8 4 S. ...	,, 5,	4 31 *m*
			,, 9,	20 14 *m*
U Coronæ	15 13·6 ...	32 4 N. ...	,, 9,	4 52 *m*
U Ophiuchi... ...	17 10·8 ...	1 20 N. ...	,, 5,	5 32 *m*
		and at intervals of		20 8
X Sagittarii... ...	17 40·5 ...	27 47 S. ..	Sept. 7,	22 0 *m*
W Sagittarii ...	17 57·8 ...	29 35 S. ...	,, 9,	0 0 *m*
U Sagittarii... ...	18 25·2 ...	19 12 S. ...	,, 10,	5 0 *M*
β Lyræ...	18 45·9 ...	33 14 N. ...	,, 9,	0 0 *M*
η Aquilæ	19 46·7 ...	0 43 N. ...	,, 7,	0 0 *M*
δ Cephei	22 25·0 ...	57 50 N. ...	,, 5,	2 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near φ Tauri	62° ...	37° N. ...	Swift; streaks.
,, 15 Orionis... ..	74 ...	14 N. ...	Swift; streaks.
,, α Andromedæ ...	355 ...	39 N. ...	Very swift.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 22.—M. Janssen in the chair.—On the eclipse of August 19, by M. J. Janssen. The reports received from the various stations in European Russia and Prussia are generally described as unfavourable, owing to the clouded state of the weather at the critical time. A telegram, however, from M. Stanoiewitch states that at the Petrovsk station it was clear enough to take some photographs and to make a few observations. Much regret is expressed that so few observers could be induced to visit the Siberian stations, where much more successful studies might have been made.—On the cooling of the terrestrial crust, by M. Faye. This is a protest against the Rev. Ch. Braun, who, in his recent work on "Cosmogony from the Stand-point of Christian Science," adopts without acknowledgment the author's fundamental theory that the chilling process goes on more rapidly and more deeply under the seas than under the continents. M. Faye complains that M. Braun refers to him by name when criticizing his views, but omits to do so when adopting and reproducing them.—Solution of a problem, by M. J. Bertrand. Supposing a scrutiny of the ballot for two candidates, A and B, the number of voters being μ; A, the successful candidate, obtaining m and B $\mu - m$ votes, what is the probability that during the scrutiny the number of votes for A will throughout exceed those of his rival? A rigorously algebraic solution is given of this problem, and it is added that the result may perhaps be shown in a more direct way. Thus, if the number of voters be sixty, the successful candidate must have obtained forty-five votes in order that the probability of keeping the majority throughout the scrutiny be equal to $\frac{1}{2}$.—Remarks accompanying the presentation of a memoir on the means of avoiding collisions at sea, by M. Moise Lion. The author considered that optical signals of great intensity could alone present sufficient guarantees of penetration in foggy weather. On board ships in motion the warning signal should consist of an electric focus projecting its light obliquely to the horizon and revolving round a vertical axis. He insists on the great advantage of imparting to the light an oscillatory motion in order to increase its luminosity.—On the partial lunar eclipse partly visible at Orgères (Eure-et-Loire) on August 3, by M. Edm. Lescarbault. The shadow cast on the upper left part of the moon was almost black; but to the left, and especially to the right, there were noticed two curvilinear triangles of 2′·5 to 3′·5 length at base, where the shadow was ruddier than a very deep maroon. The triangle to the left was even darker than that to the right, while both were connected by a thin streak of the same colour, but deeper to the south of the moon. The inner edges of these maroon surfaces blended insensibly in the black shadow, and within them could be very faintly distinguished a few cirques, which could not be otherwise accurately determined. On the disk the shadow was edged with a grayish straw-coloured band, two and a half or three times as broad as Tycho, the common edge of this band and of the shadow bei[ng] somewhat sharply traced.—On the coefficient of self-induction [of] two bobbins combined in quantity, by MM. G. Maneuvrier a[nd] P. Ledeboer. In a previous paper the authors dealt w[ith] the problem whether from the stand-point of self-induction [it] was possible to compensate two bobbins combined in quant[ity] by a single bobbin, and consequently whether it might be possi[ble] to assign to such a system a determined coefficient of self-indu[c]tion in the strict sense of the term. A fresh series of expe[ri]ments are here described which have been carried out for t[he] purpose of determining how far the results already obtained m[ay] be approximately verified for the most general case. The experiments lead to the conclusion that for the general case t[he] system of two bobbins cannot be compensated by a single bobbi[n], and consequently that such a system has no coefficient of se[lf-]induction properly so called.—On the compressibility of so[me] solutions of gas, by M. F. Isambert. From the experimen[ts] here described the author infers that a simple solution of g[as] changes very little the coefficient of compressibility of t[he] solvent; further that the solution of ammoniac gas in wat[er] behaves in the same way as that of a true chemical compoun[d]. —On the titanates of zinc, and more particularly on a trititanat[e], by M. Lucien Lévy. Metallic titanates are obtained either [by] the action of the metallic oxide on the titanic acid in the presen[ce] of the chloride or the fluoride, or else by the action of a mixtu[re] of the metallic sulphate and an alkaline sulphate on the sa[me] acid. Applied to the production of the titanates of zinc the[se] two processes have yielded different results. The first, which [is] here more specially dealt with, leads in general to a trititanat[e]. The second, on the contrary, furnishes several salts according [to] the proportions employed.

BOOKS, PAMPHLETS, and SERIALS RECEIVE[D]

Science and Art Schools and Classes Directory, 1887 (Eyre and Spotti[s]woode).—Calendar of Durham College of Science, Newcastle-on-Tyn[e] 1887–88 (Newcastle-on-Tyne).—Insects Noxious to Agriculture and Plan[ts] in New Zealand; The Scale Insects: W. M. Maskell (Wellington).—Electrical Distribution by Alternating Currents and Transformers: [R.] Kennedy (H. Alabaster and Co.).—Proceedings of the Liverpool Naturalis[ts'] Field Club, 1886-87 (Liverpool).—Economic Forestry: Prof. Boulger.—Transactions and Proceedings of the New Zealand Institute, 1886, vol. xi[x] (Wellington).—Quarterly Journal of the Geological Society, vol. xliii. Part [3] No. 171 (Longmans).—Journal of Physiology, vol. viii. Nos. 3 and 4 (Cam[bridge]).—Annalen der Physik und Chemie, 1887, No. 9 (Barth, Leipzig).—Quarterly Journal of Microscopical Science, August (Churchill).—Th[e] Asclepiad, No. 15, vol. iv.: Dr. B. W. Richardson (Longmans).

CONTENTS.

THURSDAY, SEPTEMBER 8, 1887.

HYDROPHOBIA.

THE culmination of scientific knowledge in any special direction frequently appears to the casual observer as a sudden and unforeseen event, although actually the result of a combination of well-ascertained facts accumulated during many years. The introduction of rabies from among the inexact to among the more exactly known diseases has been however so rapid as to very fairly substantiate this popular belief. It is now scarcely more than three years ago since the self-sacrificing labours of M. Pasteur helped us to pass from superstition to accurate knowledge of the real nature of rabies or hydrophobia, and this passage from the pre-scientific to the scientific epoch of the subject was actually perfectly abrupt. Nearly the same thing may also be said of the discovery by Koch of the *Bacillus tuberculosis*, for in both cases the scientific grasp of the subject undoubtedly commenced with the discovery how the virus might be isolated for purposes of experiment. While, however, this is strictly the case with tuberculosis, it is but partially true for rabies, for though there cannot be a shadow of doubt that the micro-organism which is the virus of rabies will soon be demonstrated in pure cultivations, this one factor is yet wanting to place it on the same level as that of tuberculosis. This difficulty in obtaining pure cultures, though a serious defect in our information, is yet most interesting, for it affords a distinct interpretation of several facts in the etiology of the disease, which, as we shall see directly, proved obstacles to early inquirers, and which yet at the same time, when viewed in the present light of science, are most encouraging to those who are anxious to see this miserable evil extinguished for ever.

All observers, notably Mr. Dowdeswell, are agreed that a micrococcus can be demonstrated in the tissues of the spinal cord of animals affected with the disease, but unless we accept the doubtful results of Fol no one has yet succeeded in cultivating this micrococcus. Those familiar with the difficulties of "rearing" pathogenic organisms will readily understand this obstacle in a disease with such a long incubation period as rabies.

M. Pasteur at the outset of his investigations attempted to solve the problem in this direction, but fortunately for science soon abandoned it in view of the probability that the virus would best be dealt with by endeavouring to obtain it in quantity from the central nervous system, since from the symptoms it evidently there produced its greatest effect, and so might be expected to be more especially present. He therefore made an emulsion by crushing in sterilized *bouillon* portions of the central nervous system, specially the spinal cord. With this emulsion he inoculated the disease from animal to animal by injecting a small quantity of it beneath the dura mater. By this simple procedure he established the first of his most important discoveries, viz. the real incubation period of the disease. At the same time too, as is usual in instances of a genuine scientific advance, the one important discovery led to a further one, since he has thus presented science with an infallible means of determining whether an animal had really suffered from the disease or not.

To the public this test has already been of the utmost value, since, as is well known, the characteristic lesions in the alimentary canal, &c., being absent or but feebly marked in the early stages of the disease, the possibility of thus giving a definite opinion in cases of doubt by the aid of Pasteur's method has frequently been the means of affording the utmost relief to the minds of those who have been victims to the lingering dread of hydrophobia. We are not aware however that the slightest public expression of gratitude has ever been expressed in this country towards that experimental science which in M. Pasteur's hands has led to such an important result, or that we, who by reason of rabies being endemic among us are profiting and will profit, enormously by the light thus shed on the subject, have acknowledged our indebtedness to him in any way.

From his researches M. Pasteur was led on to formulate certain deductions which might be accepted as the logical consequence of the theory of the disease thus shown by him to be indubitably zymotic.

In the first place, M. Pasteur considered it probable that he could attenuate the virus which he had just discovered the possibility of handling with certainty, and we may add safety. This he soon accomplished by the method of drying.

It might naturally be supposed that, having proceeded thus far, he would have been led to attempt protection from, and prevention of, the evil effects of the disease by inoculating with this attenuated virus. Indeed it is unfortunately the popular belief that he did do this, and that his efforts to cure the malady are conducted upon this plan.

As a matter of fact, however, M. Pasteur, with a much wider prescience of the facts and theories of zymotic disease in general, considered that the well-known arrest of development which happens to virulent organisms as a consequence of their growth in tissues or in artificial culture media was due to the production by their metabolic processes of katalyzed substances whose presence was inimical to the active growth of the microbe, and that therefore these substances might be regarded and used as antidotes to the virus. Acting on this assumption, he proceeded to endeavour to protect animals by injecting into them considerable quantities not of the virus, as generally supposed, but of these antidotal substances, or possibly of one alone. Of course, knowing that as a rule attempts to isolate in a state of chemical purity such products usually failed or seriously altered them, and having already ascertained that the baleful influence of the virus could be abrogated or rendered nugatory by the process of drying, he proceeded to employ the simple method of injection emulsions of dried spinal cords. The injection of such emulsions was naturally performed understanding that the drying would probably also impair to a certain extent the protective value of the antidotal substance; to insure, therefore, the introduction into the animal of as large a quantity as possible he considered it advisable to inject emulsions on successive days from spinal cords which had been dried for shorter and shorter periods, till ultimately, having commenced by inoculating from cords which had been dried for fourteen days, and which had been proved to be perfectly innocuous, he arrived at a cord which had been dried for only twenty-four hours. Of course the introduction of such virulent matter as this had no effect on systems already armed by the previous inoculations of antidotal substance. It is greatly to be regretted that owing to general ignorance of this fundamental principle of M. Pasteur's method the many and violent discussions upon his treatment have been rendered absolutely useless. We trust, therefore, we may be excused for having dwelt thus at length upon it.

As soon as M. Pasteur had demonstrated to the French Academy that he could secure the *protection* of dogs by injecting them in the manner above described, he was induced to attempt the *prevention* of the disease in man by similar injections after the bite. This attempt soon developed into a regular practice, from the numbers of patients who flocked to Paris seeking treatment in consequence of the unhappy prevalence of rabies in those European countries which had neglected to provide for its easy extermination by suitable legislation.

From the Report of the Committee commissioned by the Local Government Board to inquire into M. Pasteur's treatment, it would appear, as might have been expected, that a comparatively large proportion of M. Pasteur's patients were bitten by dogs which were not rabid ; but it is also evident from the same Report that when deductions have been made for these cases the death-rate among the remainder was far lower than even the lowest estimate ever formed of the mortality from hydrophobia among persons bitten by reputedly rabid dogs.

While this gratifying result was accomplished, it was at the same time evident that the method was by no means perfected, and the lamented death of Lord Doneraile (from the bite of a tame fox which had been infected by a dog), affords an illustration of this, for the deceased nobleman was subjected to treatment within a few days after receiving the virus. But while the inoculations did not prevent a fatal issue, there seems good reason to believe that they notably modified the distressing features of the malady, for in a brief account before us it is stated that the inability to swallow fluids only appeared twenty-four hours before death, and there was at no time spasm produced by swallowing moist solid food. The same gratifying modification appears to have also been present in another case that recently was observed in St. George's Hospital. Should this modification prove to be general, M. Pasteur will have deprived the malady of its worst tortures.

Before leaving the consideration of this part of the subject it is to be noted that the virulent opposition with which M. Pasteur's efforts in the cause of humanity were met went to the length of charging him with having actually caused the death of some of his patients by his inoculations. This charge, though not supported by any exact evidence whatever, was also inquired into by the above-mentioned Committee appointed by the Local Government Board. Indeed, they had a special opportunity of doing so, for one of the laboratory servants of Mr. Horsley (who carried out the experiments for the Committee), being bitten most severely by a rabid cat, died six weeks later (the usual incubation period of the disease) with the paralytic form of hydrophobia rather than the excitable form. Rabbits inoculated from the spinal cord of this man died with the shortest possible period of incubation. As the Committee point out, this would seem to have lent colour to the idea that the inoculations themselves were fatally virulent, had not similar instances of short incubation periods been occasionally observed to follow inoculation from similarly rabid animals. Stress was also laid upon the mode in which the man was inoculated—namely, by what M. Pasteur called the intensive treatment, and which he adopted in cases of very severe injury. But the whole question was dismissed by M. Pasteur altering this mode of treatment in order that there should not even be the semblance of the possibility of such an accident.

It seems to us that, in England at any rate, there is quite another view to be taken of this question of rabies and its scientific prevention—in fact, that its complete extermination should be ensured in preference to efforts to treat it after it has attacked anyone.

The data upon which legislation should be based are now fortunately at hand. The House of Lords recently appointed a Select Committee, under the chairmanship of Lord Cranbrook, the President of the Privy Council, to inquire into the whole question of the social bearing of the disease, and the means which have been adopted to get rid of it in foreign countries. The Report of that Committee is published, and we have been permitted in addition to inspect the evidence laid before it. This evidence is a most instructive comment upon the manner in which the facts of modern science are sometimes treated as being of only equal value with the most absurd misstatements dictated by charlatanism and abandoned self-interest.

As a whole, however, the Report is one with which we have good reason to be satisfied in many ways, for it recognizes the great value of the simplest means of preventing the spread of the disease, viz. the muzzle.

Those of us who remember the senseless anti-vivisectionist opposition which met the police edict enforcing this salutary measure in London, will not be surprised of course to find in the evidence before the Committee the same thing repeated, but, as was inevitably the case, deprived this time of all its deceptive influence.

For the experience of the working of the muzzle in London, where it brought the number of deaths from hydrophobia down from 27 in 1885 to 0 in the last quarter of 1886, and indeed we believe we may also say the first six months of 1887 ; the experience of its working in Nottingham, where the cases of rabies varied directly in number according to the way in which the muzzle regulations were enforced ; of Prussia, where the disease is almost extinct, being one-fiftieth part of that in Great Britain ; of Scandinavia, where it is absolutely extinct—all disproved the baseless theories and assertions of those who, under the guise of pseudo-zoophilism, endeavour to perpetuate in man and the lower animals the torments of this horrible disease. As will doubtless have been already surmised, the whole of this factious opposition to the above-mentioned beneficent legislation came from the small clique of anti-vivisectionists who were unhappily represented on the Committee itself in two of its members, viz. Lords Mount-Temple and Onslow.

The Lords' recommendation of the muzzle, however, is marred by one defect, and that a very serious one.

Here is a disease, a zymotic disease, the virus of which, as we have just seen, is most difficult of isolation, and evidently easily destroyed by ordinary conditions when it has left living tissues; a disease, too, which is fortunately infrequent compared to many others, and again fortunately one which would become extinct if not kept in existence by transmission from dog to dog; a disease, in short, which needs nothing but the firm administration of the most ordinary rules of preventive medicine to be destroyed utterly,—and yet, in deference to the professional agitation before mentioned, the Lords' Committee only recommend the use of the muzzle when the disease is "prevalent." If this means, and it is capable of being interpreted in two ways, that the Lords' Committee think the muzzle should be applied only when the disease is epidemic, nothing more regrettable can be imagined. To the scientific mind it seems almost incredible that a legislative body should hesitate to grasp the opportunity, the easiest ever offered, of eradicating a disease so painful and utterly incurable when once the symptoms have declared themselves; but here unfortunately is the example we referred to above of scientific fact overridden by vulgar prejudice. For this disease, acknowledged by all who have studied it to be transmitted solely by inoculation of one animal by another, is endemic in Great Britain, is paramount in the manufacturing districts and great cities; and yet the Lords' Report, instead of recommending the universal application of the muzzle, which would abolish the evil from these its centres, is content to leave it to the local authority—Heaven save the mark!—to apply the remedy when the disease has already made sufficient havoc (!) as to call for its suppression. It is sad, too, to see that this view, which we must call narrow, runs through the whole Report, but it is gratifying to find that that Committee, at any rate, fully appreciates the high worth of M. Pasteur's invaluable test of the disease.

In conclusion the Lords say that, should M. Pasteur's method of treating the disease be found of value, provision should be made for its introduction into England. While heartily concurring in this recommendation, we cannot but feel grieved that the necessity for it in England should be permitted to exist; for in this conntry, like Scandinavia, the introduction of the disease can be prevented; so that if proper measures were taken England would enjoy the same complete immunity from it that Sweden does at the present day. With reference to the adoption of M. Pasteur's mode of treatment into this country, a most fundamental difficulty arises at the outset, viz. that we have no public laboratory where investigations of this kind could be carried on for the nation, and that therefore an institution of the kind would have to be established for this and kindred subjects of inquiry. At the present time there is, unfortunately, little hope that this want—which we have before so frequently pointed out is nothing short of a national disgrace—will be adequately met; and, as a matter of fact, questions of this sort are usually decided at the Brown Institution, the nation being thus lamentably dependent upon the assistance of a private charity.

Oddly enough this necessity has just been provided for in France by the institution of a *quasi*-private laboratory—the Pasteur Institute. We are, however, strongly of the opinion that questions of this kind are of an Imperial character, and as such should be dealt with by the central Government in a properly-fitted institution.

Hydrophobia from time immemorial has been the most dreaded of all diseases, and justly; but no doubt this dread has been intensified by ignorance of its causation, an ignorance which, having existed for more than 2000 years, has just been dissipated to an enormous extent by the scientific labours of M. Pasteur. This advance is, of course, a source of vexation to the misanthropic anti-vivisectionists, who are shamefully endeavouring to bolster up the exploded theory of spontaneous generation in order to hamper the efforts of preventive medicine to stamp out the disease, regardless also of the evidence from countries where it has been so rooted out, and where, owing to its importation being prevented, it has never appeared again.

And while we have made this great step forward in our knowledge of the nature and etiology of the disease, we have at the same time learnt, thanks again to M. Pasteur, how to protect animals from its ravages, how to prove or disprove its existence in the absence of clinical or anatomical evidence, and, although this is still *sub judice*, apparently how its fatal effects may be warded off in the human being, and, if not successfully prevented, possibly ameliorated.

Finally, a most satisfactory outcome of this increase in our scientific knowledge is the revelation to us that by the adoption of certain legislative means we may destroy the evil once and for all.

POPULAR BOOKS ON BIRDS.

Ocean Birds. By J. F. Green. With a Preface by A. G. Guillemard, and a Treatise on Skinning Birds, by F. H. H. Guillemard, M.D. With Illustrations by Frances E. Green. 4to, pp. viii.-93. (London: R. H. Porter, 1887.)

Bird Life in England. By Edwin Lester Arnold. 8vo. (London: Chatto and Windus, 1887.)

TWO books deserving the above title are before us. It is well known that some of the most interesting works on ornithology have been written by men who do not profess to be scientific naturalists, but who exhibit an intelligent acquaintance with their subject and also possess a faculty of description the want of which adds so much to the dullness and heaviness of style with many more ambitious writers. Anyone who has made an ocean voyage knows full well that the hours are often apt to hang heavy on the hands of the passengers; and if this is true on board a steamer, it is much more true in the case of a sailing-vessel. Mr. Green therefore has compiled a volume which aims at giving assistance to voyagers in the southern oceans, providing short descriptions of the species of sea-birds most commonly met with; and as the author has travelled much by sea himself, it may be taken for granted that he knows the wants of an ornithological inquirer on board a vessel, and has done his best to supply the information. A "Glossary of Terms," and a chapter on the preparation of bird-skins, have been furnished by the author's friend Dr. Guillemard, whose excellent account of the voyage of the *Marchesa* is one of the most readable of modern books of travel. The work is divided into three parts, the first treating of the petrels, the second of the frigate-birds, gannets, and tropic birds, and the third of the gulls and terns. Mr. Green has given a very correct account of all the best-known species belonging to these groups, and for a second edition he may find a few useful notes on some of his marine friends in the volume published by the Royal Society on the Transit of Venus Expedition to Kerguelen Island. One at least of the notes here published is given by Mr. Green, but only as an extract from our volume of "Aves" in "Cassell's Natural History." The illustrations which accompany the work may be sufficient to identify the various species represented, especially the albatrosses, but they are rather roughly done, and that of the flying petrel is nothing but a caricature. No figures taken from mounted birds are ever satisfactory, and Miss Green's illustrations are no exception to the rule.

Mr. Arnold's little work will rank with any that we know of for pleasant reading, either from a sportsman's or an ornithologist's point of view. Some of the descriptions of game and wild-fowl shooting are exceptionally good, and carry with them a scent of the moor and the sea. Despite an acquaintance with several standard works on birds, the author seems to cling with respect to some of the more pretentious but second-rate books which pass muster as histories of British birds. It is, however, somewhat of a treat to find His Royal and Serene Highness the Prince of Mantua and Montferrat (!) spoken of under his original title of Groom Napier, though we should never call him a "first-class" authority. Many well-known names are wrongly spelt throughout the book, and these shortcomings should be corrected in a subsequent edition, when we should also like to see that Seebohm's excellent "History of British Birds" has come under the author's ken. It is to works on natural history like Mr. Arnold's, where real instruction is conveyed in elegant English, so that the acquisition of knowledge is rendered pleasant and easy, that we owe so much of the interest which has of late years been awakened in scientific pursuits; and we should be captious indeed were we to point out small errors in a book the perusal of which has given us so much enjoyment. Not the least useful feature of the work is a chapter by Mr. Brodie Innes on "Grouse Moors and Deer-Forests."

Should the works under review pass into a second edition, we should be glad to point out to the authors certain emendations which have occurred to us, of too little moment, perhaps, to mention in a review, but which would add somewhat to the finish of the volumes.

R. BOWDLER SHARPE.

OUR BOOK SHELF.

First Lessons in Science; designed for the use of Children. By the Right Rev. J. W. Colenso, D.D. (London: Ridgway, 1887.)

THIS book was written more than a quarter of a century ago for the use of a class of natives of the diocese of Natal, who were learning to read English. Since then the greater part of it has been rewritten in order to adapt it to the necessities of European children. As far as possible the earlier lessons are written in words of one syllable, so that they are well fitted for the use of those for whom they are intended.

The object of the work is to furnish the readers with useful information concerning the things around them, in place of the usual childish stories contained in the first books of English; at the same time presenting only such facts as ought, according to the good Bishop, and we quite agree with him, to be known by everyone. We venture to think that in this respect the native students under Bishop Colenso's care were much better off, having these lessons in their possession, than the boys and girls of our own schools who were learning English at the same time.

By far the greater part of the book is devoted to astronomy, to which subject it forms really an admirable introduction. This of course necessitates the introduction and explanation of many geometrical and optical terms, all of which are put forth in the best possible way. The physical features, and orbital and apparent motions of all the members of our system, including comets and meteorites, are fully considered, as are also the apparent motions of the stars.

The reasons are also given why the observed place of a heavenly body should be corrected for refraction, parallax, aberration, precession, and nutation. Kepler's laws and the law of gravitation also come in for a fair share of attention.

Some of the figures should be brought up to date. We are told that the earth is 96,000,000 miles from the sun, and that between forty and fifty minor planets are known; whereas the distance of the sun is between 92,000,000 and 93,000,000 miles, and no less than 268 minor planets are now on our lists.

It is to be regretted that books of this kind, written in clear, simple language, are not more appreciated by those responsible for the selection of reading-books for our elementary schools.

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. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

On the Constant P in Observations of Terrestrial Magnetism.

IN regard to the quantity P, depending on the distribution of magnetism in a pair of magnets employed for measuring terrestrial horizontal force, for the calculation of which Prof. Harkness, in NATURE for August 18, p. 366, gives a simplified expression, may I be allowed to mention that a yet more simple modification of the ordinary formula has been used in the Greenwich observations since the year 1878, in which, the difference between A and A_1 being small,

$$P = (\log A - \log A_1) \times \frac{r_1^2 r^2}{(r_1^2 - r^2) \text{ modulus}}.$$

When the two distances employed are, as is usual, always the same, the factor becomes a constant, being, for $r = 1\cdot0$ foot and $r_1 = 1\cdot3$ foot, $= 5\cdot64$. The advantage of the form is that as, in the calculation, the logarithms of A and A_1 are first arrived at, their difference multiplied by 5·64 at once gives P. Mention of this is made in the introduction to the Greenwich Magnetical Observations for 1878, and in those for some following years, although omitted from the more recent volumes.

WILLIAM ELLIS.

Royal Observatory, Greenwich, August 24.

The Svastika on English Walls.—The Solar Eclipse of August 19.

I greatly fear that practical builders will be uncourteous enough to smile at Mrs. Murray-Aynsley's idea (NATURE, August 18, p. 364) that the S-shaped iron bars seen on the walls of houses are fire-emblems or survivals of sun-worship. They are common enough in every county of England and elsewhere; in fact, wherever the scamping of jerry builders or the lapse of time has caused walls to give way or bulge outwards. The bolt in the centre is not merely to hang them up, but is the end of a long and strong iron bar passing right through the building and attached to a similar curved brace on the other side, or at any rate fixed to some firm unyielding part of the masonry. The curved shape is simply chosen as that which embraces and gives support to the greatest area of brick or stone surface without the necessity of having a solid, continuous plate.

A propos of sun-worship, it is sad to reflect how much good a little of it might have done in inducing that august but capricious luminary to show himself to the thousands who looked in vain for him on the morning of the recent eclipse. He seems to have shone in splendour in longitudes east of the Urals, where his worshippers abound, but to have hid himself in anger from nearly the whole of unbelieving, scientific Europe.

At Twer, between St. Petersburg and Moscow, where I was myself, the early dawn was beautifully clear; but first a dense ground-mist enveloped us, and then, when enough wind sprang up to clear this away and give us a glimpse of the sun about six-tenths eclipsed, a heavy bank of rain clouds came up and put an end to all hopes of observation. The commencement of totality was pretty well marked by a sudden intense gloom, not, however, greater than (if even as great as) a London fog.

At Berlin placards were extensively posted up a little later in the day stating that "in consequence of the unfavourable weather the eclipse was postponed until the next day." This might have been believed in France or Ireland, but it is harder to take in the Teuton than the Celt. H. G. MADAN.

Eton College.

Large Meteors.

A PEAR-SHAPED fireball, rivalling Venus in brilliancy, passed over Cardigan and Radnor in Wales on August 21 at 11h. 2m. It was observed by Mr. D. Booth at Leeds, and by the writer at Bristol; but the two paths, though likely to be very accurate as regards the *direction* of flight, are somewhat discordant in the beginning and end points. The radiant of the fireball was at $264° + 61°$ in Draco, and agrees with the two following showers:—

1871 August 20–25	... ...	$264° + 64°$	Tupman.
1887 August 14–23	... ...	$264 + 62$	Denning.

The meteor referred to appears to have been observed at Bristol much earlier in its track and when considerably higher in the atmosphere than when noticed at Leeds. The mean of the two places gives a height of 80 miles over a point 6 miles east of Aberystwith to 45 miles above a place 7 miles west of Rhayadergwy. The earth point was near Hay, Herefordshire.

It would be important to hear if this fine meteor was observed at any stations in the Midlands, in Wales, or on the south-east coast of Ireland. As seen from Leeds it passed through *Scutum Sobieski*, and at Bristol close to the star ι *Draconis*.

Another fine meteor about equal to Venus was observed here on August 30 at 14h. 25m. It left a bright streak in its path of 18° from $19° + 27°$ to $5° + 14°$. Radiant at $46° + 43°$ near β Persei. W. F. DENNING.

Bishopston, Bristol, August 31.

Colliery Explosions and Atmospheric Pressure.

THERE are few questions so much in need of a satisfactory solution as the relationship which exists between colliery explosions and changes of atmospheric pressure. Before anything was known of the weight, and variations in the weight, of the air, before the barometer was discovered, miners had learned to connect the state of their working-places with weather changes. The old pits were very shallow, the workings very limited, and the ventilation practically left to take care of itself, so that it is not difficult for us to understand the effect of temperature rather than pressure on the atmosphere of the mine. "Trefoil damp," "pease bloom damp," &c., sufficiently indicate the summer prevalence of the danger; in winter "the damps were scarcely felt or heard of." In the early part of the present century Mr. John Buddle, the Newcastle viewer, having watched his barometer and the mining reports became strongly of opinion that "accidents from fire-damp always occur with a low barometer." Faraday and Lyell's Report on the Haswell disaster of 1844 dwelt upon the importance of officials taking into account the variations of the barometer in the management of mines. Since then numerous public Commissions and private inquirers, English and foreign, have investigated the connexion supposed to exist between the exudation of gas and a *falling* barometer. The earlier decisions may be said to favour Mr. Buddle's opinion, but of late years there appears to be a tendency to declare that the effect of a low or falling barometer has been considerably over-rated—that in reality it has little or no influence. Under whatever conditions of pressure explosions formerly occurred, it is perfectly clear from the experience of recent years that disasters take place, as a rule, when there is an excess and not when there is a deficiency of pressure.

Mr. Dobson's Report to the British Association in 1855 showed from a large, though imperfect, number of observations that up to the year 1854 accidents from fire-damp were most frequent in the summer months June and July, the minimum at the end of January; the results being taken to prove indisputably the general dependence of explosions upon the seasons of the year.

In the papers communicated to the Royal and to the Meteorological Societies between 1872 and 1874 by Messrs. Scott and Galloway, it was however shown from 1369 accidents in twenty consecutive years that the maximum occurred at the end of January, the minimum in the middle of September.

These very different results may be regarded as indicating the great revolution which has taken place not only in the time at which explosions occur, but also in the conditions of mining operations. Pits are now of enormous depths, with most extensive galleries, and the ventilating appliances are of the most elaborate description. Possibly these changes have modified very greatly the effect of weather variations. It must be remembered that gas exists in mines under two quite distinct conditions, that in the goaves and waste places being free and in direct contact with the air, while the gas occluded in the solid coal or imprisoned in faults is not in direct contact with the atmosphere. In the former case it is generally agreed that the accumulations of gas expand or contract with the changes of atmospheric pressure. In the latter case we know that the gas exists in the coal at a pressure of many atmospheres, so that it is highly improbable that it is affected directly by the rise and fall of the barometer. Indirectly, however, it would seem that a very important effect results, but in direct opposition to the idea that it escapes only with a falling barometer.

Serious explosions are almost exclusively confined to deep mines, where the management is perfect, and where every care is taken to insure safety. Mystery surrounds each disaster, and it is left to individuals to trace them to coal-dust, gas, or some other favourite theory. Fortunately the illiterate manager has given way to a different order of men, and from the interest taken by mining engineers there is reason to believe that much of the uncertainty which at present envelops the question will be removed before long. Barometers are now common to all mines, and they are studied with more or less interest by the officials. For years past it has become clear to them that there is no apparent connexion between the escape of gas and a falling barometer: the firemen "in ordinary cases can forestall the barometer by from twelve to twenty-four hours." This conclusion, based upon the ordinary observations of officials during their daily routine of duty, has been confirmed by more precise and carefully-planned systems of collecting information.

Following the Seaham disaster of September 1880 (when the centre of an anticyclone was over the northern counties), Mr. Corbett arranged hourly observations, day and night, for several months, showing the atmospheric pressure, the measurements of gas which had escaped into the workings, and by means of water-gauges the movements of the gas in parts of the workings sealed from contact with the air. The water-gauges indicated an out-bye pressure as much as 33, 35, 41, and 48 hours before the barometers began to fall, while gas in measurable quantities was to be found many hours before the mercury gave signs of falling. On the Continent somewhat similar observations have been made at Saarbrück and Karwin. The Austrian inquiry showed that "where after a rapid rise of the barometer it continued to rise slightly, or remained stationary for some time at its maximum, a gradual increase in the volume of gas in the air would set in; or if, after a rapid fall in the barometer, it continued to fall gradually, or remained stationary at its lowest point, a decrease in the quantity of gas would become apparent."

Evidently, therefore, from these researches the greatest danger is not, as a rule, to be apprehended when the barometer is low or falling, and this is supported by actual disasters, the majority taking place under anticyclonic conditions of pressure. While Mardy, Pendlebury, Penygraig, Seaham, and many others add to the verdict, it will suffice to deal with some explosions of the present year, and see if they do not bring home to us a new view of the natural forces at work far down below the surface of the earth.

From the simultaneous observations made at 6 p.m. on Friday, February 18, the Meteorological Office reported:—"The barometer is now rising in all parts of the United Kingdom, and an anticyclone is apparently advancing from the westward." An hour later thirty-nine lives were lost in an explosion in the Rhondda Valley. The anticyclone continued on its course to the Continent, and by the morning of Wednesday, February 23, when so much damage was wrought by the earthquake, the centre had reached Southern Europe. On March 1 the anticyclone was a little further north, and over the neighbourhood of the Chatelus Mine, near St. Etienne, where ninety lives were sacrificed. Still moving northward, the night of March 4–5 found the highest barometer readings over Belgium and the Netherlands, when 144 miners perished at Quaregnon, near Mons.

In the last week of May another anticyclone moving from south to north was marked by the loss of one life at Darcy Lever on the 25th, three lives near Wigan on the 26th, and seventy lives at Udston, near Glasgow, on the 28th.

An anticyclone over Western Germany on the night of June 7–8 marked about sixty deaths at Gelsenkirchen. As this area moved to the westward, a slight earthquake was felt near Strasburg on the 11th, and a severe one in La Vendée on the 15th.

Clearly Mr. Buddle's strong opinion is not applicable to the second half of the century. The knowledge that gas is found escaping with a rising barometer, and that so many explosions take place as indicated, has led mining officials to blame the mercury for not falling even before the gas begins to escape, their idea being that pressure has actually decreased, but that barometers are many hours before taking up the changes. The idea may be dismissed as an erroneous one. The cause must be sought for in another direction, not the direct action of variations of atmospheric pressure on the gas as it leaves the coal, but the effect on the earth's crust and indirectly on the occluded gas. Whatever be the true cause of earthquakes, there seems to be no reason to doubt that fluctuations of atmospheric pressure cause undulations of the earth's crust. Prof. Darwin, taking a probable estimate for the elasticity of rocks, has calculated that with a range of two inches of the barometer we are at least three or four inches nearer the earth's centre when the instrument stands very high than when it is very low, and concludes: "It may be that the incessant straining and unstraining of the earth's surface is partly the cause of earth-tremors, and we can at least understand that these strains may well play the part of the trigger for precipitating the explosion of the internal seismic forces." The seismological records of Japan show that earthquake shocks are twice as numerous under the predominant anticyclone of the winter months, as they are in the summer with lower pressure. As a result of the discussion of earthquakes in Jamaica, Mr. Maxwell Hall concludes that "at the time of an average earthquake shock the barometer is a little above its average height. This is due to the circumstance that the winter months, December, January, and February, when the barometer is above its monthly average, are more liable to shocks than other months of the year; and that the hours from 8 p.m. to 2 a.m., when the barometer is above its diurnal average, are similarly more liable to shocks than other hours of the day." Explosions of fire-damp follow a similar rule; they are most numerous in the winter months, when the range of pressure is greatest, and usually when the barometer is very high. Allowing for the flexure of the earth's surface, we can conceive that with the downward movement under increasing pressure the pent-up gases are forced into the workings of our deep mines; it may be indeed these movements cause infinitesimal fissures in the coal-seams through which the gas passes into the workings at a time when it has been customary to believe there was least danger. There is some degree of probability in this from the fact, so frequently noted in great explosions, that there is a suddenness in the appearance of the gas which is not a common experience in shallow workings.

Taking into consideration all the recorded facts, they point to the conclusion that far greater weight should be attached to a period of high atmospheric pressure than has hitherto been deemed necessary. In any future discussion of this important subject it is to be hoped further evidence will be forthcoming, and that instead of endeavouring to connect every disaster with a low barometer, the distribution of pressure as a whole be taken into account.

The influence of coal-dust upon explosions has not been touched upon, but it may be remarked that the dry atmosphere of an anticyclone renders the dust more inflammable than the dampness of a low-pressure system, so that there is a double reason for giving closer attention to mines under anticyclonic conditions.

HY. HARRIES.

MEASUREMENT OF SPECIFIC HEAT.

HAVING regard to the comparatively large experimental error introduced by thermometers into specific heat measurements, a null method appeared to be desirable. The following method occurred to me about two months ago, but not having access to a physical laboratory, I have not been able to practically test its accuracy.

Two exactly similar calorimeters (A and B) are taken, each containing a coil of thin Pt wire of resistance R, so arranged as to be completely immersed in the liquid. A contains a mass, M (including water equivalent), of water; B the same mass of substance the specific heat of which is being measured. The wires are arranged in bridge fashion, so that the ratio of the currents flowing through the two wires may be made to take any value.

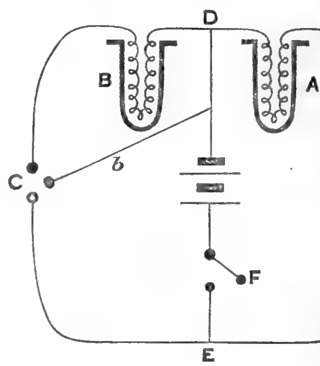


FIG. 1.

A differential thermometer (not indicated in the sketch, for sake of clearness) shows the least difference of temperature between A and B. Probably the most delicate and convenient arrangement is to use two thin Pt wires balanced in the arms of a bridge, using a very sensitive galvanometer.

First consider the calorimeter B containing the substance. It receives a quantity of heat, H, from a current, C, flowing through a resistance, R, for a time, t. Hence

$$H = \frac{C^2Rt}{J} = \theta MS$$

(where θ is the rise of temperature, and S the mean specific heat for that interval).

Similarly in A, containing water,

$$H^1 = \frac{C_1^2Rt}{J} = \theta M.$$

For by the conditions obtained θ is the same in both A and B. Hence $S = \left(\frac{C}{C_1}\right)^2 = \left(\frac{r_1}{r}\right)^2$, where r_1 and r are the resistances of the two circuits. It is obviously unnecessary to make the resistances, and the masses of liquids, equal, but the equation is thus simplified. If a smaller mass of water, m, be taken, then $S = \frac{m}{M} \cdot \left(\frac{r_1}{r}\right)^2$, thus increasing the delicacy of the method.

Since in the adjustments a considerable amount of time would be necessary to allow the calorimeters to attain thermal equilibrium after each trial, the following modification may prove more simple and more practical:—

The calorimeter B is arranged so that by a switch-key, C, the current can be diverted through a wire of exactly equal resistance, b, so that the current is the same by either path. The resistance from D to E is the same either way. The key F is pressed down for a time, t,

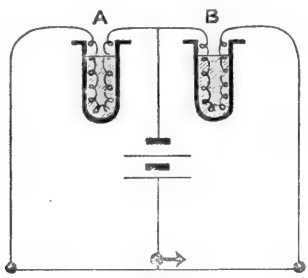


Fig. 2.

until the needle is largely deflected; then the current is switched from B and passed through A alone, until the needle is just brought back to zero, in total time, T. Then, neglecting for the present the slight error due to cooling,

$$\text{in A} \ldots \theta M = \frac{C^2RT}{J}, \text{in B} \ldots \theta MS = \frac{C^2Rt}{J}, \therefore S = \frac{t}{T}.$$

Since T and t can both be made large, this should give very accurate results. It is evidently especially applicable to the measurement of the rate of increase of specific heat with temperature, since the liquids may have any initial temperature.

In conclusion, I may say that I should not have published this method in such an incomplete state, and unsupported by experiment, but I noticed to-day (September 5) that Profs. Stroud and Gee intend to read a paper before the British Association on "A Null Method in Electro-Calorimetry," and it is possible this may refer to a similar method. GEORGE N. HUNTLY.

THE HESSIAN FLY.

I AM sorry to say that reports from correspondents acquainted with the attack of the Hessian fly show its presence now in an almost continuous line along the northern and eastern coast from Cromarty on the Moray Firth in Scotland down to Kent.

I have this morning received specimens of the *puparia* from the parish of Urquhart, in Morayshire, the most northerly locality from which I have at present received the so-called "flax-seeds."

The amount of presence varies very much. In the locality above mentioned (that is, the district from Aberdeen to Cromarty), the traces of attack are reported as to be found from 25 to 30 miles inland, but the injury slight, not more than one straw in fifty being affected, and the grain of fair quality. It is severe in some parts of Perthshire, and is found also in the eastern counties adjacent.

In East Lothian, Haddington, and Berwickshire attack is only reported from a few places at present, and in Northumberland from one locality.

Beginning again on the two sides of the Humber the attack widens much in area as it is traced south. It passes through Lincolnshire and Cambridgeshire, touching an easterly part of Northamptonshire, till it extends over the district commonly known as the eastern counties, including besides great attack in Hertfordshire, and some in Bedfordshire; and it also occurs in Kent.

In the southerly or westerly parts of England it occurs at Lymington and Petersfield in Hampshire, and to a considerable extent near the College of Agriculture, Downton near Salisbury; and I have one report of it from near Bridgwater, and it also occurs at Goring Heath, Oxfordshire.

The above localities are where I know of its presence from specimens sent to myself, or, in a few cases, from information given me by correspondents whom I know to be acquainted with the appearance of the *puparium*, and the characteristics of the attack.

It very likely may occur elsewhere, but I am only just giving a general sketch of extent of infested area from personal knowledge.

It strikes me as a very curious point that the attack should so markedly cling to the sea-side, excepting in a few isolated instances, or where the inland area is continuous with the sea-side district.

It is very satisfactory to observe that although the season has been so altogether *extraordinarily* favourable to various kinds of insects affecting corn-stems, yet that in very many instances reported to me the injury caused to wheat by Hessian fly has been slight

On this fact I venture to think we may ground a hope that, either from the varieties of wheat which we use being kinds suited to do what is called "resist" attack, or from circumstances of our cultivation, we may find that our wheat at least does not suffer as much as in some other countries.

Also the enormous prevalence of the two stem attacks caused respectively by the corn sawfly (*Cephus pygmæus*), and by the dipterous fly, the *Chlorops tæniopus* (attacks which far exceed in amount any which have been brought under my notice as caused by these insects), give a hope that the climatal circumstances which usually prevail here will have an effect in checking the attack of the *Cecidomyia destructor*, as well as the above-named crop pests, as we see that all three kinds have been exceptionally thriving in the exceptional heat and drought.

It is unnecessary to point out to your highly informed and thinking readers that the statements now appearing of the *Cecidomyia destructor* having been a corn pest in this country for many years have not the slightest foundation. ELEANOR A. ORMEROD.

THE BRITISH ASSOCIATION.

MANCHESTER, *Tuesday Evening.*

ABOUT the success of the Manchester meeting there seems to be only one opinion. In mere numbers—the most popular gauge of success—it has by several hundreds surpassed all former meetings; the number of tickets sold very closely approaches 4000. As a natural result, the amount of money collected and available for the purposes of research is unprecedentedly great, as will be seen by the list of grants which have been allotted to the various Committees. The great increase in attendance over all former years is to a considerable extent due to the large number of foreign visitors, who have formed a marked and prominent feature of the present meeting. In the proceedings of nearly every Section the representatives of foreign science have taken an active part, with the result that the time of the whole meeting has been more intensely scientific than in the case of any previous meeting. This has been especially shown in the case of the important discussions which had been arranged for, and which most of them bore the character of real debate; the only exception, we believe, being the case of electrolysis, in Sections A and B, the "discussion" consisting mostly of the reading of a series of papers. Quite otherwise, however, was it with the discussions on heredity, introduced by Prof. Lankester, and on the cell theory, introduced by Prof. Schäfer, in Section A—discussions in which the subjects were threshed out very thoroughly. To some extent it is generally conceded that the great mixture of foreigners has to some extent solved the problem of an International Scientific Congress, which in any formal way is generally considered impracticable. Their presence here has certainly added a stimulating variety to the meeting, and the honour has been duly appreciated by the Corporation and citizens of Manchester. The foreigners have all been hospitably entertained as guests, and there have been not a few special entertainments got up for their special behoof. At the great dinner to be given to-morrow by the Mayor and Corporation nearly half of the guests will be foreigners. One of the pleasantest gatherings of the meeting was at a little dinner given on Sunday night by a few of the biologists to a select few of their foreign co-workers, especially botanists, at which De Bary delighted everybody present.

The number of papers read at this meeting has been quite comparable with its other exceptional features. Sections that have never split before have been compelled to split now. Biology, though it has thrown off Section H, has this year split into two sub-sections,—Botany and Physiology,—and there is even some fear, perhaps hope, that these divisions may become permanent. On Saturday every Section met except E, and to-morrow the majority will have to sit close up to the General Committee-meeting. Out of all this rush of papers no doubt some good comes, but most of those interested in the welfare of the Association admit that it would be well to moderate it, or perhaps still more completely to organize it. For one thing the custom of reading one paper in several Sections is greatly to be deprecated, and this year it has been carried still further than ever, greatly to the indignation of those Sections which had to submit to hearing the story retold. This came to a crisis in Section E, where an eminent geologist, who condescended to read a paper to some extent already given to his own Section, was told in almost so many words that Section E had no time to listen to geological lectures. Here indeed the battle between the geologists and geographers was fought out, greatly it was thought to the discomfiture of the former, who are loth to think that there is anything worthy of the name of geography outside of their own lines.

In spite of the persistently unfavourable weather, the public lectures have been quite successful. The lecture on the rate of explosions by Prof. H. B. Dixon kept a large audience intensely interested from beginning to end; and nothing could be more striking and instructive than his experiments, some of which were on a gigantic scale. Equally attractive was the lecture to working men on Saturday evening on electricity by Prof. George Forbes. The biggest audience of any, however, assembled in the Free Trade Hall on Monday evening to listen to Sir Francis De Winton's lecture on exploration in Central Africa. The audience was evidently a popular one, and the lecturer had the warmest reception. Unfortunately, the lantern used to show maps and pictures on the screen was rather a failure. Distinctly popular as it was, probably even the specialists were glad to get a convenient summary of recent work in Central Africa, pleasantly conveyed.

The address of the President of the Association, Sir Henry Roscoe, was very numerously attended. Sir Henry was evidently audible all over the place, and his reception, as might have been expected, was enthusiastic.

Manchester is rather badly off for excursion places, and on Saturday, we believe, quite as many people spent the day in Manchester as elsewhere. Indeed, most of the Sections were so busy with work that they had no time to think of play. The little dredging excursion was joined in by about fifty men, who all seemed highly satisfied with the results, in spite of the weather. One of the most popular of the coming excursions will be that to the Isle of Man from Friday to Tuesday, under the guidance of Prof. Boyd Dawkins. There is also some talk of an excursion to the Lakes, but the weather does not encourage holiday enterprise in so notoriously rainy a region.

One popular and distinctly useful feature in connexion with the present meeting has been the Anthropometric Laboratory which has been established in connexion with Section H, under the care of Dr. Garson and Mr. Bloxam. It has been very largely frequented by the members of the Association, who have had themselves weighed, measured, and tested in a variety of ways. The object, we believe, is to obtain data from the most cultured classes to compare with those collected by Mr. Francis Galton, at South Kensington, from all and sundry. The result, it is expected, will be highly interesting.

We have already referred to the very varied exhibition which has been arranged in the galleries around the attractive reading-room. This has received various additions during the week. Prof. Boyd Dawkins shows some very instructive exhibits in various rooms belonging to the Geological Department of Owens College, including William Smith's first geological map and an autograph letter. Another exhibit deserving mention is the collection of wax models, illustrating vertebrate morphology and embryology, shown by Prof. His, of Leipzig, on behalf of Dr. A. Ziegler, of Freiburg. Naturally the model of the Manchester Ship Canal has attracted much attention, but not more than the great variety of interesting anthropological exhibits, which include the collection of casts and photographs from Egyptian monuments contributed by Mr. Flinders Petrie.

Great complaints have been made of the way in which the Press has reported the proceedings of the meeting. This may partly be due, no doubt, to the fact that Parliament is still sitting and takes up much of the space of the papers; but is also to be ascribed to a larger extent to the fact that ordinary reporters are hardly equal to the work of the British Association. When there are printed abstracts the matter is simple enough, but when discussions have to be reported the failure is almost absolute. This is certainly to be regretted, as it could not but be of the greatest service to have such discussions widely circulated. Surely it is quite worth while for the Sections to organize adequate reporting arrangements for their own sake.

The next meeting, at Bath, will be presided over by Sir Frederick Bramwell. In 1889 the Association will meet in Newcastle, and it is expected that an invitation will come from Leeds for 1890.

The following is the list of grants which have been made this year by the Association:—

A.—*Mathematics and Physics.*

Ben Nevis Observatory	£150
Electrical Standards	80
Magnetic Observations	15
Standards of Light	100
Electrolysis	50
Solar Radiation	10
Differential Gravity Meter	10
Uniform Nomenclature in Mechanics	10

B.—*Chemistry.*

Silent Discharge	10
Properties of Solutions	25
Recording Water Analysis Results	10
Influence of Silicon on Steel	20
Methods for Teaching Chemistry	10
Isomeric Naphthalene Derivatives	25
Action of Light on Hydro-acids	20

C.—*Geology.*

Sea Beach near Bridlington	20
Geological Record	50
"Manure" Gravels of Wexford	10
Erosion of Sea Coasts	15
Erratic Blocks	10
Underground Waters	5
Palæontographical Society	50
Volcanic Phenomena of Japan	50
Pliocene Fauna of St. Erth	50
Carboniferous Flora of Lancashire and West Yorkshire	25
Volcanic Phenomena of Vesuvius	20

D.—*Biology.*

Zoology and Botany of the West Indies	100
Flora of Bahamas	100
Development of Fishes, St. Andrews	100
Marine Laboratory, Plymouth	100
Migration of Birds	30
Flora of China	75
Naples Zoological Station	100
Physiology of the Lymphatic System	25
Marine Station at Granton	50
Peradenyia Botanical Station	50
Development of Teleostei	15

E.—*Geography.*

Depth of Frozen Soil	5

F.—*Economic Science and Statistics.*

Precious Metals in Circulation	20
Value of the Monetary Standard	10

G.—*Mechanics.*

Investigation of Estuaries by Models	200

H.—*Anthropology.*

Effect of Occupations on Development	25
North-Western Tribes of Canada	100
Prehistoric Race in the Greek Islands	20
Anthropological Notes and Queries	50
Total	£2025

Captain Sir Douglas Galton (one of the general secretaries) submitted the following report of the Council to the General Committee at the meeting held on the 31st ult. :—

The Council have received reports during the past year from the general treasurer, and his account for the year will be laid before the General Committee this day. Since the meeting at Birmingham the following have been elected corresponding members of the Association: Dr. Finsch, Dr. O. W. Huntington, Dr. A. König, Lieut. R. Kund, Prof. Leeds, Prof. H. Carvill Lewis, Prof. John Trowbridge. The Council have nominated Mr. Oliver Heywood a vice-president of the meeting at Manchester. An invitation for the year 1889 will be presented from Newcastle-upon-Tyne; but the invitations from Melbourne and Sydney have been withdrawn. The following resolutions were referred by the General Committee to the Council for consideration, and action if desirable:—(*a*) "That the Council be requested to consider the question of rendering the reports and other papers communicated to the Association more readily accessible to the members and others by issuing a limited number of them in separate form or in associated parts, in advance of the annual volume." The Council, after careful consideration of the question, are of opinion that a certain number of copies of the more important reports presented to the Sections of the Association should be kept in stock and sold separately, the number of copies printed and the price of each report to be fixed by the secretaries after communication with the officers of the several Sections. (*b*) "That the Council be requested to consider the advisability of selling publicly the presidential addresses." The Council have considered the question, and are of opinion that it is desirable that printed copies of the addresses of the president and the presidents of Sections should be stitched together and sold; that a number of copies, not exceeding 1000, should be printed; and that these should be placed on sale, at the price of one shilling, through agents or otherwise, as may be considered most suitable. (*c*) "That the Council be requested to consider the advisability of calling the attention of the proprietor of Stonehenge to the danger in which several of the stones are at the present time from the burrowing of rabbits, and also to the desirability of removing the wooden props which support the horizontal stone of one of the trilithons; and in view of the great value of Stonehenge as an ancient national monument, to express the hope of the Association that some steps will be taken to remedy these sources of danger to the stones." The Council have carefully considered the question, and having had the advantage of perusing the detailed report recently prepared by a deputation of the Wilts Archæological and Natural History Society on the condition of the whole of the stones constituting Stonehenge, are of opinion that the proprietor should be approached with the expression of a hope that he will direct such steps to be taken as shall effectually prevent further damage. (*d*) "That the Council be requested to consider whether a memorial should be presented to Her Majesty's Government, urging them to undertake and supervise agricultural experiments, and to procure further and more complete agricultural statistics." The Council have considered the question, and are not prepared to memorialize the Government on the subject. The question of the re-arrangement of the journal has been brought before the Council by Mr. J. B. Martin, and, after careful consideration, the Council are of opinion that it is unnecessary to print in each number of the journal the list of the papers read on the previous day; also that it would be well to place the list of officers of each Section at the head of the list of papers to be read in that Section. The Council wish to obtain the sanction of the General Committee to these alterations. The Council having considered a letter addressed to them by Mr. R. H. Scott, are of opinion that it should be an instruction to the secretaries of all committees, other than committees of Sections, to send notices of all meetings to each member of a committee, and that the draft report of the committee should first be sent in proof to each member, and then submitted to a meeting of the committee specially called for the purpose. The Corresponding Societies Committee, consisting of Mr. F. Galton (chairman), Prof. A. W. Williamson, Sir Douglas Galton, Prof. Boyd Dawkins, Sir Rawson Rawson, Dr. J. G. Garson, Dr. J. Evans, Mr. J. Hopkinson, Prof. R. Meldola (secretary), Mr. W. Whitaker, Mr. G. J. Symons, and General Pitt-Rivers, having by an oversight not been re-appointed at Birmingham last year, the Council have requested these gentlemen to continue the work of their committee, and now nominate them for re-election, with the addition of the names of Mr. W. Topley, Mr. H. G. Fordham, and Mr. William White. In accordance with the regulations the five retiring members of the Council will be Mr. W. Pengelly, Sir Richard Temple, Dr. De La Rue, Sir F. J. Bramwell, and Mr. J. C. Hawkshaw.

The Council recommend the re-election of the other ordinary members of the Council, with the addition of the gentlemen whose names are distinguished by an asterisk in the following list:—Capt. W. de W. Abney, F.R.S., Sir R. S. Ball, F.R.S., W. H. Barlow, F.R.S., W. T. Blanford, F.R.S., W. Crookes, F.R.S., Prof. G. H. Darwin, F.R.S., Prof. W. Boyd Dawkins, F.R.S., Prof. J. Dewar, F.R.S., *Sir James Douglass, F.R.S., Prof. W. H. Flower, F.R.S., Dr. J. H. Gladstone, F.R.S., Lieutenant-Colonel H. H. Godwin-Austen, F.R.S., Prof. O. Henrici, F.R.S., Prof. J. W. Judd, F.R.S., J. B. Martin, F.S.S., Prof. H. McLeod, F.R.S., Prof. H. N. Moseley, F.R.S., Admiral Sir E. Ommanney, C.B., F.R.S., Prof. W. C. Roberts-Austen, F.R.S., *Prof. Schuster, F.R.S., *Prof. H. Sidgwick, *Prof. Schäfer, F.R.S., W. T. Thiselton-Dyer, C.M.G., F.R.S., Prof. T. E. Thorpe, F.R.S., *H. Woodward, F.R.S.

Sir H. E. Roscoe, M.P., moved, and Sir R. W. Rawson seconded, the adoption of the report. The motion was adopted.

The Chairman submitted the treasurer's report, which stated that the receipts for the past year were £5081 6*s.* 3*d.*, including a balance of £1869 brought forward at the Birmingham meeting. The disbursements included the sum of £1186 18*s.* in respect of grants in aid of scientific research. The balance in hand was £1718. The report was adopted.

SECTION B.

CHEMICAL SCIENCE.

Opening Address by Edward Schunck, Ph.D., F.R.S., F.C.S., President of the Section.

It is, I can assure you, with a feeling of extreme diffidence that I take the chair to-day as President of the Chemical Section at this meeting of the British Association. When I look round me and see the many distinguished men who are prepared to take part in our proceedings I cannot but very strongly feel that the Council's choice might have fallen on a worthier representative of chemical science than myself. Having in the course of my career bestowed more time and attention to technical matters than to purely scientific subjects, and having moreover arrived at a time of life when active participation in work of any kind must necessarily be drawing to a close, you must not expect from me the accurate knowledge of the present state of chemical science and the questions that are at this moment presenting themselves for solution such as would naturally be required from anyone occupying the post which I have on this occasion the honour to hold. The marvellously rapid progress of chemistry during the last twenty years has made it difficult for the most industrious cultivator of the science to keep abreast of the knowledge of the day, and for a *dilettante* like myself one may say it is next to impossible. I confess myself painfully conscious of my defects in this respect, and I shall therefore have to claim the indulgence of the Section should questions arise on which I am unable to speak with authority, or even to discuss with advantage.

Considering, however, how efficiently I am supported by the gentlemen with whom I have the honour to be associated, and to whom I am sure in any case of difficulty I may appeal for assistance, I trust to be able to perform the duties of my office without discredit. I will not, however, trouble you with merely personal questions, which are always more or less tedious, but proceed with the few remarks which I wish to make, and which, if not new or instructive, may perhaps serve to entertain you during the time usually devoted to addresses of this kind.

I think you will hardly expect me, even were I fully competent to do so, to review the progress of chemistry during the last half-century, for the time at my disposal would be too short and the result at my hands, I fear, unsatisfactory. I shall prefer to call attention in a few words to the chemistry of other days as I knew it, and the chemistry of the present time as known to us all, and to point out what I consider to be the chief characteristics of each. I shall then, with your permission, point out a few of the directions in which, in my opinion, the chemistry of the future will probably be developed, and in this undertaking I shall perhaps be more successful than in the other; for to discuss the history of science requires exact knowledge; but in speculating on its future the imagination comes into play, and to imagine is easier than to describe.

When I first entered on my studies, exactly fifty years ago, chemistry could hardly be called a science—it was rather a collection of isolated facts unconnected by any consistent theory covering the whole field. Most of the important elements were known, but of the exact proportions in which they combine together we were ignorant. The law of definite proportion had been generally accepted, but so imperfect were the data then at our disposal that we may say the law was rather taken for granted than proved. The atomic theory of Dalton as explaining this law had also been adopted by chemists; but it is not unlikely that this theory, then in its infancy, might by the vigorous onslaught of a man of Berthollet's acumen have been upset, and we should then have been left entirely without a guide through the bewildering labyrinth of facts. Of any connexion between chemistry and physics there was in those days no question; of any but the most superficial notions regarding the effects of heat, light, and electricity on chemical substances we had no conception. The idea that chemistry could have any bearing on or connexion with physiology or pathology would have been ridiculed as absurd. I cannot think of the then state of organic chemistry without feeling amused. The state of this branch of chemistry could hardly perhaps be called chaotic or rudimentary, for, after all, what had been done had been well done and neatly done, but the assemblage of facts of which it consisted was devoid of systematic arrangement; it resembled a cabinet of curiosities, the components of which stand in no recognizable relation to one another, or a miscellaneous collection of books placed in an orderly manner on shelves, but without any attempt at classification. As to the genesis of organic compounds, what would now be called absurd notions prevailed. I distinctly remember eminent chemists maintaining that no strictly speaking organic body, even of the simplest constitution, could possibly be formed without the intervention of the so-called vital force. The fact, then recently discovered by Wöhler, of the artificial formation of urea from inorganic substances, was considered as something almost miraculous—*i.e.* as a phenomenon the like of which would never again recur. Without, however, entering into further details, I think I may, without fear of contradiction, assert that the main distinction between the chemistry of fifty years ago and the chemistry of the present day consists in this, that whereas formerly the science dealt chiefly with qualitative reactions, it now occupies itself principally with quantitative determinations. To have established the fact that every chemical phenomenon may be represented in figures, denoting either number, measure, or weight, such figures, when once accurately determined, remaining constant and unchanged through all time—this seems to me the crowning glory of modern chemistry. It is the firm establishment of this principle that has transformed the face of chemistry and has changed it from a mere descriptive into an exact science.

In justice to our predecessors it should, however, be remembered that this principle, though more fully developed in our own day, was not for the first time set up in quite recent times. The labours of Dalton, conducted on quantitative lines, were performed in this city of Manchester in the early part of this century. At the same time Berzelius was engaged in analyzing the most important inorganic compounds and establishing the fact, not previously recognized, strange as it may now appear, that every well-defined substance has a definite chemical composition. But going still further back, we come to the alchemists. Now alchemy, if it has any logical basis at all, is founded on quantitative notions as regards matter. All metals, the alchemists said, consist of sulphur, salt, and mercury (these terms signifying not so much elements in the modern sense as qualities) in various proportions; hence their convertibility. Take copper, remove from it a certain proportion of its sulphurous constituent, and add more of the mercurial, and you have silver; repeat the process with silver, and gold results. At the time of which I speak, though much important analytical work had been done by Berzelius, Rose, and others in organic chemistry, though the veteran Chevreul had led the way in placing organic chemistry on a quantitative basis, and the composition of the most important organic compounds—thanks to the labours of Liebig and his method of organic analysis—had been ascertained, still quantitative determinations were not considered of such paramount importance as at present. In fact, scientific thought did not run in that direction, but satisfied itself, for the most part, with the study of qualitative reactions. It was still possible to see memoirs by eminent chemists containing not a single quantitative determination. Strange as it may seem, two able chemists, Boettger and Schoenbein, were living until quite recently who worked and obtained valuable results without resorting to the balance, the instrument which of all others seems the most indispensable to the chemist of to-day. The balance was indeed universally employed in my younger days, but no other instrument, properly so called, was ever seen in the laboratory. The spectroscope was not yet invented; the polariscope had not come into use; volumetric analysis was still in its infancy. Even the thermometer was but seldom used. What a different picture does the laboratory of the present day present, with its instruments of precision and its various appliances for effecting quantitative determinations of all kinds!

Whether the universal prevalence of, and exclusive attention to, quantitative methods in chemistry has been an unmixed good may be doubted. Who has not run with a weary eye over the long array of figures, the never-ending tables of which some modern memoirs seem to consist, and not longed for some mere description—were it only regarding trivial matters—to relieve the monotony and fix the subject treated of on the memory? That quantitative determinations given in quite precise terms may occasionally be entirely futile may be seen on referring to the history of alchemy. One of the later alchemists professes to have converted 5400 parts by weight of copper into 6552 parts of silver by the action of 1 part of a metal-improving substance—philosopher's stone (Kopp, "Die Alchemie"). Here we see the quantitative method applied to a purely chimerical process, elaborated from the depths of the experimenter's inner consciousness, and of no value whatever. Much of what is at the present day carefully worked out and presented to the world in numerical form may, like this statement of the alchemist, pass away and be forgotten. This may possibly be the case with the numerous carefully-made analyses of water which we now meet with, and which we would gladly exchange for a few decided qualitative tests of its hygienic properties. In the case of air and water it is not the minuteness of the noxious matter which causes doubts to arise, but the absence of any decided and undoubted chemical characteristics of the impurities present. It is probable that a refined sense of taste, uncorrupted by the luxurious indulgences which civilization has introduced, would be able to detect differences in drinking-water which might escape the attention of the most consummate analyst.

Whatever objections may, however, be entertained to the application of quantitative methods in natural science, to the exclusion of others, it is certain that important results have flowed from their adoption, insomuch that we seem to have arrived at the conclusion that the expression of quantitative results is the be-all and end-all of science; that all differences are merely quantitative; that there is no such thing as mere quality. The whole philosophy of our age is expressed in this one proposition: All differences within the sphere of our experience are quantitative. It is the basis of Darwinism, if I am not mistaken, and underlies many of our political and social theories. Of course it is a mere assumption if stated generally, for the phenomena that admit of purely quantitative expression are few in number compared with those that do not; but then it is surmised, and with some degree of probability, that the vast region outside the quantitative sphere will in time come to be included within it. The past history of science seems to render this likely in the future. The science of chemistry has so far, however, presented an insuperable barrier to the general adoption of this view, and will continue to do so as long as the so-called elements remain what we now admit them to be—indestructible, immutable, inconvertible. It is possible to denote all the known properties of gold and silver, their atomic weight, specific gravity, hardness, malleability, action towards heat, light, and electricity, in precise numbers with reference in each case to a certain standard; and yet we cannot say that silver minus a little of this, plus a little of that, constitutes gold—the two elements are essentially and radically distinct. Unless we admit with the alchemists that by taking away a little of A and adding a little of B we can convert one metal into another, one element into another, the quantitative method must fall short of its complete development in chemistry. Numerous attempts have, therefore, been made to show the theoretical probability, even if it should not be possible to prove it experimentally, of the so-called elements being really compound bodies, or at least of their containing a basic matter common to all. My predecessor in this chair has endeavoured to show, in the brilliant address delivered to this Section on the occasion of its last meeting, that the barrier hitherto presented to us by the intractability of our present elements may be overcome, and has adduced experimental illustrations in favour of his views of the compound nature of the elements. Mr. Crookes has called to his aid the doctrine of evolution, which has proved so valuable an instrument in the hands of the biologist, maintaining that the elements, like the species of plants and animals, were gradually evolved by some process of condensation from a primordial matter called by him protyle, each step in the process being represented by a distinct element. This is doubtless taking very safe ground, for if the process of evolution was the same in the inorganic as it is supposed to have been in the organic world, the process can never be repeated, and we shall, therefore, never be in a position to illustrate it experimentally. I may, however, have misunderstood what Mr. Crookes meant to convey, and, if so, must apologize for the dulness of my apprehension. Granting, however, the possibility of our resolving our present elements, were it in theory only, into modifications of one basic material out of which they have been evolved, the question would still remain to be answered, What has caused this primordial matter to be split up into groups and forms having distinct and opposite qualities? and when this question is answered, if it can be answered even in a problematical way, then other questions would arise, until by degrees we should arrive at the confines of physical knowledge and find ourselves in the region of metaphysics, where scientific reasoning ceases, and thinking for scientific purposes becomes unprofitable. Excursions into this region would indeed be very useful if on returning to physical regions we could every time bring back with us an instrument as potent and far-reaching as the atomic theory has proved to be, a theory which still remains the basis of all our reasoning in chemistry; but then the atomic theory has been quite an exceptional instance. Metaphysical speculation, such as the *Naturphilosophie* of the Germans has dealt in, has, generally speaking, been utterly barren in natural science.

I will not on the present occasion dwell on the vast addition made to the number of useful and beautiful substances by chemists during the last fifty years. Their number is legion, and their mere description fills volumes, whereas half a century ago a dictionary of moderate size would have sufficed for the purpose. Among these newly-discovered substances none are more remarkable than the metals rubidium, cæsium, thallium, indium, gallium, the existence of which was revealed by the spectroscope, and which, indeed, would have probably remained unknown but for the labours of Bunsen and Kirchoff in perfecting and applying that instrument.

I must not, however, omit all reference to a department of chemistry which has been, one may almost say, created within the time to which I am referring—I mean that of synthesis. When I began to study chemistry we only heard of analysis; of synthesis, so far at least as regards organic bodies, we only dreamt as a remote and unattainable region. The only instance then known of the synthesis of an organic substance was that of urea by Wöhler. Synthesis was, indeed, supposed to be an essentially vital process effected under the influence of the vital force, and quite outside the sphere of the chemist. Since then what marvels have we not seen? Alizarin and purpurin, the colouring-matters of madder, have been prepared artificially by Graebe and Liebermann, indigo by Baeyer, not to mention bodies of simpler constitution obtained by comparatively less complicated processes. We are honoured to-day by the presence of Prof. Ladenburg, who has succeeded in artificially preparing coniin, the alkaloid to which hemlock owes its poisonous properties; the first natural alkaloid, indeed, which has been obtained artificially. Looking back at what has been achieved, I think we may entertain the confident anticipation that all the most important organic bodies, acids, alkaloids, and neutral substances will, in course of time, be obtained in a similar manner, though of one thing we may be pretty sure—viz. that we shall never succeed in forming any really organized matter as distinct from organic. The term organic matter is in fact only employed for the sake of convenience, and as an expression handed down to us from former days, since so-called organic compounds are subject to the same laws with regard to composition as the bodies which we name mineral or inorganic, but organized matter such as we find constituting the vessels of plants and animals is a different thing. The protoplasm contained in the vegetable and animal cell is something very distinct from the same matter after the death of the organism, but the difference between living and dead matter is not of a chemical nature. In referring to chemical synthesis I cannot refrain from expressing regret that so little has hitherto been done in the artificial production of minerals with a view to elucidating the processes by which they were formed in Nature, but it is possible that more has been done in this direction than I am aware of, since this is a department of chemistry with which I am not familiar. It is certain that inorganic chemistry generally does not now receive the attention which it formerly did. The exclusive devotion to the chemistry of the carbon compounds which we find in most of our laboratories at the present day may, however, be accounted for when we see the brilliant results to which the study of those compounds has led.

After these few remarks on the development of chemistry during the last fifty years, of which I know a little, it may seem presumptuous on my part in the presence of some of the most eminent chemists of our day, whose opinions must be of infinitely more value than mine, to say anything about the future of our science and the direction it will probably take. Nevertheless, trusting to your kind indulgence, I will venture on some speculations in this direction, which, if they do not instruct the younger members of the Section, may serve to amuse their seniors, and at all events will refer to subjects on which some thought is well bestowed.

As regards the future of chemistry, the question has frequently suggested itself to me as it has doubtless done to others —Will chemical science go on expanding and developing during the next few generations as it has done in the course of the last hundred years? Will discovery follow discovery, and fact be added to fact, until the record occupies not a few volumes only, but a whole library? Will systematic chemistry, *i.e.* the history and description of all possible combinations of the elements, have any limits? I am inclined to answer in the negative. All human institutions pass through the same phases; they have their rise, they culminate, and decay; and I do not see why the science of chemistry should form an exception. Moreover, it is a natural law that whatever develops rapidly also declines rapidly, and the development of systematic chemistry since the commencement of this century has been perfectly unprecedented. I think it probable that in the course of time, at the rate at which we are now progressing, nearly all possible compounds will have been prepared, all the most important chemical facts will have been discovered, and pure chemistry will then be practically exhausted, and will be in the same condition as systematic botany and mineralogy now are. New compounds will now and then be discovered, just as new plants and new minerals now are, but nothing further will be brought to light that will affect the theories at which we shall then have arrived, whatever they may be. All the material with which the science has to deal having then been brought together, what will happen? Will chemical science cease? Will chemists, satisfied with past achievements, cease to work, confining themselves to practical questions and the history of the days gone by? I think not. The science will continue to develop, but in other directions than those previously pursued. The exhaustion of systematic botany has not put an end to botanical science, for vegetable physiology has opened a wide field to the botanist, one that will take a long time to explore thoroughly. To indicate the directions which chemical science will take in its various applications to other departments of knowledge, as, for instance, in connexion with the study of the physical properties of matter, or in elucidation of the chemical processes whereby minerals have been formed, or those through which geological strata have passed in bygone ages, would not be within my competency, as I should have to touch on subjects with which I am not familiar; but I may be permitted to refer by a few words to a subject, with which, by reading at least, I have become better acquainted, and which seems to me to offer a wide field to the investigator who shall come well provided with physical and chemical knowledge to its cultivation. I allude to the processes whereby the substances constituting the various organs of plants and their contents are formed, and those again to which the decomposition and decay of vegetable matter are due; a subject as to which our knowledge is quite elementary, but which, it seems to me, admits of an extension and development of which we have at present not the least conception.

De Saussure, it is well known, first discovered the fact that plants under the influence of light absorb carbonic acid and give off oxygen, the inference of course being that the carbonic acid and the water present are decomposed, the carbon of the former and the hydrogen of the latter going to form the various organic constituents of the plant, while the oxygen or a part of it is set at liberty and poured into the atmosphere. The facts as they stand are simply these: what the plant requires for its subsistence is carbonic acid, water, nitrogen in some form (presumably that of a nitrate), certain bases—potash, lime, magnesia, iron oxide, and phosphoric acid. Out of these it constructs the whole of its organic frame, its cells and their contents, rearranging the elements of which its food consists in such a manner as to convert inorganic into organic matter, *i.e.* changing bodies in which the affinities of the atoms are thoroughly satisfied into such as contain them in a state of more or less unstable equilibrium, and therefore liable to alteration when their atoms are allowed to act in accordance with their natural affinities. More than this we do not know; our ignorance of the several steps or stages of the process, if there are any such steps, is complete; all that has been added to the general statement just given is mere speculation. Yet it is impossible to remain satisfied with the present state of our knowledge on the subject. Accordingly numerous attempts have been made to bridge over the chasm which separates the inorganic and organic worlds, not indeed to show that the change does not involve the creation, as was once supposed, of new matter—for this was proved long ago —but to exhibit in its details the hidden mechanism which produces it, but hitherto without success. We know that light is essential to the process of assimilation in plants, since this does not proceed in the dark, but this fact does not help us to an explanation, for light in this case is a mere stimulant, and never produces the same or similar effects outside the vegetable organism. Liebig and others have attempted to show that the process of assimilation in plants commences with the formation of some simply constituted body, such as oxalic or formic acid, with the elimination of oxygen, out of which by condensation and further separation of oxygen more complex bodies, such as sugar, fats, &c., are formed; but there is not the slightest evidence at present in favour of this view. The first product of assimilation that is distinctly recognized is starch, a highly complex organic, we might almost say an organized body, which appears at once with all its characteristic properties, like Minerva springing fully armed from the head of Jove. If we are to adhere to the facts so far observed, we must conclude that the plant does not proceed as we should do in the laboratory, beginning with the more simply constituted compounds and advancing to the more complicated, but that the reverse process is the one actually adopted, the supposed intermediate products being more probably the results of retrogressive metamorphosis. This conclusion is, however, so much opposed to ordinary chemical views that one cannot feel surprised at the constantly repeated attempts to clear up the question. There can be no doubt indeed that much here remains to be done and to be discovered.

Intimately connected with this subject is that of chlorophyll, the green-colouring matter of leaves, which is always found wherever the process of assimilation in plants is going on, and nowhere else, and is therefore doubtless an essential factor in the process. What part it plays in this process is, in my opinion, still unknown. Its action is probably in part chemical, in part physical, and this adds, it may be, to the difficulty of understanding it. It is generally supposed that it is chlorophyll which by its direct action on the carbonic acid and water with which it comes into contact leads to the formation of organic matter with elimination of oxygen. But this is, I think, a mere assumption—an error due, like many others, to a mistaken use of terms. The chlorophyll of chemists is simply an organic colouring matter, like alizarin or indigo, but being in the vegetable cell intimately associated with other matters, vegetable physiologists have attributed to the action of one, and that the most obvious, constituent what is really due to the complex, perhaps even to some quite other constituent of the complex. It is impossible to conceive that the chlorophyll of chemists can be endowed with the remarkable and exceptional properties attributed to it by physiologists; it is a chemical entity, nothing more. It may indeed be said that chlorophyll only acts as it is stated to do when inclosed within the vegetable cell, but this merely amounts to saying that its action is not merely chemical, but is controlled by the vitality of the cell, which, I suppose, means the action of the protoplasm. If chlorophyll is the agent whereby the decomposition of carbonic acid and water is effected, how, it may be asked, is the agent itself produced? It does not come from without; the plant must be able to form it in the first instance. We are told by vegetable physiologists that the Coniferæ when raised in total darkness from seeds produce chlorophyll. In light or in darkness I am convinced it is the same; the plant forms chlorophyll as a means to an end. What the end is we know; it is the assimilation of carbon and hydrogen to form organic matter. How does the chlorophyll assist in attaining this end?

In propounding a new theory in reply to this question I venture to claim your indulgence, such as has been accorded to some of my predecessors and others who at these meetings of the British Association have been permitted to make statements and use arguments of a novel or paradoxical character, which, if they effect nothing else, at least afford a relief to the usual routine of scientific reasoning. My experiments on chlorophyll have led me to infer that the constitution of that body is much less simple than it is generally supposed to be. I do not mean by this that chlorophyll is a mixture in the usual sense; everyone who has paid any attention to the subject knows that ordinary chlorophyll consists of several colouring matters, some of which are yellow, not to mention fatty matters which are unessential. What I mean to say is this, that the pure green substance, the chlorophyll *par excellence*, does not belong to the same class of bodies as alizarin or indigo, but contains three elements, each of which is essential to its constitution, one being a basic nitrogenous colouring matter, the second a metal or a metallic oxide, the third an acid, the three together constituting green chlorophyll. The basic colouring matter is a body of very peculiar properties; it is the phyllocyanin of Fremy; the metal may be iron or zinc, the acid I will suppose to be carbonic acid. Now the plant having formed its colouring matter, the metallic oxide being present in some form or other, and the carbonic acid being supplied by the atmosphere, all the necessary conditions co-exist for the formation of chlorophyll. The compound is an unstable one; it easily parts with its carbonic acid, giving it up to the protoplasm or whatever the agent may be that effects its actual decomposition under the influence of light. The advantage of this arrangement would consist in this, that the carbonic acid would be presented in a more condensed state to the agent which effects its decomposition than if it were merely contained in a watery solution, but more loosely combined, and therefore more easily accessible than if it were united to a strong base such as potash or lime. The carbonic acid having been disposed of, the other two constituents would be in a state to take up fresh quantities of carbonic acid, and so on. Chlorophyll would therefore act as a carrier of carbonic acid in the plant, just as hæmoglobin serves to convey oxygen in the animal economy. Numerous objections may of course be raised to the theory of which I give an outline; I only throw it out as a tentative explanation, showing that the function of chlorophyll may be, in part at least, chemical, and that we need not suppose it to be endowed with the marvellous and exceptional powers usually ascribed to it. Other and more probable explanations will doubtless suggest themselves when this difficult subject has been more thoroughly worked out. Eventually, too, it will be found, I imagine, that physical forces as well as chemical affinities play a part in this as in every other process of the vegetable economy. In the case of cholorophyll there can be no doubt that the green colour and the peculiar behaviour towards light have something to do with its action, but on this point it is not necessary for the chemist to pronounce any opinion. I may take this oportunity of mentioning the important experiments of Sachs and Pringsheim on the optical properties of chlorophyll in their relation to assimilation in plants, as they are probably not so well known to chemists as to botanists.

What I have said may serve to show that the very first steps of the process whereby organic or organized matter is formed in plants are hardly understood. We understand still less the further steps leading to the production of the more complex vegetable bodies—acids, alkaloids, fatty matters. Granted that we were able to trace the formation in the plant of a compound of simple constitution, such as oxalic or formic acid, how far should we still be from understanding the building up of such compounds as starch, albumen, or morphia? The syntheses so successfully and ingeniously carried out in our laboratories do not here assist us in the least. We know the steps by which alizarin is artificially produced from anthracene; but can anyone for an instant suppose that the plant commences in the same way with anthracene, converting this into anthraquinone, and having acted on the latter first with acid, then with alkali, arrives at last at alizarin? Indeed the plant never contains ready-formed alizarin at all. What we observe from the commencement is a glucoside a compound of alizarin and glucose, which, so far as we see, is not gradually built up, but springs into existence at once. When we think of the complicated process by which indigo is produced in the laboratory with the various substances and appliances required, and then see how in the minutest seed-leaves of a plant like woad a still more complex substance, indican, is found ready-formed, we stand confounded at the simplicity of the apparatus employed by the plant, and are obliged to confess that we have no conception of the means whereby the end is attained. The same difficulties occur in other cases, and it will therefore probably be conceded that the synthetic process carried on in plants, from the first step to the last, are not in the least understood.

It might be supposed that after all the labour and attention bestowed on the inorganic constituents of plants we should know something of the part played by these constituents in the processes of assimilation and nutrition, but here the obscurity is as great as elsewhere. We know by experiment that certain inorganic matters—potash, lime, magnesia, iron oxide, phosphoric acid—are essential to the growth of plants; but of their mode of action, or of the reason why certain plants require potash salts, others lime, and so on, we know nothing. Phosphoric acid is no doubt an essential constituent of the protoplasm of the plant; but why cellulose, of which the various organs chiefly consist, should require mineral matters, which do not enter into its composition, for its formation and building up is still a mystery.

The department of chemistry which relates to the decomposition of organic and organized matters presents problems almost as difficult of solution as those relating to their formation and building up; that is to say, the phenomena observed do not apparently obey the same laws as those prevailing in the inorganic world. When I began my chemical studies the difference in this respect between mineral and organic compounds was less clearly seen than at present. The conversion of alcohol into acetic acid, the putrefaction of animal and vegetable matter were thought to be simply due to oxidation; they were phenomena, it was supposed, exactly similar to the rusting of iron, the tarnishing of metals, the fading of colours. That a third body was required to initiate and continue the process of decomposition, that organic matter in contact with purified air would remain unchanged for any length of time—was not known nor suspected. I am not quite sure whether spantaneous decomposition—*i.e.* the splitting up of a complex body without the intervention of an external agent—might not at that time have been considered possible. In order to explain the phenomena of fermentation, the decomposition of sugar into alcohol and carbonic acid, for instance, we had only the theory of contact—devised by Berzelius and Mitscherlich, the latter of whom used to expatiate on the subject at great length in his lectures. When this ghost of a theory was laid by Liebig, who suggested an intelligible explanation of the phenomena in accordance with the facts then known, it was felt to be quite a relief, as affording a resting-place—if only a temporary one—for the mind. The brilliant researches of Pasteur, which have thrown so much light on the action of the insoluble organized ferments, I need only refer to, as they are so widely known, even outside scientific circles; and since also investigations such as his cannot be discussed without some reference to biological questions, which cannot be entered on now. I will confine myself therefore to a few remarks on the unorganized or soluble ferments, one of which I had occasion to examine when engaged in investigating madder and its colouring matters. These ferments, the type of which is diastase—a substance found accompanying starch in the seeds of plants—are soluble in water, perfectly neutral, devoid of all definite form, and though apparently inert, able when acting within the sphere in which Nature has placed them to cause changes and decomposition of the most profound character. Their action excludes everything in the shape of vitality, and yet it is as mysterious and unaccountable as anything that the vitality of the organized ferments is capable of effecting. Indeed, in vegetable, and especially animal, organisms they seem expressly intended for the attainment of certain ends necessary for the well-being, or even the existence, of the organism, insomuch that it has been supposed, with some show of reason, that it is to bodies of this class existing within the cells of organized ferments, but not separable by any means at our disposal, that the changes produced by the latter are really due.

A great deal of attention has been paid to the product and results of fermentation, but very little hitherto to the *modus operandi* of the ferments themselves, and yet this seems to me to offer a wide field for interesting research, especially in the case of those of the soluble class, which are easily prepared, and can be manipulated in the laboratory like any chemical substance without the tedious precautions and preliminary operations necessary in the case of the organized ferments. In what way, it may be asked, do these soluble ferments produce the effects peculiar to them? Is the action essentially chemical, or is it due to physical causes as well? Is the quantity of fermentable matter acted on by a certain quantity of ferment unlimited in amount, or are there limits to that amount somewhere? Does the ferment itself undergo any change during the process of fermentation, or is it the same afterwards as before and capable of acting on fresh quantities of fermentable matter? When a ferment is replaced by a strong mineral acid, the products of decomposition being the same, is the *modus operandi* in both cases alike, or must a different explanation be in each case sought? These questions have never been satisfactorily answered, and await solution. I know of only one attempt to show what actually takes place during a process of fermentation set up by a soluble ferment.

The experiments of Wurtz (*Comptes Rendus*, xci. 787), on papaïn, the soluble ferment of *Carica Papaya*, led to the conclusion that the fibrin on which it is made to act combines in the first instance with the ferment itself, the latter after the hydration of the fibrin is completed being again set at liberty, and then able to act on fresh quantities of fibrin. Thus, according to Wurtz, the action is found to be the same as that of chemical agents, properly so called, such as sulphuric acid, of which minute quantities may exert a hydrating action in consequence of the transitory formation of compounds which are constantly being produced and again decomposed.

There is another question referring to these soluble ferments to which in the present state of our knowledge it is impossible to frame a probable answer, viz. why does it so frequently happen that each ferment exerts a specific action, an action peculiar to itself, this being in fact, in the absence of any marked chemical characters, the only means by which they can be distinguished one from the other? Why does one ferment act on starch only, while the function of another consists in the hydration of fibrin, that of another in the decomposition of a glucoside, and so on? In accordance with the explanation of Wurtz, we should say that a specific ferment is one capable of combining only with the body on which it is to act, and with no other. I was led to ask this question when engaged in the examination of the colouring matters of *Rubia tinctorum*. The root of this plant, the madder of commerce, contains glucosides, which, though coloured, are quite devoid of tinctorial power. Nature has at the same time placed in the root a peculiar ferment, which, coming into contact with these glucosides at a certain temperature, effects their decomposition, splitting them up into glucose and true colouring matters. Now this ferment is a body *sui generis*, and cannot be replaced by any other ferment that I have tried; its action is specific. Why Nature should have deposited this body in the recesses of the plant for the express purpose of acting on certain glucosides and forming colouring matters, the object of which, so far as the economy of the plant is concerned, can only be guessed at, is difficult to understand. One is inclined in such a case to revert to the old-fashioned doctrine that some natural processes were devised for the use and delectation of man. It is quite certain in the case of madder that had it not been for its peculiar ferment erythrozym, the valuable tinctorial properties of the root, which have for centuries been applied in the production of that splendid dye Turkey red, would have remained unknown perhaps to the present day, since the only efficient substitute for the natural ferment is a strong mineral acid, and such acids and their uses were unknown in former days.

I am inclined to think that some of the younger chemists and physiologists of to-day may live to see the time when all the at present mysterious and unaccountable processes going on in the organisms of plants and animals, including those of fermentation, will be found to obey purely physical and chemical laws. To the biologist it may seem derogatory to the dignity of his science to have the principle of vitality, which has so long reigned supreme, dethroned and replaced by hard, unbending law. Such, however, is not the opinion of that distinguished botanist Sachs, who says, referring to this very point:—"Der Organismus selbst ist nur die aus verschiedenen Theilen bestehende Maschine, die durch weitere Eingriffe äusserer Kräfte in Bewegung gesetzt werden muss: von ihrer Struktur hängt es ab, welchen Effekt diese äusseren Kräfte an ihr bewirken. Es würde einen sehr niedrigen Horizont wissenschaftlicher Bildung verrathen, in diesem Vergleich eine Herabsetzung des Organismus sehen zu wollen, denn in einer Maschine, wenn auch nur von Menschenhänden gemacht, liegt das Resultat tiefsten und sorgfältigsten Nachdenkens und hoher Intelligenz, soweit es ihre Struktur betrifft, und wirksam sind in ihr schliesslich dieselben Naturkräfte, welche in anderer Combination die Lebenskräfte eines Organs darstellen. Die Vergleichung des organischen Lebens mit unorganischen Processen kann nur dann als eine Erniedrigung des ersteren gelten, wenn man so thöricht gewesen ist, die letzteren als etwas Niedriges und Gemeines aufzufassen, während die unbegreifliche Grösse und Durchgeistigung der Natur in beiden Fällen sich gleichartig offenbart" (*Vorlesungen über Pflanzenphysiologie*). The time may be far distant when these views of the great botanist shall be universally accepted; but they will, I think, sooner or later prevail.

The little known territory which separates the domains of chemistry and physiology will, in my opinion, offer a wide and interesting field for research, after that of pure chemistry shall have been exhausted or lost its interest. Most important problems connected with life and its relation to the inorganic world there await solution, and I confess that I am inclined to envy the young investigator who, coming provided with an ample store of chemical and physical knowledge, shall apply himself to the solution of these problems. The pleasures derived from the successful pursuit of such studies belong to the highest and purest that we are able to conceive. I can, however, only repeat what has so often been said before, and what the young man of science should not forget, that a life devoted to research only involves no material rewards; it certainly never secures wealth, sometimes not even honour nor fame. Looked on with indifference or even dislike by the State, the Church, and the public at large, all that the man of science can certainly look forward to at the close of his career is the addition at his hands of a few stones to the vast edifice of truth, and the consciousness of having attained a higher stage of intellectual insight.

You may probably expect me, before I conclude, to make some reference to technological matters, to the various chemical arts and manufactures for which the Manchester district is noted. At the last meeting of the British Association in Manchester a report on the condition at that time of manufacturing chemistry in the South Lancashire district, by Sir Henry Roscoe, the late Dr. Angus Smith, and myself, was laid before the Chemical Section. A similar report showing the progress made in chemical technology since that time would have been interesting. Great changes have taken place during the period that has elapsed, especially as regards the alkali trade, and quite a new branch of industry has been developed, that of the coal-tar colours. A description of these new features of our chemical industry with statistics of production would therefore have been acceptable. The idea of a report had however to be given up on account of the difficulty of obtaining reliable information as to details, and in these matters it is the details only which are interesting, the general features of the subject being well known. It can hardly be a matter for surprise, I think, that our manufacturers, considering the active competition to which they are exposed, and the disadvantages under which they labour in consequence of the exclusiveness of foreign nations, should be loth to furnish information which would benefit their rivals in trade. Several interesting papers on branches of chemical industry by gentlemen well versed in them will, however, be read before the Section, and these will, to a great extent, make up for the want of a general report. In the Chemical Section of our Jubilee Exhibition, too, you will see a very fine collection of chemical products, more extensive and beautiful, perhaps, than any previously brought together, and these will give you a good idea of our industrial activity. It would have been interesting to witness step by step some of the processes employed in the manufacture of these various products, but this, I am sorry to say, must not be expected generally.

To some it may seem that this Jubilee Exhibition shows the manufacturing industry and prosperity of this district at least at their highest state of development; that they are now at their meridian, and in the future are doomed to decline. If this be so—and there are certainly indications which seem to favour this view—it would be well for those whose visits here are only occasional to take especial note of the present state of things so as to be able to compare their impressions when they next visit us with those now received, since gradual changes in communities, as in individuals, are more patent to casual observers than to those who are always on the watch.

From some points of view the signs of the times are certainly not encouraging. It should not be forgotten that the manufacturing prosperity of this district depends to a great extent on the ample supply of a product which is brought to us at some cost from tropical and semi-tropical countries to be re-exported in the shape of manufactured goods. A political convulsion abroad, and this, unfortunately, is a casualty that may at any time be expected, or even the determination on the part of other nations to starve us out, however short-sighted such a determination might be, might cut off our supplies and disable us permanently as we were partially disabled twenty-five years ago. If to this be added the fact that foreign nations are becoming increasingly hostile and exclusive commercially, we cannot feel surprise at the dismal forebodings entertained and the confident predictions of decline uttered by some who claim to know all the facts. I ought to apologize for alluding to so gloomy a subject on the occasion of this to a great extent festive gathering, but then men of science like to look at a question not only from a hopeful but from every point of view. Fortunately on this question they are not called upon to pronounce any opinion one way or the other.

Should this be the last time that Manchester shall entertain the British Association in the day of its prosperity, I can only say with the German poet—

"Schliesst den Kreis und leert die Flaschen]
Diese Sommernächte feiernd,
Schlimme Zeiten werden kommen,
Die wir Auch sodann ertragen."

Whether in prosperity or adversity I feel sure that this city will always endeavour to entertain its visitors to the best of its ability. On the present occasion I may with confidence on the part of the chemical world of Manchester offer to the many friends from near and far who honour us with their presence at this meeting a most hearty welcome.

SECTION C.

GEOLOGY.

Opening Address by Henry Woodward, LL.D., F.R.S., V.P.G.S., President of the Section.

Since I received the friendly intimation from Prof. Bonney your distinguished and able President of last year, that the Council of this Association had done me the honour to select me to occupy the Presidential chair of this Section which he had vacated, I have been greatly exercised as to what subject to choose for the brief address with which it has now become customary to open the session. Not that there is any lack of materials ready to hand for the purpose—on the contrary, the subjects embraced by geology are now so varied and extensive that the effort to focus them in a single mind is ever becoming a more difficult task to accomplish, and demands the literary skill of a Lyell or a Geikie to marshal and arrange them from year to year in a manner suitable for presentation to you at our annual gathering.

Foremost in interest must necessarily be that which relates to our home affairs, and in this I have been most kindly favoured by Dr. A. Geikie, the Director-General of the Geological Survey of Great Britain, who sends me a brief notice of the progress of the Survey for 1886, taken partly from his Annual Report as Director-General and partly from information supplied by the office through the kindness of Mr. William Topley, our Recording Secretary. The following is the statement which I have received :—

The survey of the solid geology of England and Wales was completed at the end of 1883, and the field staff has since been occupied in surveying the drift deposits, making at the same time such revisions of the ordinary (solid) geology as may be necessary. In the north and east of England the drift and solid have been surveyed at the same time. The areas examined in the earlier days of the survey, in the south, centre, and west of England, and in Wales, were done for the solid rocks only.

In order to meet the great need for a general map of England and Wales on a moderate scale, one is being engraved by the Survey on the scale of 4 miles to 1 inch (1 : 253440), and will be issued in fifteen sheets.

A few of the survey memoirs relate to large areas, and give complete descriptions of the formations therein exposed, but most of the memoirs are explanations of special sheets of the map. A series of monographs is now in preparation giving full descriptions of special formations. Mr. Whitaker has charge of that on the Lower Tertiaries ; Mr. H. B. Woodward and Mr. C. Fox-Strangways are preparing the Jurassic memoir, the former taking the rocks south of the Humber, and the latter those of Yorkshire ; Mr. Jukes-Browne is writing the Cretaceous monograph ; and Mr. Clement Reid that on the Pliocene beds.

In Scotland some advance has been made in mapping the important and complicated area of the north-west Highlands. The surveyors there were chiefly engaged between Loch Stack and Ullapool, subsequently completing the area about Durness and Eriboll. The other parts of Scotland now being surveyed are the north-eastern and western side of the Grampians, all south of the latter having been already completed.

Ireland is entirely surveyed with the exception of a small area in Donegal, which will probably be completed this year. This district is of interest from its resemblance to the north-west Highlands, and from the problems which it presents as to the origin of the crystalline schists. The recent discovery of organic remains amongst the Donegal schists adds additional interest to this inquiry.

The publications of the Survey during the past year are as follows : England and Wales, six sheets of the map, two sheets of horizontal sections, three of vertical sections, and six memoirs ; Scotland, three maps and one memoir ; Ireland, two maps and six memoirs.

The next matter which has arisen since our last meeting relates to our colonies, and comes to us in the shape of a message from the retiring President of the Association, Sir William Dawson, who has embodied his ideas in a letter to the President of the Royal Society (Prof. Stokes), copies of which have been sent also to all the learned Societies. To the former I am indebted for a copy accompanied by a favourable report thereon from the Royal Society of Canada.

As the object of this communication is one in which I am sure we, as Englishmen, must all feel a hearty sympathy, appealing as it does to our patriotism in its widest sense, as well as to our devotion for and interest in the science of geology, I feel I shall not need to apologize for introducing it to your notice here.

We are invited by it to enrol ourselves, as geologists, in a Federal Union, composed of all our brethren at home, in our colonies, and in all the dependencies of the British Crown. Nor are we to stop here, for when this has been satisfactorily accomplished it is suggested that we should invite our English-speaking cousins of the great United States, with whom we are already in such close alliance upon so many objects of common scientific interest, to join our Geological Confederation, and, having thus obtained an overwhelming majority, we are to proceed—without armies or vessels of war—to extend our peaceful conquest over every country on the habitable globe, urging and persuading those countries which have not established geological surveys to do so forthwith, and inviting those which have surveys of their own to join our British Association Geological Union. And when all has been accomplished in this direction our exertions as a confederacy may well be extended to secure the mapping of all those outlying regions of the earth's surface at present imperfectly known or still geologically unexplored.

Suggestions such as these could hardly come at a more fitting and appropriate moment, for are we not now on the eve of the completion of the geological surveys of the British Islands? if such a task can ever be said to be completed which has occupied the attentive study of so many able geologists during the last eighty years or more, and from the very nature of the case must always require additional research and revision.

India, Africa, and our colonies may all hope for future assistance from the many geological students now being trained in our schools and colleges, who may not be required in the near future for home surveys, and must needs go further afield to win their title of admission to the ancient and honourable order of "Knights of the Hammer."

This idea of scientific federation was referred to by Prof. Huxley in his Presidential Address to the Royal Society in 1885, and subsequently by the present President (Prof. G. G. Stokes) in November last.

If we would devise a scheme by which we might from time to time, recognize in a suitable manner—whether by corresponding membership, or honorary fellowship, or by medals and awards—as Prof. Huxley has suggested, the good scientific work being done by members of the many Societies in our distant colonies of Canada, South Africa, Australia, New Zealand, and elsewhere, that would indeed be a step in the right direction, and would doubtless prove most helpful and encouraging to all our fellow geologists abroad.

The Geological Society of London, no doubt, to some extent covers this ground; but it should be noticed that in the view of this Society, our colonies and other dependencies are not, and I think rightly, recognized as foreigners, that designation being employed for those who are not in any sense subjects of the Queen.

As a consequence, the geologists of our colonies are not looked upon as eligible for honorary connexion with the Geological Society, and though in the distribution of the medals and awards their work is no doubt noticed, yet that is now so important and extensive that it might be desirable to secure for it a more specific and extensive recognition than has hitherto perhaps been possible.

Might we not also through the home influence we could bring to bear by means of this great Section of the British Association succeed in inducing our practical colonial Governments to see the enormous commercial as well as scientific gain that must eventually accrue to themselves if they would, with wise liberality, continue to completion their much-needed geological surveys, instead of (as has too often happened) abandoning the work before its end has been attained, or making its maintenance from year to year contingent on the chance discovery of gold, or the successful boring for coal or water—results not always to be attained within twelve months by a geologist in a new country, however good he may be, unless he have a fairy godmother or a divining-rod at his command?

If by means of our confederation such useful and helpful works can be inaugurated, we shall have fulfilled an object well worthy the initiation of Sir William Dawson, and of all those whose names may be connected with so laudable an undertaking.

Nor need such a development of the work of this Section interfere in any way with the labours of the "International Geological Congress," which occupies a distinct field of its own; for whatever we might accomplish in carrying out the suggestions put forward by Sir William Dawson would really be in effect to second and support—not to hinder—the work of that most useful body of geologists.

Our next topic relates to foreign affairs.

The International Geological Congress, which met in Bologna in 1881, and in Berlin in 1885, will hold its next meeting in London in 1888. This year the Committee of the Congress on Geological Nomenclature will meet during the Association week at Manchester. Prof. Capellini, of Bologna, is the President of this Committee, and Prof. Dewalque, of Liége, is the Secretary. Its object is to discuss various questions respecting the classification and nomenclature of European rocks, and to report thereon to the Congress in London.

It is quite certain that a large number of Continental and American geologists will be present in London next year, and it rests with English geologists to determine whether the meeting shall be as successful as those which have preceded it. The Berlin Congress left the arrangement in the hands of a small Committee of English members (Messrs. Blanford, Geikie, Hughes, and Topley), and advantage will probably be taken of the presence of so many geologists in Manchester to further the organization of the English meeting.

The occasion of the Congress visiting London next year should also be a sufficient reason to enlist new members here, and it is to be hoped that a very cordial reception will be accorded to all those who come from abroad to attend the meeting. It ought to be a great success, and deserves our warmest sympathies and co-operation.

Geology seems, at present, to be passing through what may not inaptly be termed a transitional or metamorphic period in its history, when old-established ideas are rapidly melting away, and under fresh influences are crystallizing out into quite other forms.

"New lights for old" is the popular cry both in science and politics, and, like the Athenians, nothing delights us more than to hear tell of some new thing.

If the proposition lately made by Prof. Judd, the President of the Geological Society in London, in his recently delivered Anniversary Address, holds good, that mineralogy is the father of geology, it seems not improbable that, like Saturn's offspring, our science is in danger of being devoured by its reputed parent; for certainly mineralogy, in the form of petrology, has of late years most largely occupied the geological field, whilst palæontology, once the favourite child of geology, is in its turn threatened with imminent extinction, as a separate study, by biology, which, without any substantial gain, now replaces, *in name only*, the hitherto better known sciences of botany and zoology.

Indeed, could the views so eloquently put forward by Prof. Judd be maintained, mineralogy itself would have to be added to the list of sciences included under biology. But notwithstanding the well-known aphorism of Linnæus—

> "Lapides crescunt; Vegetabilia crescunt et vivunt;
> Animalia crescunt, vivunt, et sentiunt"—

the growth of the first is of a totally different nature from that which takes place in the last.

Minerals, or more properly crystals, increase or grow in size by additions to their *external surfaces* of molecules of matter identical with themselves. They are therefore as a rule homogeneous throughout, almost rigid, and remain under ordinary circumstances unchanged irrespective of time.

Plants and animals, on the contrary, increase by intussusception, or the taking of matter within their tissues. Their bodies are *not homogeneous*, and they exhibit all the various phenomena of growth and decay.

We stand, then, still like "watchers on the threshold," not yet admitted beyond the veil. We are not prepared to include minerals in the study of living beings, nor are we, I submit, any nearer the solution of the problem, What is life? whether we call it "*vitality*" or "*vital force*," nor can we produce it like "electricity" or "electrical force" by the aid of mechanics. That it has existed ever since our planet became habitable by living organisms is beyond doubt; and since life first dawned it seems equally certain that this "vital force" was never at any time extinguished, but, like the sacred flame of Iran, its light has always gladdened our earth with its presence.

I have already referred to the vastness and diversity of the domain which Geology claims as her own; indeed, we might, if so disposed, pursue our subject in its *cosmical aspect*, and, inviting the astronomer and the physicist to our aid, proceed to consider the evolution of our earth and its subsequent history as a part of the solar system.

Or, taking up *geognosy*, we might inquire into the materials of the earth's substance and the chief rocks and minerals of which its crust is built up.

Should *dynamics* charm us, then we may study the various agencies by which rocks have been formed and altered, and the frequent changes in relation to sea and land which the terrestrial surface has undergone in former times.

Does *rock-architecture* attract us? It is ours to inquire how the various materials of the earth came to be arranged as we find them—whether wrought by living agents, or ejected by volcanic forces, or laid down quietly by water.

Or is *chronology* the object of our study? Then our task will be to investigate the well-marked succession of the stratified rocks and the sequence of events which they record.

Again, we might prefer the *physiographical aspect of geology*, embracing the history of the features of the earth and the causes which have brought about its varied conditions of continent and ocean, of mountain and valley, hill and plain, making up that grand diversity of surface which constitutes its scenery.

Yet more, it is within our domain as geologists to investigate *the past life of the globe* through all its successive changes and to trace it from its earliest dawn in pre-Cambrian times down to its grand development at the present day.

One result of the very vastness of this kingdom is that there is a tendency amongst its subjects to form into separate constituencies, and these in an incredibly short time evolve languages of their own, so that, unless this fissiparity can be successfully arrested, we shall speedily repeat the story of "the confusion of tongues," and our geological tower, which once promised by our combined labours to reach grandly heavenwards, may soon cease from building altogether.

This incoherence in our body politic may, I think, be traced to that great development by the microscope in mineralogical geology and petrology, which has no doubt been necessitated by the investigation of those remote pre-Cambrian or Archæan rock-masses in the north-west Highlands, Shropshire, the Malverns, South Wales, Cornwall, and the west of Ireland, whose fossils, if they ever existed, have been entirely obliterated[1] by the changes which their matrix has undergone, and whose very stratification has been lost by metamorphic action. In such investigations some of our ablest geologists have now been for long occupied with the best possible results, and Bonney, Callaway, Cole, Davies, Geikie, Hicks, Hull, Judd, Lapworth, Peach, Sorby, Teall, and many others have been labouring most zealously on these most ancient sediments, barren though they be of life forms, and often destitute of bedding.

It is refreshing, however, to find Prof. Judd at times abandoning volcanoes, and turning his attention most successfully to lizard-hunting with Prof. Huxley in the Elgin sandstones, or studying the micro-organisms in the cores from the Richmond boring or the valley of the Nile; to see Dr. Hicks leaving his patron St. David far behind, and digging for bones in the pre-Glacial caves at St. Asaph. Prof. Lapworth, too, we see avoiding Cape Wrath, and discoursing on the beauties of Canadian Graptolites and the Cambrian rocks at Nuneaton.

Thus there is still a bond of union connecting stratigraphical geology and palæontology and a common ground of interest whereon all geologists may meet. It should then be our endeavour not to dissociate ourselves or our interest from any subject of geological inquiry, but to maintain the union between all branches of our science and with all workers in whatever field they may labour, adopting for our motto the ancient maxim, "*Vis unita fortior est.*"

Especially should we adhere to the study of palæontology, seeing that it is indissolubly connected with one of the earliest chapters in the history of our science. Indeed, through the evidence afforded by organic remains, William Smith (better known by the title given to him by Prof. Sedgwick, "the father of English geology") was led to those remarkable generalizations as to the identification of strata by means of their contained fossils, which have exercised so great an influence over our own science during the past ninety years, and is still the guiding principle on which our classification of the sedimentary rocks is based. What Wollaston has done for mineralogy and crystallography, William Smith initiated for stratigraphical geology; and we cannot overlook our obligation to Smith whilst we reverence the work of his distinguished contemporary, Wollaston.

Palæontology, or the study of ancient life forms, stands somewhat in relation to geology as the science of archæology does to history, or as zoology and botany to physical geography. But, whereas the investigator of recent living forms deals with entire organisms and can study both their morphological and their physiological history as well as their geographical range, the palæontologist has too often to deal with imperfect remains, many of which have no exact modern representative, and has, in consequence, to look for and seize upon minute characters for his guidance, which the worker on recent forms would probably neglect as too trivial for even specific diagnosis.

The palæontologist, if he would succeed, must in fact be a trained zoologist or botanist, as the case may be, and an accomplished geologist also; such combinations of qualities like those possessed by the earlier race of "naturalists" are less frequently to be met with at the present day. They represent amongst us the same class of men as the "general practitioner" does in medicine; they are the all-round good scientific men, but not "specialists."

Biology, or the study of living things, has now become so vast a field that everyone is compelled to take up some special subject, and in striving to master it he makes his reputation as an authority on this or that group of organisms.

There is much to be said in favour of such a method of working, but I hold that everyone who so elects to spend his life must first of all pass through a thorough grounding in general biology, and should on no account take up special work until he has mastered thoroughly the general principles of scientific classification and the various types of organized beings, otherwise he will be for ever viewing all Nature with distorted vision, seeing, in fact, "men as trees walking." If as a student he shall have been nurtured wholly on the anatomy of the *sole*, all objects will be viewed from the stand-point of that one-sided fish. If the cockroach has engrossed his youthful studies, all nature will swarm with *Periplaneta orientalis*.

We have to guard against the starting of *student-specialists*.

[1] Traces of fossils are said to have been met with in Donegal, and I have just received evidence of Trilobites in the Upper Green Llanberis slates at Penrhyn, hitherto considered unfossiliferous!

They must begin by being "general practitioners" if they are ever to do any good in the world of science, and after serving their time in a museum or elsewhere, then by all means let each follow his own "bent" and devote himself to some particular group, as did Davidson to the *Brachiopoda*, to the exclusion of all else.

It is the absence of "all-roundness" which has retarded more than any other thing the constant interchange of ideas between zoologists, botanists, and palæontologists, without which science languishes. Biologists as a body do not care to look at or study fossils; they see neither form nor beauty in the petrified fragments of a plant or animal such as would induce them to study these more closely, and they turn to the exquisitely perfect specimens of recent objects in their cabinets with a sigh of relief. But *Nemesis* is at hand created by our modern system of extreme biological training. The student of to-day is averse to the systematic work of both zoology and palæontology in our museums, and, technically inclined, craves for nothing so much as to be allowed to embed some interesting embryo in paraffin and cut it into 10,000 slices.

As a consequence our museums will suffer unless we can revive amongst our students a taste for and a love of general natural history; such, we mean, as the *taste for Nature* which excited the enthusiasm of Charles Kingsley and stimulated the zeal of Charles Darwin. We cannot all sail round the world as did Banks and Solander, Darwin, Huxley, Hooker, Wyville Thomson, Moseley, and so many other naturalists, though the mere act of travelling has now become so ridiculously easy that our own Association awoke one morning in Montreal, and may, for aught we know, find itself some day in Sydney or Melbourne! But we can fully appreciate Nature in a dredging expedition or feel her influence on a moor or mountain, in a quarry or down a mine.

What we want for our students in these high-pressure days are less frequent attendance in the examination room and a more frequent examination of Nature in the field. Our professors must take their men more often afield, and show them how to collect specimens and familiarize them with the aspects of natural objects as seen *without microscopes*, and they will return to their studies with far better and keener eyesight after their own *macroscopic* vision has been enlarged by contact with Nature.

Whoever then takes up the study of fossils must also be well acquainted with the structure of living animals and plants; he may also be expected to go on adding to his store of biological knowledge—but as some division of labour is absolutely essential, the man who pursues palæontological research must be prepared to concentrate all his energies to the elucidation of these extinct organisms, studying, but not occupying himself in describing, recent forms.

In order, however, to work satisfactorily at any particular group of extinct organisms, his eyes and his understanding must go through a long and careful training before he will be able to interpret correctly the appearances presented by the specimen before him, and to avoid the fallacies by which he is liable to be misled, arising out of the necessarily imperfect materials and their different modes of preservation in the matrix.

He must learn to distinguish between a suture and a fracture, and to know when a specimen has been distorted by cleavage or other mechanical cause, or altered by mere difference of mineralization. Such deceptive appearances have too often led to the multiplication of species, and even the creation of spurious genera.

Thus occupied in the investigation of ancient life forms, he will in truth be only writing the first chapters on the botany or the zoology of the earth, and whilst his carefully obtained results are of the greatest importance to the speculations and conclusions of the geologist they are equally essential to and a part of biological science.

My friend Dr. Traquair has recently thus expressed, in relation to his own subject, what I have attempted to make more general:—"The man who satisfactorily investigates the structure or determines the systematic position of a fish or reptile *preserved in stone* is as much a zoologist as he who describes a similar creature *preserved in spirits*, though with this difference, that the former task is in some points rather the more difficult, seeing that we have *only the hard parts* to go upon, and these generally in a crushed, fragmentary, or scattered condition. And," he adds, "without a genuine interest in, as well as a thorough knowledge of, recent biology, no one can hope to produce work of any value in palæontology."

Of course the value of all palæontological work, as of all zoological or botanical work, must depend entirely upon the care and exactness with which the work is performed.

Time, the great assessor of all human labours, will sit in judgment upon them and pronounce by their durability or instability the comparative value of each.

It appears to me that to the careful palæontological worker, as to the careful archæologist, the greatest merit is due if he succeed correctly in deciphering the too often fragmentary and blurred remains of a bygone age, and giving us in the present an accurate interpretation of a page from the life-history of the past.

Then, too, there is the geological aspect of palæontology. And here I may state that one of the charges made by a brother zoologist against us is "that we use fossils *merely* as counters by which to record the progress of geological time."

As well might exception be taken that the milestones along a turnpike road had been used by a traveller to calculate the length of his journey.

But omitting the word *merely* (for fossils have been made to give up many secrets to the investigator besides their age), I gladly accept the charge as conveying a great and important truth.

Do not the historian and the antiquary use the coins and medals dug from the ruins of the dead and long past dynasties of the world as sure guides in the chronology of the human period?

And may not the geologist also use "the medals of creation" as Dr. Mantell aptly called them—coined in no counterfeit mint, as the best and most trustworthy guides to enable him to establish the chronology of the stratified rocks of the earth?

Great then as is the benefit which zoology has derived from palæontology in enabling the zoologist to learn the earliest appearance in time of each group of organisms, and the modifications in structure, so far as we are enabled to ascertain them, which each may have undergone from the ancient to the modern period—it may be doubted whether even this valuable aid equals the service performed to stratigraphical geology by the careful study of organic remains—in enabling us to write the chronology of the rocks over so large a portion of the habitable globe.

Without fossils stratigraphical geology would be as unsatisfactory as it would certainly be uninteresting; with their aid it becomes, both in the field and in the cabinet, one of the most attractive and delightful of studies.

Owing to the very nature of sedimentary deposits, being of necessity either lacustrine, estuarine, or marine in origin, our knowledge of the ancient land surfaces of the globe is necessarily very limited, but we know much concerning its old marine areas. These are the more constant and widespread, and it is mainly upon these deposits, and not so much upon the more limited evidences of ancient land surfaces, that our chronology has been based.

Of the antiquity of cave-folk and their contemporary Mammalia we may expect to hear the very latest utterances from Prof. Boyd Dawkins and Dr. Hicks. The former is also to be congratulated upon his renewed work on the Mammalia in the Palæontographical Society's volume for 1886 (just issued). Prof. O. C. Marsh has added a further contribution to American palæontology in the shape of a memoir describing and figuring sixteen new species of Mesozoic mammals from the Upper Jurassic rocks in Wyoming, on the western slope of the Rocky Mountains. Mr. Lydekker has just completed Part V. of his most useful and much-needed Catalogue of the Fossil Mammalia in the British Museum, containing the Sirenia, Cetacea, Edentata, Marsupialia, and Monotremata.

The fossil birds remain to be catalogued. In the Reptilia it is refreshing to see Prof. Huxley once more taking up the pen and writing upon *Hyperodapedon* and *Rhynchosaurus* in his old vigorous and earnest style. We can only regret that his health precludes him from continuous labour, to the no small loss of science. Prof. Marsh shortly promises us his memoir on the Sauropoda, the plates of which are progressing rapidly to completion.

Our late veteran chief, Sir Richard Owen, although retired from active official duties, contributes a paper on *Galesaurus planiceps*, a Triassic saurian from South Africa, and a further memoir on *Meiolania* from Lord Howe Island.

Prof. Seeley and M. Louis Dollo are both occupied with Dinosauria, the former from the Cape (whence he has also detected part of a mammalian skeleton in the Triassic rocks), and the latter is adding to our knowledge of Iguanodon and other forms from the Wealden of Bernissart.

In the Amphibia, Prof. Dr. Herman Credner has added a most valuable paper on the development of *Branchiosaurus*, a small Labyrinthodont from the Keuper of Saxony, in which he has been able successfully to trace the development through a long series of individuals of a water-breathing naked larva of the Palæozoic epoch into an air-breathing adult form, clad in a strong coat of mail.

In fossil ichthyology, A. Wettstein has been occupied in the study of the Eocene fishes of the Glarus slates, and in his recent memoir he shows that out of one fish (*Anenchelum*), so constantly distorted by slaty cleavage, Agassiz had made no fewer than six species. The fish is now found to be identical with the living "scabbard-fish," *Lepidopus*; and the author reduces the forty-four species of Glarus fishes to twenty-three, and adds four new ones. Among the latter is the first fossil *Remora* yet met with, named *Echeneis glaronensis*. Its first dorsal is modified as a sucker, exactly as in the living *Remora*.

Baron Zigno, of Padua, has figured and described the first entire *Myliobatis* hitherto discovered in the Eocene of Monte Bolca.

M. Louis Dollo records the occurrence of two skeletons of *Carcharodon heterodon* in the Eocene of Boom, Antwerp, one measuring 7 metres, and the other nearly 9 metres in length. They are now mounted and exhibited in the Brussels Museum.

Mr. J. F. Whiteaves is commencing to publish the detailed descriptions of the Devonian fishes from Scaumenac Bay, Quebec.

Mr. James Wm. Davis, of Halifax, has produced a second monograph for the Royal Dublin Society. The first, which appeared in 1883, was devoted to the teeth and spines of Elasmobranch fishes from the Carboniferous limestone of Great Britain; the present monograph, illustrated by twenty-four plates, is devoted to the description of the fishes of the Cretaceous rocks of the Lebanon, and makes us acquainted with a wonderful series of Selachian fishes, representing nine genera and sixteen species, of which two genera and twelve species are new to science. The Ganoids comprise two species of Pycnodonts and two forms related to Amia; there are also a number of Teleostean fishes, amongst which are *Pagellus*, *Beryx*, *Homonotus*, *Platax*, and many other genera. Two species of eel, *Anguilla*, are the first Mesozoic examples recorded. Altogether we have ten genera and sixty-three species of fish recorded as new. The author is to be congratulated upon having contributed to fossil ichthyology one of the most extensive works published in recent years.

Mr. Arthur Smith Woodward (a former student of Owens College, Manchester) has this year also contributed numerous papers on fossil fishes: on *Ptychodus* from the Chalk; *Squaloraja* from the Lias; on the Brazilian genus *Rhacolepis*; on a Maltese *Holocentrum*; "On some Eocene Siluroid Fishes from Bracklesham"; and "On the Canal-System in the Shields of Pteraspidean Fishes."

Mr. E. T. Newton describes a *Semionotus* from the Trias of Warwickshire.

Both Mr. James W. Davis and Dr. R. H. Traquair have given us descriptions of the anatomy of *Chondrosteus acipenseroides* from the Lias of Lyme Regis.

Mr. William Davies describes two species of *Pholidophorus* from the Purbeck beds of Swanage, Dorset.

But the groups which have proved of the greatest service in the chronology of the sedimentary rocks have been the Mollusca, the Brachiopoda, and Crustacea (especially the Trilobita, Phyllopoda, and Ostracoda), the Echinodermata, Corals, Graptolites, Sponges, and Foraminifera.

It would be an interminable task merely to record the workers in the various sections of palæontology, but in glancing at these one cannot prevent many illustrious names arising in one's mind—many who have finished their work, and are reckoned among the fathers of the science, but many also who are still our companions, and from whom we may expect further important help before they lay down their hammer, their lens, and their pen.

In the Cephalopoda the task so lately left by our countryman Dr. Wright, after a long life devoted to palæontological science, has been taken up by Mr. S. S. Buckman, who has already presented one fasciculus of a monograph on the Ammonitidæ of the Inferior Oolite.

The Gasteropoda of the Oolites have an able historian in Mr. W. H. Hudleston, whose contributions on this subject enrich the pages and plates of the *Geological Magazine* and the Proceedings of the Geologists' Association; the Palæozoic forms are in the hands of Dr. Lindström.

The Lamellibranchiata cry for help at present in vain, and we regret more than ever the loss of Stoliczska, who promised such good work had his life been spared.

The Brachiopoda, so long and so well cared for by Dr. Davidson, now also demand a successor to that illustrious name.

The Polyzoa, which suffered so severe a loss in the death of Mr. Busk, have since been well cared for by Mr. Arthur W. Waters and Mr. Vine.

Until quite lately, the oldest fossil insects known were the six fragments of wings of Neuroptera, from the Devonian of New Brunswick, obtained by Mr. C. F. Hartt and described by Mr. S. H. Scudder. More lately the wing of a cockroach has been obtained from rocks of Silurian age in Calvados, France; whilst almost simultaneously fossil scorpions have been met with by Dr. Hunter, of Carluke, in the Upper Silurian of Lanark, and determined by Mr. B. N. Peach, and from the Upper Silurian of Gotland, described by Dr. Lindström.

These discoveries carry back our records of old land surfaces to a far more remote period than that of the Coal-measures, vast as its distance is removed from recent times.

Mr. B. N. Peach is the discover of several scorpions, and I have also recently figured and described three new forms of cockroach and several spined myriapods from the Coal-measures. Another cockroach, also new, which has been kindly sent me for study by Mr. Peach, brings to our knowledge a larval stage of *Blatta* from the Scottish Carboniferous.

Dr. McCook has just added a genus of spiders, *Atypus*, to our Eocene beds from the Isle of Wight.

The Crustacea have found in Mr. B. N. Peach and in Prof. Rupert Jones able and willing historians. Mr. Peach has taken up the Carboniferous Macrouran Decapods, and Prof. Rupert Jones the Palæozoic Phyllopoda, aided by myself; Prof. Jones is attacking the Tertiary and Cretaceous as well as the Palæozoic Ostracoda, so that his hands will be full for many years to come.

The Echinodermata have lost Dr. T. Wright, who for years acted as their monographer in the Palæontographical Society's volumes, but they have secured the services of other accomplished naturalists. Mr. Robert Etheridge, Jun., and Dr. P. Herbert Carpenter have produced a grand monograph on the Blastoidea in the British Museum; and no doubt this is but the beginning of good things to come, for although Mr. Etheridge has entered upon a new sphere of work in the Australian Museum, Sydney, Dr. P. Herbert Carpenter hopes to take up the stalked Crinoids before long, and Mr. Percy Sladen, who, with Prof. P. Martin Duncan, has already done so much good work amongst the Indian Echinoderms and elsewhere, promises to take the star-fishes in hand for us later on.

The Corals have many friends, chief amongst whom is Prof. P. Martin Duncan, and Prof. H. A. Nicholson, and various other excellent workers, but they are even a more difficult and a less attractive group than the Echinodermata, and their determination is not so satisfactory owing to their irregular and heteromorphic growth.

The Stromatoporoids have lost an investigator in the field in Arthur Champernowne, whose unexpected and early loss we all deplore. But in Prof. Nicholson they will find a most careful and painstaking monographer, who has already given us one fine instalment of his work in the Palæontographical volume.

In Prof. C. Lapworth we have an exponent of the structures and affinities of the Graptolites as a class and of their stratigraphical position in the rocks unsurpassed by any other worker. With him must be associated the names of Barrande, Carruthers, Hopkinson, Nicholson, and a long list of foreign workers, all of whom, however, look upon Lapworth as the highest authority in this group.

In the Spongida we are especially indebted to Dr. G. J. Hinde, first for an excellent well illustrated quarto catalogue of these organisms in the geological collection of the British Museum, and secondly for the Palæozoic part of a fine monograph of these for the Palæontographical volume just issued.

Nor must we omit to recall the names of Prof. Zittel, of Dr. Carter, of Prof. Sollas, and many other able workers in the fossil sponges.

In the Foraminifera we naturally recall the names of D'Orbigny, D'Archiac, Carpenter, Parker, Brady and Jones, and Sir William Dawson, our illustrious ex-President. Prof. Rupert Jones is still at work on this group, and has recently published a paper on *Nummulites elegans* from the Eocene beds of Hampshire and the Isle of Wight.

Of late years fossil botany, too long neglected, has taken a place of note in all those inquiries concerning the origin of floras, the age of the stratified rocks, the former distribution of land surfaces, and especially in all questions relative to the climate of the globe in past times.

Passing over the earlier period of the present century, when fossil botany was known only by the works of Artis, Witham, Schlotheim, Sternberg, Goeppert, Cotta, Lindley and Hutton, Steinhauer and Adolphe Brongniart, we have to recall the names of other workers who have only passed away in our own time, such as Binney, Bunbury, Corda, Bowerbank, Heer, Unger, Schimper, and Massalongo.

In the period of fifty years, whose completion we have just celebrated, the names of our countrymen Binney, Bowerbank, Williamson, and Hooker, stand prominently forward contemporarily with those of Geinitz, Unger, Rossmasler, and Schimper, in Germany. In 1845 Dawson and Lesquereux entered the field in America, Hooker in England, and one of the ablest writers on fossil plants, Oswald Heer, entered upon his great work in Switzerland. In 1850 Massalongo in Italy, and von Ettingshausen in Austria, were added to the roll of famous palæobotanists, and in 1853 Newberry joined the American field of research. In 1860 the work so long abandoned by Brongniart, in France, was taken up by de Saporta, and it is no small gratification to have him with us here to-day, and to welcome him amongst our distinguished foreign guests.

About the same time my friend and colleague William Carruthers commenced to write on fossil botany, and brought to bear upon the subject that accurate and careful knowledge of living forms without which such investigations must always prove but futile.

It is extremely difficult to estimate the number of species of fossil plants that had been described up to the year 1837, but it probably fell far short of a thousand. In 1828 less than 500 species were known to Brongniart.

In the first edition of "Morris's Catalogue," published in 1843, the number of British fossil plants recorded is 628.

Careful lists were published by Goeppert and by Unger in 1844 and 1845, giving a total of known species from 1600 to 1800.

In 1849 the number had increased, according to Bronn's "Index Palæontologicus" to over 2000, and the following year Unger enumerated 2421 in his "Genera et Species Plantarum," rather more than 500 of which may have been British. In 1852 Morris (2nd edition) gives the number of species as 750. Since then, chiefly through the labours of Heer, Ettingshausen, Lesquereux, Massalongo, Unger, and de Saporta, this number has been more than quadrupled. Mr. Gardner estimates that at least nine thousand species must have been described. This great increase is chiefly due to the more careful exploration of the Tertiary strata, in which the more highly organized and consequently more differentiated plant forms occur.

The number of plant remains described in Great Britain during the whole fifty years has been extremely small, but much has been accomplished in the study of fossil plants generally, and in this task no one has been more earnest than Prof. Williamson, of Owens College, Manchester.

His investigations of the plants of the Coal period have been of the most exhaustive nature, and from his researches into their microscopic structures we are almost as well acquainted with the minute tissues of these ancient denizens of the forests of the Carboniferous epoch as we are with those in the parks around Manchester to-day.

Mr. Carruthers's "Memoirs on the Coniferæ and Cycadeæ, and on the Fruiting Organs of the Lycopodiaceæ" have greatly advanced our knowledge of these interesting types, heretofore but imperfectly known from their fossil remains.

Mr. R. Kidston has devoted himself most earnestly to the investigation of the fossil plants of our British coal-fields, and he has determined not to rest satisfied merely to work out the plants obtained by others in our museums, but he has visited all our coal-fields and searched the shales on the spot for himself. The results of his collectings may now be seen in the valuable additions made to the Coal-measure series of plants in the British Museum (Natural History).

But it is more especially in reference to the Tertiary flora of Britain that progress has been made of late years.

Thanks to the labours of Mr. Starkie Gardner, who has not only obtained abundant materials for an exhaustive monograph with his own hands from Sheppey, Alum Bay, Bournemouth, Reading, Mull, Antrim, and many other localities, but has already favoured us with several memoirs in the Palæontographical Society's annual volumes and elsewhere on the British Eocene flora, we may hope before long to have a more complete history at this period of our islands than we already possess of the flora of the Carboniferous age.

Nor has any research, favoured by the aid of this Association, brought so large a return in beautiful and instructive specimens to our National Museum of Natural History as have the investigations carried out by Mr. J. S. Gardner.

We must not omit to mention Mr. Clement Reid, who has so diligently traced many of the specimens of our existing flora in the Pleistocene strata of the eastern counties.

"Large numbers of ferns and gymnosperms," says Mr. Gardner, "have been discovered in Mesozoic rocks, but remains of the interesting monocotyledons which must have accompanied them are provokingly scarce. We know that palms, grasses, &c., appear at certain definite horizons, but we are ignorant regarding their ancestry. We know that temperate floras, largely composed of dicotyledons, flourished as far north as man has been able to penetrate, in the Cretaceous and Tertiary periods, but nothing in the least suggesting a transitional form has been found amongst them. Lastly, we have learnt that floras now indigenous to Japan and the Himalayas, to Australia and South America, once inhabited Europe, groups of wholly different plants succeeding and displacing each other in such rapid succession on the same spot as to suggest that the normal condition of floras is one of slow but perpetual migration, and that the term 'indigenous' has no geological significance."

In reference to the question of geographical distribution of organized beings in geological time, the conclusion is strongly forced upon us, from a study of fossil remains, that the great zoological provinces into which the earth's surface and the seas of the globe are now subdivided have been brought about by the limitation of species at no more distant date than the Secondary period, and probably even later than this.

That in Palæozoic times there must have been a great uniformity of marine conditions, and the fauna of each of the primary formations was consequently not only of vast duration but of world-wide extent, is evident.

When, as in Carboniferous times, we are enabled to study the contemporary land conditions of the globe, we find they must also have been very uniform, at least so far as the explored parts of this hemisphere are known, both the fauna and the flora at this epoch being co-extensive with the northern hemisphere, indeed, in all probability far wider, seeing that identical species occur in the Carboniferous series of Australia and North America. Even those well-marked lines which at present follow more or less closely the isotherms of our hemisphere seem not to have exercised the same influence on the fauna and flora as they do at present. Thus in high northern latitudes and within the Arctic Circle we find abundant evidence of life in Palæozoic, Mesozoic, and even down to Tertiary times, unaffected by latitude; so that we are justified in assuming that a far milder temperature extended to much higher northern regions than that which at present exists on the globe, and consequently that a larger portion of the earth's surface (as well as its seas) was then habitable.

How great, then, is the field of research still open to our investigation, and how far distant must that day be ere the last problem shall have been solved, and the last chapter written, in the ancient life-history of our earth!

"We write in sand, our labour grows,
And with the tide the work o'erflows."

With unskilled hand I have struck here and there only a few chords on the many-toned harmonicon of geology. I fear they may not all have vibrated quite in unison as a perfect composition would; but, however crude the performance has been, I trust that it will not be provocative of discord. If some few ideas suggest themselves as worthy of your acceptance, I shall not have spoken altogether idly, nor you have listened so long and so patiently entirely in vain.

NOTES.

DR. EMIL HOLUB has arrived in Europe, after three years of adventurous exploration in Africa, and although he brings with him only a part of his scientific collections—the rest having been plundered by the Mashukulumbe, a tribe to the north of the Zambesi—yet this fragment is certain to prove of great scientific value. It includes over 2000 specimens of birds, 27,000 of insects, and 6500 of plants. Dr. Holub brings with him also many hundred observations in meteorology and measurements of all nations. The collection will be exhibited in Europe; and it is stated that, if circumstances enable them to do so, Dr. and Mrs. Holub will resume their African explorations. They are expected to arrive in Vienna on the 15th or 16th inst., and will be officially received by the Austro-Hungarian Exploration Society.

A SLIGHT shock of earthquake was felt at Bonn and in its vicinity at fifty-two minutes past four on Monday afternoon, accompanied by a dull subterranean rumbling.

THE Government of India has sanctioned the purchase for the Lahore Museum of a zoological collection illustrative of Indian silk culture.

M. PAVIE, who undertook an adventurous journey from Siam into Tonquin, starting from Luang Prabang, has been compelled to return to Siam, having been driven back by bands of Chinese, who are described as ravaging the country.

MR. J. B. LILLIE MACKAY, of the Royal School of Mines, and at present Lecturer in Chemistry at Trinity College, Melbourne University, has been appointed Director of the School of Mines, Sandhurst, Victoria.

MR. OMOND, the Superintendent of the Ben Nevis Observatory, replying in the *Times* to criticisms in Parliament of the work of the Observatory, to which we referred in a note last week points out that the report of the Meteorological Office, on which these criticisms were based, merely states that Mr. Omond's telegrams were useless for a particular purpose, viz. forecasting storms. He then proceeds to state the position of the Observatory in the matter as follows:—"When the Ben Nevis Observatory was opened at the close of 1883, the directors offered to send the Meteorological Office daily weather telegrams—the only satisfactory way in which observations can be used for forecasting, The Office declined this offer, mainly on the ground of expense, but asked that occasional messages might be sent when any unusual or interesting events occurred, adding that these messages might be sent more frequently at first. I had some difficulty in understanding why the Office wished a record of sudden changes to be sent by wire instead of waiting till they got them in the ordinary way by post, but considered that the best way was to follow their instructions literally and send telegrams recording sudden changes or unusual occurrences, with a due regard to economy and with gradually diminishing frequency. A reiterated request for economy at the time of the introduction of sixpenny messages led to the entries sent being usually restricted to two hours' reading of the various instruments —one before the change in question had set in and one after it was fairly established. Though most sudden changes are connected with storms, yet a few of the messages recorded changes from bad to good weather, especially when the occasional great dryness characteristic of high-level stations began suddenly or in any unusual manner. How a Committee, 'composed of men of the very highest scientific standing,' had come to regard these messages, and especially this last sort, as 'storm-warnings' passes my comprehension. The question of the value to meteorological science of the Ben Nevis observations I may safely leave to abler hands—it needs no vindication of mine; but I must protest against the unfairness to me and the other members of the Ben Nevis Observatory staff of first asking for records of changes which have occurred, and then declaring them useless because they are not also forecasts of what is to be. It should be noted, however, that in two cases the record from Ben Nevis arrived at the

Meteorological Office before the warning from the sea-level stations —a fact which proves, all the more conclusively because unintentionally, the great value Ben Nevis would have if properly utilized as a forecasting station by means of daily reports sent to some one able both to interpret their indications and to compare them with similar daily reports from low-level stations all over the country. The former of these conditions can only be fulfilled by those who have studied the Ben Nevis records of the last three years and a half, while the latter in this country necessarily is limited to the Meteorological Office, which alone receives such daily telegrams. It is to this unfortunate hiatus and not to any defect in the position of Ben Nevis that the alleged uselessness for practical forecasting of its observations is due." Mr. Omond's vigorous defence is therefore the same in substance as that of Mr. Buchan which we reproduced last week. It amounts to this: the Observatory is blamed for not doing certain work which it offered to do, but which the Meteorological Office refused to permit it to do; Mr. Omond did what he was asked, and did not do that which the Meteorological Council refused to have from him on the score of expense.

THE Committee of the International Geological Congress dealing with the question of geological nomenclature is holding its meeting in Manchester at the same time as the British Association. The representatives at present in Manchester are Prof. G. Capellini, Rector of the University of Bologna, and representative of Italy (President); Prof. Dewalque, Belgium (Secretary); Prof. O. Torell, Sweden; Prof. Vilanova-y-Piera, Spain; Dr. T. Sterry Hunt, Canada; Dr. W. T. Blanford, India; and Prof. T. McK. Hughes, England. Meetings of the Committee, at which the foregoing were present, were held in the Committee-room of the Geological Section on Tuesday and Wednesday last week, at which Mr. J. E. Marr and Mr. W. Topley, of the English International Committee, were also present. The object of this Committee of the Congress is to discover the nomenclature and classification adopted by different authorities and in different countries, with the view of bringing their views into harmony, and also to lay down rules for the guidance of geological workers in the future. The subject chiefly discussed has been the classification of the Cambrian and Silurian rocks, some reference having also been made to the question of the Archæan rocks. The full Congress will meet in England next year, when these and other similar matters will be considered. Another meeting of the Congress Committee was held on Friday.

THE autumn meetings of the Iron and Steel Institute are announced to take place this year at Manchester. They will be held at Owens College, the use of which has been granted by the Governors for the occasion, and will begin on the morning of September 14. The programme is a very comprehensive one viewed from a metallurgical and manufacturing point of view, while the more strictly holiday features of such a gathering have not been neglected. The President of the Institute (Mr. Daniel Adamson), will give a paper on "Machines for the Testing of Metals," a subject to which he has devoted a great deal of attention. Mr. Thomas Ashbury is down for a paper on "Recent Metallurgical and Mechanical Progress, as illustrated at the Manchester Exhibition," and as that Exhibition, so far as relates to machinery in motion, is probably the best that has ever been held, and illustrates with unusual completeness every department of the engineering art, such a paper can scarcely fail to be an instructive and valuable record. A third paper will be read by Mr. James Johnson, on "The Mechanical Apparatus for Continuous Moulding at the Works of M. Godin, at Guise," the interest of which will be of a two-fold character—first, as regards the processes and apparatus to be described, which are of a novel and improved character; and, secondly, as regards the system of co-operation which has been adopted there on a more complete and successful scale as between employers and employed than probably in any other part of Europe. Dr. Fleming, of University College, London, will contribute a paper on "Electric Light Installations for Works and Factories," a subject with reference to which he has had a very large amount of experience as the consulting engineer for the Edison-Swan and other companies. The manufacture of ordnance, respecting which there has been so much discussion in military and political circles of late, will be brought forward by Capt. Cubillo, who occupies a responsible position at the Royal Arsenal of Trubia, in Spain. Capt. Cubillo has studied the conditions of the manufacture of ordnance at all the leading arsenals both in this country and abroad, and as the Spanish Government has recently exhibited a strong disposition to be abreast of the world in naval and military affairs, this paper is likely not only to show what they have so far achieved, but also to bring to the front comparisons with reference to ordnance that will probably be extremely useful and interesting at the present time. One other paper, that by Mr. Wailes, of the Patent Shaft and Axle-tree Company, at Wednesbury, promises to give to the world some extremely interesting data respecting the recent progress of the basic process for the manufacture of steel on the open hearth. Hitherto, as is well known, the progress of this system, both on the Continent and in England, has been chiefly in the direction of Bessemer working, but recently the open hearth has come into competition with the Bessemer process for this purpose, and Mr. Wailes's paper will probably lead to a discussion as to the comparative merits of the two systems which will be of value both to producers and consumers of steel.

THE *Times* Correspondent in Philadelphia telegraphed on Monday night that an International Medical Congress was formally opened by President Cleveland at Albaugh's Opera House in Washington on Monday. Five thousand physicians are in attendance, including over 2000 delegates sent here from nearly all parts of the globe. One of those present, Dr. Fanny Dickinson, of Chicago, is the first woman who has been admitted to an international medical gathering as delegate. Many of the most distinguished doctors of the day were present. An address of welcome was delivered by Mr. Bayard, Secretary of State. The welcome was acknowledged and responded to by Dr. William Harris Lloyd, Inspector-General R.N., on behalf of Great Britain; Dr. Léon Laforte, of Paris, on behalf of France; Prof. P. G. Unna, of Hamburg, on the part of Germany; Senator M. Semmola, of Naples, for Italy; and Dr. Charles Reyber, of St. Petersburg, representing the Government of Russia.

THE centenary of the first ascent of Mont Blanc was celebrated at Chamounix on the 28th ult., when a monument for which the various Alpine and tourist clubs of the world, as well as the Paris Academy of Sciences, contributed the funds, was at the same time unveiled to De Saussure, who made the ascent on August 28, 1787, with the guide Jacques Balmat. The monument is of bronze on a granite pedestal. The principal feature of the procession with which the proceedings opened was the band of old guides, forty in number, and all over seventy years of age. M. Spuller, French Minister of Education, unveiled the statue, and delivered an oration in honour of De Saussure.

CONSIDERABLE additions have recently been made to our knowledge of those interesting substances, the chemical products of the action of Bacteria upon the animal body, called ptomaïnes. In the last number of the *Berichte* of the German Chemical Society, p. 2217, Dr. Ladenburg clears up completely all doubt as to the composition of one of the best known of the ptomaïnes, cadaverin, which he shows is perfectly identical with artificially prepared penta-methylene-diamine. A short time ago Dr. Bocklisch published (*Ber.* 1887, p. 1441) the results of his researches upon the products of the action of Finkler's bacillus (*Vibrio proteus*) upon sterilized flesh, showing that this bacillus decomposes flesh with formation of the alkaloid cadaverin, $C_5H_{14}N_2$, which is non-poisonous, and ammonia. On repeating his experiments, however, in presence of ordinary putrefaction germs in addition to the Finkler bacilli, he made the remarkable discovery that an entirely different base, methyl-guanidine, of intensely poisonous properties, was the chief product; hence the symptoms of particular diseases may be due to the poisonous alkaloids formed by the joint action of specific bacilli and ordinary putrefaction germs. Bocklisch made several analyses of the cadaverin which he obtained in the first series of experiments, due to the action of pure cultivations of the *Vibrio proteus*, and showed that its hydrochloride forms a crystalline compound with mercuric chloride of the composition $C_5H_{14}N_2.2HCl.4HgCl_2$, and as this differed somewhat from the composition formerly assigned to the artificial preparation by Ladenburg, the subject was involved in some doubt. Happily, Ladenburg has made fresh and purer preparations of his penta-methylene-diamine, and finds that its compound with mercuric chloride has precisely the composition assigned to the double chloride of mercury and cadaverin by Bocklisch. Hence cadaverin is conclusively proved to be none other than penta-methylene-diamine, and, consequently, must be added to the list of products of animal life which have been synthesized. The formation of these alkaloids, during disease or after death, has a most important bearing upon the treatment of cases of suspected poisoning, inasmuch as, whether poisonous or not, their reactions differ very little from those of the deadly alkaloids; and in the interests of justice it is to be hoped that our knowledge of this branch of organic chemistry may soon be rendered as complete as possible.

MESSRS. MACMILLAN announce the following scientific works for the forthcoming publishing season:—"Electricity and Magnetism," by Amédée Guillemin, translated and edited, with additions and notes, by Prof. Silvanus P. Thompson; "The Nervous System and the Mind," by Charles Mercier; "Popular Lectures and Addresses on Various Subjects in Physical Science," by Sir William Thomson; "Radiant Light and Heat," by Balfour Stewart, F.R.S. (the last three belonging to the Nature Series); "Kinematics and Dynamics," by J. G. Macgregor; "Geometrical Conics," by A. Cockshott and Rev. F. B. Walters; "A Treatise on Analytical Statics," by I. Todhunter, F.R.S., a new edition, revised by Prof. J. D. Everett, F.R.S.; "A Key to Mr. Todhunter's Conic Sections," by C. W. Bourne; "A Key to some Examples in Messrs. Jones and Cheyne's Algebraical Exercises," by Rev. W. Failes; "A Key to Mr. Lock's 'Arithmetic for Schools,'" by the Rev. R. G. Watson; "A Companion to 'Weekly Problem Papers,'" by the Rev. John J. Milne; "A Key to Dr. Todhunter's Treatise on the Differential Calculus," by H. St. J. Hunter; "A Treatise on Chemistry," by Sir H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S. (Vol. IV. Part I.); "Algebra for Schools and Colleges," by Charles Smith; "The Elements of Chemistry," by Ira Remsen; "Absolute Measurements in Electricity and Magnetism," by Andrew Gray; "A Practical Text-Book of Pathology," by D. J. Hamilton; "A Course of Quantitative Mineral Analysis for Students," by W. Noel Hartley, F.R.S.; "School Course of Practical Physics," by Prof. Balfour Stewart, F.R.S., and W. W. Haldane Gee (Part I. "Electricity and Magnetism); "Examples in Physics," by D. E. Jones; "Inorganic and Organic Chemistry," by Sir Henry E. Roscoe, F.R.S., and Prof. C. Schorlemmer, F.R.S. (Vol. III. "Organic Chemistry," Part IV.); "Greenland," by Baron A. E. von Nordenskjöld; and "Corea," by W. A. Carles.

MESSRS. SWAN SONNENSCHEIN AND CO.'s announcements of new books include the following works:—"The Microscope," edited from the work of Profs. Naegeli and Schwendener, by Frank Crisp and J. Mayall, Jun.; "Animal Biology," by Adam Sedgwick; "British Fishes," by F. A. Skuse; "Mammalia," by F. A. Skuse; "Reptiles," by Catherine Hopley; "Ants and Bees," by W. Harcourt Bath (the last four in the Young Collector Series); "The Solomon Islands and their Natives" and "The Geology and Physical Characteristics of the Solomon Islands," with maps, by Dr. H. B. Guppy.

MESSRS. LONGMANS announce the following works of scientific interest:—"The Literary Remains of Fleeming Jenkin, F.R.SS.L. and E., late Professor of Engineering in the University of Edinburgh," edited by Sidney Colvin, with a Memoir by Robert Louis Stevenson; "Picturesque New Guinea," by J. W. Lindt; "A Manual of Operative Surgery, having Special Reference to many of the Newer Procedures," by Arthur E. J. Barker; "A Course of Lectures on Electricity, delivered before the Society of Arts," by George Forbes, F.R.S.

MESSRS. KEGAN PAUL AND CO., London, and Messrs. Appleton and Co., New York, will publish shortly the Hon. Ralph Abercromby's work on "Weather" as a number of the International Science Series. This will be the first book in the English language which deals exclusively with the nature of weather changes from day to day, as distinguished from the climatic or statistical treatment of the subject. There will be ninety-nine charts and diagrams, of which a considerable number will relate to the United States, and others to India and Australia, so as to illustrate the nature of weather on the widest possible basis.

THE additions to the Zoological Society's Gardens during the past week include a Pig-tailed Monkey (*Macacus nemestrinus*) from Sumatra, presented by Mr. B. Lynch; a Fettered Cat (*Felis maniculata*), a Spotted Eagle Owl (*Bubo maculosus*), a Hoary Snake (*Coronella cana*), four Spotted Slowworms (*Acontias meleagris*) from South Africa, presented by Dr. E. Holub, C.M.Z.S.; a Carrion Crow (*Corvus corone*), British, presented by Mrs. MacLochlin; a Martinique Gallinule (*Ionornis martinicus*), captured at sea, presented by Mr. R. Drane; two African Lepidosirens (*Protopterus annectens*) from the River Gambia, West Africa, presented by Mr. H. H. Lee; a Malabar Parrakeet (*Palæornis columboides* ♂) from Southern India, a Malaccan Parrakeet (*Palæornis longicauda* ♂) from Malacca, a Laughing Kingfisher (*Dacelo gigantea*) from Australia, deposited; a Tiger Bittern (*Tigrisoma brasiliense*) from Brazil, purchased; a Red-faced Ouakari (*Brachyurus rubicundus* ♀) from the Upper Amazons, received in exchange; a Collared Fruit Bat (*Cynonycteris collaris*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

BROOKS'S COMET.—Mr. H. V. Egbert, from observations made on August 26, 28, and 30, has computed the following elements for the comet discovered by Mr. Brooks on August 24:—

$$T = 1887 \text{ October } 6{\cdot}48\text{d. G.M.T.}$$

$$\left.\begin{aligned}\pi - \Omega &= 63^\circ\ 18' \\ \Omega &= 84\ 33 \\ \iota &= 44\ 10\end{aligned}\right\} \text{Mean Eq. } 1887{\cdot}0.$$
$$\log q = 0{\cdot}08718$$

It will be seen that these elements bear a great resemblance to those of Olbers' comet of 1815. Dr. Holetschek, in a supplemental circular of the *Astronomische Nachrichten*, supplies the subjoined ephemeris for the comet, basing it upon the sweeping ephemeris for Olbers' comet given by Ginzel in the *Astronomische Nachrichten*, No. 2696, the comet's orbit being assumed to be a parabola for the sake of simplicity, and the following observation made by Herr Palisa at Vienna being used :—

August 27, 15h. 27m. 12s. Vienna M.T.
R.A. = 8h. 42m. 55·71s., Decl. 29° 34′ 24″·7 N.

Ephemeris for Berlin Midnight.

1887.	R.A. h. m.	Decl. ° ′	Log *r*.	Log Δ.
September 5	9 21·1	30 16 N.	0·120	0·311
9	9 39·7	30 20	0·112	0·304
13	9 58·8	30 16	0·105	0·297
17	10 18·4	30 2 N.	0·099	0·291

THE MORRISON OBSERVATORY.—The first number of the publications of the Morrison Observatory, Glasgow, Missouri, U.S.A., has just appeared. This Observatory was founded, in 1875, by the liberality of Miss Berenice Morrison, and possesses an equatorial refractor by Alvan Clark, of 12¼ inches aperture, and a transit-circle by Troughton and Simms, with objective of 6 inches aperture and 77 inches focal length, the circles being 24 inches in diameter. In this first volume Prof. C. W. Pritchett, the Director, gives a history and description of the Observatory, with an account of the determination of the longitude and latitude of the meridian pier, besides a selection of such observations and notes made at the Observatory as are likely to be of use to astronomers. These latter include measures of double stars, observations of occultations, of the transit of Mercury, 1878, measures of the diameter of Mars, observations of comets, of Jupiter and Saturn, and of the figure and dimensions of Uranus. Prof. Pritchett's work appears to have been seriously crippled through lack of means, and, considering the excellent use which he has made of the resources at his command, it is to be hoped that he may speedily find himself in a position to carry on the operations of the Observatory on a more extended scale.

NEW OBSERVATORY AT JUVISY.—The current number of *L'Astronomie* contains a description of a new Observatory belonging to M. Camille Flammarion, which has just been completed. An admirer of M. Flammarion had presented him some five years ago with a little chateau and park situated on the road from Paris to Fontainebleau of historic name and interest. The house, which was built in 1730, possessed walls so thick and solid as to serve as a perfectly stable base for the equatorial and dome with which M. Flammarion has surmounted it. The dome is 5 m. in interior diameter, and covers an equatorial by Bardou of 0·24 m. aperture and 3·75 m. focal length, with clockwork by Bréguet, furnished with a Villarceau governor. Two smaller telescopes—one by Secretan of 108 mm. aperture, the other by Foucault of 160 mm., stand on the adjoining terrace. The Observatory, the co-ordinates of which are East longitude from Paris 0h. 0m. 8s., N. latitude 48° 41′ 36″, commands an uninterrupted horizon, and an atmosphere noticeably purer than that of Paris.

THE TOTAL SOLAR ECLIPSE OF AUGUST 19.—We learn from the current number of *Ciel et Terre* that M. Niesten, of the Brussels Observatory, was fairly successful in his observations of the eclipse. It had been his intention to push on as far east as Perm, but a delay in the arrival of his instruments led him to accompany M. Belopolsky to Jurjewitz on the Volga. The sky was cloudy here as at most of the other stations, but cleared a little round the sun at the time of totality, and M. Niesten was able to see the chromosphere and prominences, and the appendices of the corona, and his assistant secured eight photographs, of which six were good. The exposures varied from 8 seconds to half a minute; the chromosphere and prominences were shown on all, and two gave traces of the corona and also of Regulus, which was near the sun. M. Karinne, a Moscow photographer of the same station, also secured several photographs. A drawing which M. Niesten made of the corona showed a strongly-marked coronal ray, about a degree in length, in the direction of the solar equator.

MINOR PLANET No. 267.—M. Charlois, of Nice, who discovered this object, has named it Tirza.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 SEPTEMBER 11–17.

(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 11

Sun rises, 5h. 30m.; souths, 11h. 56m. 36·1s.; sets, 18h. 24m.; decl. on meridian, 4° 35′ N.: Sidereal Time at Sunset, 17h. 46m.

Moon (New, September 17, 14h.) rises, 22h. 27m.*; souths, 6h. 22m.; sets, 14h. 22m.; decl. on meridian, 19° 12′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	5 28	12 1	18 34	5 48 N.
Venus	7 23	12 40	17 57	9 9 S.
Mars	1 43	9 29	17 15	18 55 N.
Jupiter	9 46	14 48	19 50	12 1 S.
Saturn	1 8	9 0	16 52	19 47 N.

* Indicates that the rising is that of the preceding evening.

Sept.	h.	
14	2	Saturn in conjunction with and 1° 39′ north of the Moon.
14	18	Mars in conjunction with and 1° 48′ north of the Moon.
17	16	Venus in conjunction with and 12° 50′ south of the Moon.
17	22	Mercury in conjunction with and 2° 33′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
Algol	3 0·8	40 31 N.	Sept. 11,	2 19	*m*
			,, 13,	23 8	*m*
λ Tauri	3 54·4	12 10 N.	,, 11,	3 6	*m*
			,, 15,	1 59	*m*
δ Libræ	14 54·9	8 4 S.	,, 12,	4 5	*m*
			,, 16,	19 48	*m*
U Coronæ	15 13·6	32 4 N.	,, 16,	2 34	*m*
R Draconis	16 32·4	67 0 N.	,, 12,		*m*
U Ophiuchi	17 10·8	1 20 N.	,, 11,	22 34	*m*
			and at intervals of	20 8	
X Sagittarii	17 40·5	27 47 S.	Sept. 14,	23 0	*m*
W Sagittarii	17 57·8	29 35 S.	,, 12,	4 0	*M*
U Sagittarii	18 25·2	19 12 S.	,, 14,	0 0	*m*
R Aquilæ	19 0·9	8 4 N.	,, 16,		*M*
η Aquilæ	19 46·7	0 43 N.	,, 14,	4 0	*M*
W Cygni	21 31·8	44 52 N.	,, 11,		*M*
δ Cephei	22 25·0	57 50 N.	,, 19,	2 0	*M*
			,, 15,	20 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near χ′ Orionis	88°	18° N.	Very swift; streaks.
,, 50 Aurigæ	98	43 N.	Very swift; streaks.
,, α Lyræ	282	42 N.	Swift; bright; long.
,, γ Piscium	346	0 N.	Slow; bright.

SCIENTIFIC SERIALS.

THE most interesting item of information in the *Journal of Botany* for August is the record of an addition to the flowering plants of Great Britain, in the discovery, by Mr. H. C. Hart, of the Arctic *Arabis alpina* in Skye.—Mr. Tokutaro Ito, has an interesting paper on the history of botany in Japan.

Rendiconti del Reale Istituto Lombardo, June 30.—On the normal derivatives of the potential function of surfaces, by G. Morera. This paper forms a supplement to the author's late communication (*Rendiconti*, vol. xx. Part 8) on the derivatives of the potential function of space. The extremely simple analytical method by which he succeeded in determining general conditions for the existence of those derivatives and their effective expressions has also enabled him to solve the analogous question regarding the normal derivatives of the potential function of surfaces.—On the part played by sensuous images on the development and exercise of the reasoning faculty, by Tito Vignoli. In this paper the author investigates the actual form and genesis of perceptions acquired through the senses, from the standpoint of their efficacy in developing and sharpening the intelligence of animals. The subject is treated comparatively, it being impossible to understand any act or fact of human psychology unless studied in connexion with similar manifestations in other animals. The general conclusion is arrived at that the impression communicated through the senses is the true instrument of intellectual progress, and that in it lies the potentiality of abstract science. Pure geometry, arithmetic, and algebra are merely the last term of abstract simplification reached by the sensuous perception in its intrinsic evolution.

IN the *Nuovo Giornale Botanico Italiano* for July, Sig. P. Voglino publishes critical remarks on a number of species of Fungi belonging to the Agaricini; and Prof. Caruel gives his Annual Report of the Botanical Museum of Florence for the year 1885-86.—Prof. Delpino discusses the chemical and physiological equation of the process of alcoholic fermentation, which he considers to be more simple than it has been regarded by recent writers. Succinic acid and glycerin, which are found in the liquid after fermentation, he believes to be only secondary products of the process, which consists in the simple removal from grape-sugar of a portion of its carbon and oxygen, and its consequent reduction to the constitution of alcohol.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 29.—M. Hervé Mangon in the chair.—On tornadoes in the United States, by M. H. Faye. Some observations are made in connexion with the popular work on tornadoes recently issued by Mr. Finley, of the United States Signal Service. In reply to that writer, M. Faye maintains that tornadoes are not ascending but descending movements, being whirlwinds with vertical axes due to the different velocities of moderately elevated atmospheric currents, which, like the eddies in running waters, always descend till arrested by the resistance of the ground. They penetrate like a corkscrew through the lower strata, continually contracting and tapering to a point owing to the increasing pressure of these lower strata. Their progressive movement, mainly towards the north-east, is due to the upper currents where they take their rise, and whose mean velocity and direction they retain. Their ravages are due to the violent shock of the descending spirals against the obstructions of the ground, and their fury is not spent or diminished in overcoming these obstacles, because the source of their energy is always in the upper regions, where it is constantly renewed and transmitted to the earth by the descending motion. It is further shown that the United States comply more than any other region of the globe with the conditions most favourable for the development of these destructive cyclones.—Observations of Barnard's comet, May 12, 1887, made at the Observatory of Bordeaux with the 0·38 m. equatorial, by MM. G. Rayet and Flamme.—Determination of the longitude of Haiphong, Tonquin, by telegraphy, by M. F. La Porte. Its longitude was for the first time determined in 1874 by MM. Héraud and Bouillet, who deduced it from that of Saigon. But at the beginning of this year the meridian of Haiphong was connected with that of Hong Kong by means of the submarine cable, and from the observations taken at both extremities a mean was obtained for the cathedral of Hong Kong of 7h. 27m. 20·43s., and for the Observatory of Haiphong, 6h. 57m. 22·63s. east of Paris. For the latter point Héraud's chronometric observations had given 6h. 57m. 19·8s.—Note on a projection saccharimeter, by M. Léon Laurent. The saccharimeters already constructed by the author are of two types: the rotatory polarimeter, requiring monochromatic light; and the compensating saccharimeter, more specially adapted for sugar, and using ordinary light. The present apparatus, of which a sectional view is given, has the advantage of being adapted for use with the electric light now so generally employed in large scientific establishments.—Experiments in agricultural chemistry, by M. J. Raulin. The experiments here described were carried out last year at the agricultural station of the Rhone. Their special object was to ascertain how the disturbing influences due to the varying fertility of the soil may best be obviated. The land being disposed in three equal plots, A, B, C, the extremes A and C are treated identically, while B serves as the point of comparison for the special circumstance under consideration. Normally the fertility increases or diminishes with a certain uniformity from A to C, so that half of the joint yield of A + C would be equal to that of B if the three plots were subjected to the same treatment. The cause of error due to the inequality of the soil being thus for the most part removed, the relation of the yield of $\frac{A + C}{2}$ to that of B will express the actual influence of the circumstance under consideration. In an experiment carried on according to this method, the superphosphate and precipitated phosphate used with Dattel wheat gave a very decided increase, while the result of the application of fossil phosphates and scoriæ was somewhat doubtful.—Note on the waterspout of August 19 on the Lake of Geneva, by M. Ch. Dufour. This waterspout, formed by the collision of the west and the *vaudaire* or south wind, immediately disappeared on reaching the shore half a mile west of the Rivaz railway-station on the Swiss side. From the data supplied by various observers the author calculates its height at about 106 metres. It churned up the surface of the lake, producing an effect somewhat like that of paddle-steamers, but did no damage of any kind on the land.—M. R. Guérin presented a note on a process by means of which the question of the lunar atmosphere might be elucidated. He remarked that the diurnal motion of the moon, owing to its proper motion, is not the same as that of a star. Hence, under certain conditions, a photographic lunette would give to our satellite a sharp edge, and for a star in the neighbourhood of this edge a luminous streak. It therefore seems certain that, however attenuated may be the lunar atmosphere, the photogenic conditions will be changed at the point of contact of the two heavenly bodies, and that the streak made by the image of the star should show some trace of this change.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Animal Alkaloids: Sir W. Aitken (Lewis).—City and Guilds of London Institute Programme of Technological Examinations, Session 1887–88.—First Steps in Geometry: R. A. Proctor (Longmans).—Easy Lessons in the Differential Calculus: R. A. Proctor (Longmans).—Australian Museum; Report of Trustees.—A Treatise on the Animal Alkaloids: A. M. Brown (Baillière).—Hydrophobia: R. Suzor (Chatto and Windus).—The Glasgow and West of Scotland Technical College Calendar for the year 1887–88 (Anderson, Glasgow).—Elementos de Calculo de los Cuaterniones: V. Balbin (Buenos Aires).—Les Plantes des Champs et des Bois: G. Bonnier (Baillière, Paris).—Bench Book for Test-Tube Work in Chemistry: H. T. Lilley (Hamilton).—Notes upon the History of Floods in the River Darling: H. C. Russell.—Notes upon Floods in Lake George: H. C. Russell.—Results of Rain and River Observations made in New South Wales and part of Queensland, 1886: H. C. Russell (Sydney).—Journal of Chemical Society, September (Gurney and Jackson).

CONTENTS.

PAGE

THURSDAY, SEPTEMBER 15, 1887.

A BATCH OF GUIDE-BOOKS TO THE NORFOLK BROADS.

Hand-book to the Rivers and Broads of Norfolk and Suffolk. By G. C. Davies. Ninth Edition. (London and Norwich: Jarrold and Son, 1887.)

The Land of the Broads. By Ernest R. Suffling. New Edition. (London: L. U. Gill, 1887.)

Three Weeks in Norfolk. By J. F. M. Clarke. (London: Wyman and Sons.)

A Month on the Norfolk Broads. By Walter Rye. (London: Simpkin, Marshall, and Co., 1887.)

Notes on the Broads and Rivers of Norfolk and Suffolk. By Harry Brittain. (Norwich: P. Soman, 1887.)

SURELY no spot in the British Isles has been so "beguided" as the Norfolk Broads. For the last twenty years the literature of the subject has been on the increase, till hardly a magazine or newspaper exists from *Blackwood* to *Exchange and Mart* which has not opened its pages to the flood of contributors on this apparently fascinating subject; and the whole has culminated in a shower of guide-books which enlivens the railway bookstalls with their gay exteriors, rendering it difficult to say which of the twain is the more largely advertised—Colman's mustard or the "Norfolk Broads." The bulk of the "articles" are of the feeblest sort by people who, having spent a few days on the Broads, returned to their distant homes imbued with the erroneous impression that they were qualified to enlighten the world with regard to the features and peculiarities of a tract of country difficult of access and still more difficult to appreciate, and the very names of whose towns and villages they had not learned to spell correctly. Some of the more pretentious productions, by the aid of excellent illustrations reproduced by various processes from photographs, and accompanied by maps, most of which have a more than family resemblance, appear to carry a weight of authority which their letterpress by no means warrants. Another feature which strikes the reader familiar with the country to which these articles refer is the supreme self-reliance of their authors; for although they contain in some instances the most barefaced plagiarisms, it is from one another that they borrow, and not from what may be termed the standard authorities on the subject, which probably some of the writers have never seen.

It is remarkable that Mr. Stevenson's general description of the "Broad District" in the introduction to his "Birds of Norfolk" (1866), perhaps the best ever written, appears to be quite overlooked, whilst the Rev. Rd. Lubbock's "Fauna of Norfolk" seems to be known only to Mr. Davies and Mr. Brittain, and of course also to Mr. Rye, who does not quote it simply because he has no necessity, owing to the plan of his book, to do so. A mere perusal of either of these two authorities would have saved some of the writers from committing what are palpable absurdities.

Mr. Davies's "Hand-book to the Rivers and Broads of Norfolk and Suffolk," first issued in 1882, and which has now reached its ninth edition, of course claims priority of notice both from its having been the first hand-book and from its general usefulness. It is needless to say that from the author's long and intimate acquaintance with the district his directions as to the best methods of procedure are all that could be desired. In reviewing the first edition of Mr. Davies's book the writer had occasion to make some observations upon the false impression conveyed by all the numerous writers on the Broads as to the supposed abundance of legitimate shooting to be had by the visitor; we are glad therefore to see that, to use his own expression, Mr. Davies has "put the break on a little" in the present edition, but we could have wished that in his remarks on "Shooting and Skating" (p. 170) he had omitted the following passage: "The usual plan is to row along the river while your dogs work through the reeds on the bank inside the river wall, or embankment, which generally runs parallel with the rivers on each side," and had confined himself to the sensible remark: "Don't take guns on board unless you have leave to shoot on somebody's land." The yachtsman may have the *right* to shoot in the navigable channel, but it is as discreditable to work with dogs along any proprietor's foreshore as it would be to do the same thing from the Queen's highway; and it is such acts as these, added to the many others which Mr. Davies schedules, which are gradually compelling the owners of the soil to assert their rights more and more stringently. We cannot agree with Mr. Davies that the disorderly conduct and depredations which are becoming more and more noticeable on the rivers are by any means "home products;" unfortunately there are some glaring instances of such improprieties by Norfolk men; but it is undoubtedly the visitors from a distance, here to-day and gone to-morrow, and who care not who may suffer for their rowdyism, who thus misconduct themselves. It has been our lot more than once to travel from London in a carriage full of young fellows bound for Norfolk for a trip on the Broads, and in each instance the gun-case and a liberal supply of cartridges has formed part of their outfit, and this probably in the close time. Their eager talk of the big bags and enormous catches of fish in prospect has often led us to wonder whether these sanguine young fellows were doomed to disappointment, or did their exuberant spirits and the joy and freedom of their untrammeled life on the water cause them to make light of such trifles as the non-fulfilment of their somewhat extravagant expectations? Big catches of fish are undoubtedly frequently made, but almost invariably by the skilled *habitué*, and very rarely by the casual visitor. Mr. Davies's book is increased from 108 to 173 pages, has twenty-three excellent illustrations and a capital folding map, and is altogether a very useful and readable book.

The second book on our list is entitled "The Land of the Broads," by Mr. Ernest R. Suffling. It originally appeared in 1885 and in a subsequent edition in a gorgeous cover, embellished by the portraits of a bird and a fish, the former a great improvement on nature, and the latter fearful to behold; a still later edition is in a prettily got up cloth cover, and the illustrations are for the most part excellent; the letterpress is also increased from 80 to 322 pages. Mr. Suffling's book is much more pretentious than Mr. Davies's, although he, like the rest of the authors we shall have to mention, has adopted the narrative form, a style excellent if accompanied by plenty of incident, but rather tame otherwise; it not only purports to be a guide-book to the Broads and rivers, but also to the principal towns and villages in their neighbourhood, and enters somewhat fully into the archæology of the district, having a special feeling for the churches. Chapters are also devoted to the Broad District in the seasons of spring, summer, autumn, and winter, the dialect and characteristics of the natives of East Norfolk, the fish, and how to take them, and a variety of other useful information. Mr. Suffling, although dating from London, claims to be a native of Norfolk, and internal evidence proves him to be no stranger to the country he writes about. His book is, therefore, free from many incongruities so jarring in similar books written by evident strangers to the places and people with regard to whom they undertake to instruct others. His chapter on the characteristics and dialect of the natives, whom we wish he would call "Marshmen," and not "Fenmen," the latter (inhabiting quite another part of the country, and of Girvian descent), a much inferior people in many respects to the hardy inhabitants of the Broads. In the introduction to the first edition Mr. Suffling asks for corrections of inaccuracies, and in the subsequent edition acknowledges that one or two errors have been pointed out to him. He will, we are sure, therefore excuse our making a few remarks which may be of service to him in a future edition. Mr. Suffling appears very loose about his natural history observations, and when he speaks of the decoys which still linger in this county (p. 28) he is altogether at sea. This is inexcusable, for, as a native of the Broads, he certainly ought to be acquainted with Mr. Lubbock's charming "Fauna of Norfolk," in which so long ago as 1845 a full explanation of the mode of working these ingenious contrivances was given, not to mention Sir Ralph Payne-Gallwey's more recent and exhaustive work on the same subject. The great essential of a decoy is absolute quiet and freedom from disturbance both inside and out; the ducks are taken by *decoying* them into the pipe, not by driving, and the dog is used for the purpose of exciting the curiosity of the fowl, which *follow* him up the pipe; it is not till they are so far up the pipe as to be hidden by its curve from the fowl that are hanging about its mouth, that the decoy-man shows himself, and then it is done in such a way as not to alarm the fowl outside the pipe which he hopes to entice later on. A boat would destroy the sport for many a day, perhaps for the remainder of the season; a dog which entered the water would be hanged at once, and decoy-ducks which rushed into the purse-net with the wild ones would be useless; their business is not to follow the wild fowl too far up the pipe, but to remain quietly on the water when the man shows himself. We cannot imagine what bird is meant by the "long-winged owl" (p. 4), which is said to be "the most destructive of its tribe." We presume *Arvancusis*, at p. 7, should be *Mastodon arvernensis*. The tools mentioned as found by Canon Greenwell, at p. 8, were not in a "barrow," but in the working of an ancient chalk-pit. The Honorary Secretary to the Yare Preservation Society will be delighted to receive contributions (p. 31) for providing river-watchers, not the Board of Conservators, a totally different body with perfectly distinct functions. The so-called "monkey house" mentioned at p. 61 is a very modern erection, built by the late Sir Robert Harvey as a ferry house, and is altogether innocent of the days of "good Queen Bess." The kingfisher is known to naturalists as *Alcedo ispida*, not *hispida* (p. 143), (surely the "halcyon days" could not have been suggestive of roughness in any sense of the word!), and the cuckoo does not turn out the eggs from the hedge-sparrow's nest and deposit a *clutch* of its own in their place, as implied at p. 217, but lays a single egg, the young one hatched from which subsequently appropriates the nest entirely to its own use by turning out the eggs or young of its foster parents. It was the father of the late Mr. Robert Rising who purchased the Horsey estate (p. 199), and the fine collection of local birds was dispersed by auction in September 1885. As to the story of the Bishop of Norwich being the only abbot left in England, and sitting in the House of Lords by virtue of that office (p. 126), Mr. Walter Rye, no mean authority on Norfolk archæology, can find no foundation for such a statement, and believes it to be "just as true as the tale that William the Conqueror besieged the place, and that a recreant monk who betrayed it to him was first made abbot and then hanged by him." There are many other little matters which might be amended in a future edition, but space forbids our referring to them.

The next book on our list is "Three Weeks in Norfolk," by Mr. J. F. M. Clarke, a book chiefly devoted to a narration of the troubles experienced by the voyagers, owing, in the first instance, to an unpunctual skipper, and subsequently to a drunken one. Mr. Clarke certainly seems to have been very unfortunate in this respect, but, judging from our experience of these men, much of the trouble may have arisen from his friend H. addressing the man "sternly, in the cold unemotional tones characteristic to him in moments of wrath," a mode of proceeding not likely to be appreciated by men of his class; probably more *suaviter in modo* would have been attended with greater success and less final loss of dignity. Doubtless yachting men will be deeply grateful for the lucid explanation of the art and mystery of "tacking," accompanied by a diagram, given at p. 13. The author tells a tale of a boy of whom he "made free to ask in a spirit of banter" what fish he had caught; the reply was, in an equally "bantering" vein, "half a last." The author remarks that as he "for the moment forgot that the 'last' meant 4000 lbs.," he was without retort, and the boy gained the day. Had our author been a Norfolk man, he would have known that a "last" is 13,200 fish. Mr. Clarke seems surprised that he should meet with a gentleman in the wilds of Norfolk. Whether Mr. Girling will recognize his own portrait or not we cannot say, for he is a modest man, but such men are happily far from uncommon in this county. Scarcely in better taste are the remarks with regard to that "tal graceful, fair, and, above all, refined" creature whom he christened "Evangeline." Such remarks could hardly be very edifying to a rustic village maid should they mee her eye; but it may interest Mr. Clarke to know that th "being so refined, with so much polish of manner, an having so good a taste in dress," and her fond mamm have disappeared from Wroxham, and we believe hav left no address.

"A Month on the Norfolk Broads," by Mr. Walter Rye, i a book of quite another kind, as might be expected from s accomplished a writer. The "poet, the liar, the athlet and the antiquary" form an exceedingly well assorted crew; and the addition of an American gentleman and his wife visiting Norfolk, whence his people originally came, "on genealogical searches intent," helps to make the fun of the voyage greater. From the first page to the last the pleasant banter never flags, and there is more real information both with regard to the topography, antiquities, and local peculiarities of the country through which they passed conveyed in this agreeable manner than in many a book of greater pretensions; whilst his specimens of the Norfolk dialect, as given in the story of the ghost of Irstead Shoals, and other passages, are really excellent. The account of "Roger's Blast," at p. 51, and the adventure with the otter (p. 53), are exceedingly clever satires on the writings of a well-known author of "Broad" books, and the *finale* of the "Ancestor Hunt" is exquisite. The writer also ventures to tell the truth with regard to the too much vaunted shooting and fishing. The trip ended, as we suspect many another has done both before and since, by the companions getting just the least bit tired of each other, and the Americans departing to the much more congenial region of Scarborough, whilst the rest of the party returned to London. The maps are very useful, although mere outlines, and the pretty little sketches by Mr. Wilfred Ball charming.

The last book on our list is "Notes on the Broads and Rivers of Norfolk and Suffolk," by Harry Brittain, with sectional maps and illustrations. Like the preceding guide-books Mr. Brittain has adopted the narrative form, and conducts the reader in a very pleasantly written journal to all the principal points of interest on the rivers and Broads, with a sea trip to the quaint old towns of Dunwich and Southwold. Altogether Mr. Brittain has contrived to embody an immense amount of information in his 154 pages, including lists of fishing quarters, distance tables, table of high water at Yarmouth Bar, and a copy of the Bye-laws of the Conservators under the Norfolk and Suffolk Fisheries Act of 1877. He has, like Mr. Davies, the advantage of being a local man and an enthusiastic yachtsman, thoroughly familiar with the country, and therefore perfectly reliable; the illustrations are excellent and thoroughly characteristic, and the sectional maps, with which the text is interspersed, will be found exceedingly useful.

Some years ago a very florid article on the Broads appeared in a magazine giving such a glowing description of the abundance of fish that pike, it was said, were actually used for manuring the land, and the shooting was not less remarkably productive. The result was that a well-known naturalist residing in Norwich was flooded with letters of inquiry as to fishing and shooting quarters in this *El Dorado* of sport. His reply was that undoubtedly both fish and fowl were there, and that at certain seasons good bags of both could be made, but that unfortunately there were people selfish enough to imagine that they had some sort of proprietary right to what was found on their own land or in their own water; and as to trespassing on the snipe grounds which surround the Broads, so little right had the public that if any unfortunate individual should chance to fall into the water he must remain there till he had written to the owner of the soil for permission to land. This is literally true, with the exceptions of the towing-paths in the navigable rivers; and visitors, whilst seeking the healthful pleasure undoubtedly to be derived from a trip on the Norfolk Broads, should be careful to respect the property and rights of the riparian proprietors.

OUR BOOK SHELF.

Connaissance des Temps, ou des Mouvements Célestes à l'usage des Astronomes et des Navigateurs pour l'an 1888, *publiée par le Bureau des Longitudes.* (Paris: Gauthier Villars.)

THIS valuable ephemeris has now reached its 210th volume in unbroken annual succession since its first publication by Picard in 1679. Its form and contents have undergone a wide development since that date, a development which is still in progress, for the present volume shows three additions on those of previous years. These are (1) the insertion of local time of the moon's transit for twenty-four successive meridians; (2) a development of the tables for transforming sidereal into mean time, and reciprocally, so as to render the performance of the calculation more rapid; and (3) the insertion of the co-ordinates of 65 southern stars, 5 being circumpolars for which ephemerides are given from day to day, the co-ordinates of the remaining 60 being supplied for every tenth day. The positions of the stars have been drawn from all the existing Catalogues, and from unpublished Cordoba observations communicated by the Director of the Cordoba Observatory, M. Thome.

A Treatise on Analytical Statics. With numerous Examples. By I. Todhunter, M.A., F.R.S. Fifth Edition. Edited by J. D. Everett, M.A., F.R.S. (London: Macmillan and Co., 1887.)

MESSRS. MACMILLAN have just issued the fifth edition of the late Mr. Todhunter's work on analytical statics, edited by Prof. Everett. In his preface the editor states that the most important changes he has made in the old matter relate to attraction, virtual velocities, and general theorems on systems of forces. He has added a brief chapter on graphical statics, a series of articles on the connexion between centres of gravity and resultants of forces at a point (with an exposition of vectors), and a new theorem on a string under a central force. The omissions include most of the articles on the attraction of ellipsoids, in conformity with the design of the book in its present form, which is intended to contain such a selection of subjects as may with advantage be studied in a first course of analytical statics.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Measurements of the Heights and Motion of Clouds in Spitzbergen.

THE first measurements, as far as I know, of the heights and motion of clouds, by the method described by the Hon. R. Abercromby in NATURE for August 4 (p. 319), and practised at Upsala by M. Hagström and myself since the summer of 1884, were made in the summer of 1883 at Cap Thorelsen in Spitbergen under the Swedish Polar Expedition stationed there, of which the chiefship as well as the guidance of the meteorological observations had been committed to me by the Swedish Academy of Sciences. Those measurements having been made by the same instruments[1] and the same method as the Upsala observations, it will perhaps interest you to see some of the results. The measurements will soon appear *in extenso* in the publication of the works of the Expedition.

Mean, greatest, and least heights of clouds at Cap Thorelsen (above the mean level of the sea).

Name of Cloud.	Number of Measurements.	Number of Clouds.	Height in metres. Mean.	Max.	Min.
Strato-cumulus	6	6	2464	3123	2032
Alto-cumulus	16	13	3229	5306	2126
Cirro-cumulus	7	3	6389	7411	5180
Cirrus	15	7	7317	8590	5676

For want of time the number of observations was rather small, but nevertheless the heights agree tolerably well with those obtained afterwards at Upsala. The mean error of a single determination of lower clouds (below 3500 metres), I have found to be 3·4 per cent. of the height of the cloud, that of a higher cloud (above 4700 metres) to be 16·6 per cent. We had two bases, but the longer one was not more than 572·6 m., as I could not, for that purpose, dispose of a greater length of wire for the telephonic line. This explains the great mean error found for the higher clouds. The greatest velocity observed for higher clouds was 27 m. per sec. at a height of 7300 m. The calculations are made by the method worked out by M. Hagström and myself in the summer of 1884, and fully described in our first paper on the subject ("Mesures des Hauteurs et des Mouvements des Nuages" in *Nova Acta* Reg. Soc. Sc. Ups., ser. iii., Upsala, 1885). NILS EKHOLM.

Upsala, August 24.

Occurrence of Apatite in Slag.

I SHOULD like to be permitted to ask whether any mineralogical readers of NATURE have themselves come across, or have anywhere seen mentioned, the occurrence of crystallized apatite in a metallurgical slag or other artificially-formed silicate?

Having recently observed such an occurrence, I have been looking into the authorities I have at hand here to see whether any similar formation is previously recorded. The result is negative, and indeed Rosenbusch ("Mikroscopische Physiographie der petrographisch wichtigen Mineralien," 1885), after enumerating the various artificial preparations of the mineral, distinctly states that "the formation of apatite from fused silicate magmas has not yet succeeded." My former teacher, Prof. Weisbach, of Freiberg, who takes special interest in artificial formations of minerals, and carefully records all cases coming to his knowledge, writes to me that he is not aware of any instance of the occurrence of apatite crystals in a slag except in the case of the "Thomas slags" of the basic Bessemer process. These can, of course, not be classed as "silicate magmas," containing as they do so large a proportion of phosphoric acid, and a relatively small amount of silica; nor do they bear any analogy whatever to the silicate rocks in which we are accustomed to observe the occurrence of apatite in Nature.

The slag in which I have observed the formation of apatite is produced during the smelting of lead ores in a blast-furnace. It is a basic silicate of lime and ferrous oxide, containing about 30 per cent. of silica. The principal "flux" used in the reduction of the ore is "tap-cinder" from the puddling furnaces, and it is mainly from this source that phosphoric acid is introduced into the slag. The slag itself, in bulk, is dark brown to nearly black in colour. It flows into slag-pots of about 3 cwts. capacity, and cools slowly.

I recently prepared some thin sections of this slag for microscopic examination. The greater portion consists of a mass of crystals of olivine, surprisingly colourless and transparent considering how much iron is present. The spaces between the crystals are occupied by deep-brown and yellow amorphous slag, and black sulphides of iron, &c.

Both olivine crystals and dark amorphous matter are penetrated through and through by great numbers of apatite crystals in long needles. It is a most beautiful occurrence, analogous in every way to what one sees in rocks.

[1] The altazimuths employed were constructed by Prof. H. Mohn in Christiania for the use of the Norwegian, Swedish, and Danish Polar Expeditions.

Nearly all the apatite crystals have taken up and inclosed more or less of the amorphous dark material, which forms in the majority of cases a rod running down the centre, but there are also many cases of symmetrical arrangement of dark matter parallel to the sides of the hexagon.

The apatite does not only occur in the mass of the slag as above described; it is formed also in free crystals, lining cavities which are formed in the centre of the lumps of the slag owing to gases carried over from the furnace and liberated during cooling. Some of these cavities are of considerable size, and are often lined entirely with a thick growth of apatite needles, some as thin as the finest hair, others of much larger dimensions. I have taken out crystals over a quarter of an inch long for microscopic and chemical examination. Most of them contain a good deal of the amorphous slag, &c., inclosed, as in the case of those in the mass of the slag.

Sometimes in such cavities very beautiful little crystals of volatilized sulphides are seen among and on the apatites. I have seen galena crystals in this manner, but it is very difficult to remove them from the cavities without damage or loss.

It appears to me strange that while we have here so plentiful a formation of apatite going on constantly, in many tons of slag daily, it should still be on record that experiments purposely conducted with a view to obtaining the mineral in a silicate magma have not succeeded. W. M. HUTCHINGS.

Eversley Park, Chester, September 3.

Electricity of Contact of Gases with Liquids.

MAY I be allowed to reply to Dr. Lodge (NATURE, Sept. 1, 412)? That the escaping gas was charged was proved (1) by *collecting* it in an insulated vessel; (2) by *generating and collecting* it in insulated apparatus. Electrification resulted in the first case, but not in the second. Details of these experiments I intend to publish later.

With all possible respect for Dr. Lodge I cannot accept his explanation of the electrifications I have described. I fail to see any analogy between Armstrong's machine and the experiments with zinc and hydrochloric acid which I have made, except, indeed, that, as I hold, they are all cases of contact electricity. Sir W. Armstrong directs a jet of steam against solid wood grooved and shaped to increase the surface of contact. In my experiment, on the contrary, I find that the effect is distinctly lessened when the hydrogen passes through narrow tubes or openings, and accordingly the strongest deflections are obtained when a large open dish is used. I take an evaporating dish, 8 or 10 inches in diameter, and put a small quantity of a 10 or 16 per cent. solution of HCl into it. The acid is at every point at least 4 or 5 inches distant from the edge of the dish. When a small fragment of zinc is thrown in a gentle effervesence is set up, the hydrogen shooting straight up through the middle of the liquid into the air. I submit that in this experiment whatever electrification results is due to gas and liquid, and not to gas and solid. Moreover, after a lapse of a few minutes when a sufficient quantity of $ZnCl_2$ gets into solution, the charge on the dish changes its sign. Is Armstrong's machine also in the habit of reversing itself? On this reversal of the electrification I base my case. The dish does not vary, but the liquid does, and its variation is accompanied by a change in the sign of the charge.

With regard to the atomic charges I do *not* hold that the charge on the hydrogen has anything to do with them. It is true that at first I set out to inquire as to the equality of these charges, but when I found that the charges on the evolved gas and the generator were of opposite sign, I was constrained to admit that the electrifications were not connected with the atomic charges as I had, during the earlier experiments, supposed. Had I found things the other way—that is to say, that the charge of the gas had been of the same sign as the charge on the generator—I should not have thought it at all improbable that the electrifications were due in some way to atomic charges. In fact, at the outset this is exactly what I thought I might possibly fall upon. However, the unexpected occurred.

"But that a gas should be thus electrified strikes one as improbable." Can this be so? Surely the distinguished champion of the "air effect," who has so stoutly maintained that the contact of copper or zinc with gas gives rise to a difference of potential, cannot consider it very improbable that the contact of a gas and a liquid should produce a similar effect. Perhaps it is well to remember that the hydrogen in these experiments is in the nascent state. J. ENRIGHT.

Cocoa-nut Pearls.

REFERRING to the letter of Dr. Sydney J. Hickson, published in your paper of June 16 last (p. 157), I have the pleasure to communicate to you that I have a collection of fourteen cocoa-nut pearls (one of them I myself found in 1866 at Holontalo, North Celebes, in the endosperm of the seed of the cocoa-nut); two melati pearls (*Jasminium sambac*); one tjampaka pearl (*Michelia longifolia*), found in the flowers, according to the natives. One of the cocoa-nut pearls has a pear-shaped form, the length being 28 mm. The common name amongst the natives for this kind of pearls is mustika.

Utrecht, September 6. J. G. F. RIEDEL.

STARS WITH REMARKABLE SPECTRA.

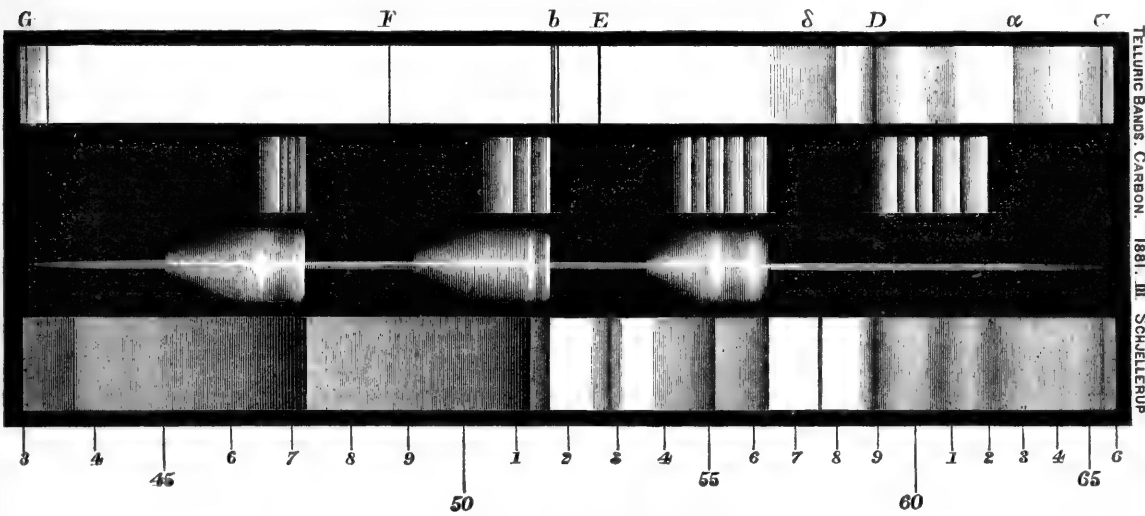
I.

No. 152 *Schjellerup* (*D.M.* + 46° *No.* 1817).

Place 1887·0, *R.A.* 12*h.* 39*m.* 47*s.*, *Decl.* 46° 3'·5 *N.*

THIS star, No. 290 in Mr. Birmingham's Catalogue of Red Stars, may very fittingly be taken as a sample of the stars possessing spectra of the fourth type, to use Secchi's nomenclature, or of the second division of the third type, to follow Vogel's—spectra, that is, in which the prominent feature is a series of dark bands alternating with bright spaces, and in which the dark bands are, as a rule, sharp and dark on the less refrangible side, or that nearer the red, but which gradually fade away into nothingness on the more refrangible side, or that towards the violet. The present star, though not perhaps the one in which the series of bands is most completely developed, has yet a spectrum which is a very beautiful example of the type; the bright interspaces, or zones as they are technically called, are vivid and striking, and the bands broad and dark, and it possesses the additional advantage that, though only of magnitude 5·5, it is yet the brightest star of its class in the northern heavens.

The purpose of the accompanying diagram, in which the spectrum of 152 Schjellerup is seen side by side with that of Tebbutt's comet of 1881, and with a particular carbon spectrum, is to bring into prominence the meaning of the remarkable series of shaded bands which characterize it. In 1869, Secchi had declared that these bands coincided as to position with the bands of the carbon spectrum; but, as Dr. Huggins shortly after stated that he had compared the spectrum of carbon with that of a red star, and found that the two did not coincide, it was generally assumed that the Italian observer was mistaken, the well-known skill and accuracy of the great English spectroscopist rendering it very unlikely that his observation should be in error. As the event proved, both were right; it was only the natural inference that the two observations were contradictory that was at fault. Our knowledge of the beautiful and complicated spectra of carbon had not then attained its present precision, and it escaped remark that the spectrum with which Secchi had compared the red stars was not the same that Huggins had used for that purpose. Even now spectroscopists are not wholly unanimous as to whether we should regard these two spectra as both belonging to elemental carbon at different temperatures, or as belonging to two different classes of carbon compounds—those with oxygen and those with hydrogen. The spectrum which Secchi had used was that which, according to Thalén and others, characterizes the hydrocarbons; whilst Huggins used that of the oxides.



SCALE OF WAVE-LENGTHS.

The former spectrum is one which was already of high importance to the astronomer. Huggins had shown, in 1868, by comparing Winnecke's comet with olefiant gas, that the three bright bands so typical of a comet coincided precisely with this form of the carbon spectrum; and now Dunér and Vogel have placed it beyond a doubt that in the spectrum of the red stars we see the same spectrum, only reversed—an absorption instead of an emission spectrum. The agreement as to the place of the sharp, well-marked, less refrangible edge of the three principal bands—the yellow, the green, and the blue—is exact within the limit of errors of observation; the shading-off towards the violet is similar in character, and there are indications of the presence of some at least of the secondary flutings which in the carbon spectrum follow the great leaders of the bands in so charmingly rhythmical a manner. The orange band also, placed in a fainter part of the spectrum, and so more difficult to observe, is present, there can be little doubt, in the absorption spectrum of the red stars, though its bright analogue has seldom been satisfactorily traced in the spectrum of a comet; the violet band, on the other hand, appears to have been better seen in the comet than in the red star.

The following table will show the character of the correspondence of the principal bands of the three spectra—

hydrocarbon, comet, and star. The wave-lengths are expressed in millionths of a millimetre :—

Colour of band.	Carburetted hydrogen. Thalén.[1]	Comet 1881 III. (Tebbutt's). Copeland.[2]	Maunder.[3]	Typical red star. Dunér.[4]	Vogel.[5]
Orange	618·7		"A band about half-way between C and D."	621·0	622·0
	611·9				
	605·6			604·8	606·5
	600·1				
	595·4				
Yellow	563·3	563·2	563·0	563·3	563·1
	558·3	555·4			
	553·0				
	550·0			551·0	552·0
	546·6			545·0	544·0
Green	516·4	516·7	516·3	516·3	515·9
	512·8	513·4			513·2
	509·8				
Blue	473·6	473·3	473·4	472·7	472·9
	471·4				
	469·7				
	468·2	467·5			
Violet	431·1	430·2	"A band in the violet near G."		End of spectrum. 430·0

Beside the above, there is in the spectrum of the star a faint band in the violet at λ 437·0, which agrees according to Vogel, with a hydrocarbon band, not included in the above series.

The carbon bands thus account for the best-marked of the dark bands characteristic of the type, but there are three or four bands of a slightly different character which do not fall into the series. Thus, the green zone is interrupted by a narrow band at λ 528·0, and the yellow zone by another at λ 575·7 somewhat similar, both of which remain at present unexplained; and in the orange and red we find two bands in which the abrupt commencement on the redward side, and the gradual shading off towards the blue, is no longer apparent. The darkest part of the orange band is, indeed, near its centre, a dark line, coincident, there is scarcely any room to question, with the giant doublet of sodium, the great D lines of the solar spectrum. The red band, though without a nucleus which can be identified as a typical line of this or that element, gathers round the site of the red hydrogen line C. The two bands therefore strongly recall, though the resemblance may perhaps be a misleading one, the great water-vapour groups around C and D in the absorption spectrum of our own atmosphere. The dry-air bands a and δ—A and B being out of sight in the extreme red—do not appear to be represented. With a view to exhibit the relationship of these telluric bands to those in the less refrangible part of the spectrum of the typical red star, an outline of the solar spectrum has been added to the diagram, and the positions of the great Fraunhofer lines and of the principal bands due to the absorption of our own atmosphere have been indicated.

THE BRITISH ASSOCIATION.

SECTION D.

BIOLOGY.

Opening Address by Alfred Newton, M.A., F.R.S., F.L.S., V.P.Z.S., &c., Professor of Zoology and Comparative Anatomy in the University of Cambridge, President of the Section.

In opening the business of this Section I cannot but call to mind the last occasion when the British Association met in the city of Manchester, just six-and-twenty years ago; and, while my memory brings back to me many pleasing recollections of that gathering, I cannot help dwelling upon the extraordinary difference between the state of things that then existed and that which we have before us to-day. The moral of the contrast I shall not seek to enforce. Those, if any there still be, who despair of the future of our Association may reflect upon it at their leisure; while those who believe, as I do, that our Association has no justifiable cause for thinking that its work is accomplished, that it had better settle its worldly affairs, and compose its robes around it in a becoming fashion, before lying down to die, will at once appreciate the difference.

Yet there is one difference between our proceedings to-day and those of more than a quarter of a century since which I, personally, do not appreciate. In that remote and golden age it had not become obligatory on the President of this Section to prepare beforehand an address to be delivered to a critical, even though kindly, audience. A few words of friendly greeting to old faces, and a hearty welcome to those that were new, with a general statement of the objects of our coming together, comprised all that was expected from the occupant of the chair. Such was my case when my predecessor, who was, I may observe, my excellent friend and colleague, Prof. Babington, opened the proceedings of this Section—then called the Section of Zoology and Botany—at Manchester in 1861; and I am sure I have reason to envy his happy lot, for, on refreshing my memory by turning to the report of that meeting, I find that his introductory "remarks" occupy a space of less than eight lines of print. In this respect, but in this only, I must confess myself *laudator temporis acti*, and it having now been for so many years the practice of your President to deliver an address on occasions like the present, I feel that I should be filling my position under false pretences did I not conform to established usage, though I am well aware that what I have to say will, for many reasons, hardly bear comparison with what has been said by many of my distinguished predecessors.

But to continue the contrast of what took place in this Section at our last meeting in Manchester with what may be expected to happen now, I would remark that the year 1861 was one which, when the history of biology comes to be written, will be found to deserve particular recognition. This is not merely because of the all-important discovery of *Archæopteryx*, for that had not been made known when the Association met, and did not affect our proceedings here. When we met, it was a time, so to speak, of "slack water"; but slack water is commonly the effect of two contrary streams, and perhaps I ought to state how this came about. All present should be aware that it was before the Linnean Society on July 1, 1858, that the stupendous announcement was made of a theory which for the first time brought to the notice of biologists a reasonable explanation of the mode by which what had hitherto passed under the name of the transmutation of species could be effected. It is notorious that this announcement attracted but little attention at first, and, though it were easy to account for this fact, I see no need to occupy your time by so doing. I would, however, beg your attention to another fact which is by no means notorious. So far as I am aware, the first zoologist publicly to accept and embrace the theory propounded on that memorable evening on behalf of Mr. Darwin and Mr. Wallace, was my old friend Canon Tristram, and moreover he did this ere little more than a twelvemonth had expired (*Ibis*, October 1859, pp. 429-433). To me it will always be a matter of rejoicing that the adoption of this theory was so early accepted, and additional evidence in its favour adduced, by one who has devoted so much time and energy to the particular branch of zoology which has long recommended itself to me; for thereby I hope that the study of ornithology may be said to have been lifted above its fellows. This, however, is a digression, for introducing which I trust I may be pardoned. And now to return to my main business. Late in the autumn of 1859, as you know, Mr. Darwin's essay on the "Origin of Species" appeared—a mere abstract, as it still remains, of an enormous mass of materials industriously accumulated by him through many long years—a mass out of which, as he himself has modestly said, a competent man might have written "a splendid book"—but a mass with which he, chiefly through ill-health, had been unable to deal properly. Yet I am not sure that we have any reason to lament the result. The handy size of that celebrated little volume gave it a power of penetration and circulation that would not have been possessed by a work of greater bulk, while the studied absence of tech-

[1] "Récherches sur les Spectres des Métalloides."
[2] *Copernicus*, vol. ii. p. 227. [3] *Observatory*, vol. iv. pp. 305, 306.
[4] "Sur les Etoiles à Spectres de la Troisième Classe," p. 122.
[5] "Public. des Astroph. Obs. zu Potsdam," vol. iv. p. 31.

nicalities and of reference to scientific authorities in the form of foot-notes (which last, I need scarcely point out, would have largely increased its dimensions) brought its closely-reasoned argument within the comprehension of hundreds whom it would have at once repelled had it been made up of learned phraseology.

Much of what followed on the publication of this work will be in the recollection of many of my audience, while the rest must have heard of it from their seniors. The ever-memorable meeting of this Association at Oxford in the summer of 1860 saw the first open conflict between the professors of the new faith and the adherents of the old one. Far be it from me to blame those among the latter who honestly stuck to the creed in which they had educated themselves; but my admiration is for the few dauntless men who, without flinching from the unpopularity of their cause, flung themselves in the way of obloquy, and impetuously assaulted the ancient citadel in which the sanctity of "Species" was enshrined and worshipped as a palladium. However strongly I myself sympathized with them, I cannot fairly state that the conflict on this occasion was otherwise than a drawn battle; and thus matters stood when in the following year the Association met in this city. That, as I have already said, was a time of "slack water." But though the ancient beliefs were not much troubled, it was for the last time that they could be said to prevail; and thus I look upon our meeting in Manchester in 1861 as a crisis in the history of biology. All the same, the ancient beliefs were not allowed to pass wholly unchallenged; and one thing is especially to be marked—they were challenged by one who was no naturalist at all, by one who was a severe thinker no less than an active worker; one who was generally right in his logic, and never wrong in his instinct; one who, though a politician, was invariably an honest man—I mean the late Prof. Fawcett. On this occasion he brought the clearness of his mental vision to bear upon Mr. Darwin's theory, with the result that Mr. Darwin's method of investigation was shown to be strictly in accordance with the rules of deductive philosophy, and to throw light where all was dark before.

Now the reason why I have especially mentioned this essay of Prof. Fawcett's is not merely that the approval of the disputed theory by such a man did not a little contribute to the success which was then impending, but because I have for a long while maintained that, as a matter of fact, what is now known as the Darwinian theory did not, except in one small point, require a naturalist—and much less naturalists of such eminence as Mr. Darwin and Mr. Wallace—to think it out and establish its truth. Pray do not for a moment imagine that I wish to detract from the value of their demonstration of a discovery that is almost unrivalled in its importance when I say that the demonstration might have been perfectly well made by any reflective person who was aided by that small amount of information as to the condition of things around him which is presumably possessed by everybody of common sense. It might have been perfectly well made by any of the sages of antiquity. It might have been as well made by any reasoning man of modern time, even though he were innocent of the merest rudiments of zoology or botany; and, as is admitted, the discovery was partly and almost unconsciously made by Dr. Wells in 1813, and again by Mr. Patrick Matthew in 1831—neither of whom pretended to any special knowledge of those branches of science. It is equally a fact that anyone who applied the doctrine of Malthus, the political economist, to the animal and vegetable populations of the world, could have seen that what came to be called "natural selection" was the necessary consequence of the principles enunciated by him; and we have Mr. Darwin's acknowledgment that his reading the "Essay" of Malthus was with him the turning-point which settled his conviction as to the soundness of the crude speculations in which he had been indulging. Moreover, years before Malthus wrote, a great French writer, though no naturalist, had pointed out, in terms that were *mutatis mutandis* repeated as regards plants at a later time by the elder De Candolle, that all animals were perpetually at war; that each, with a few exceptions, was born to devour others; and that the males of the same species carried on an internecine war for the females.[1] The fact of the "struggle for life" being thus recognized, all the rest should follow, and really no close acquaintance with natural history was needed to guide an investigator to the end so far reached.

But in order to see the effect of this principle upon organic life the knowledge—the peculiar knowledge—of the naturalist was required. This was the knowledge of those slight variations which are found in all groups of animals and plants—a point on which I need not now dwell, for to my present audience it must be known in thousands of instances. Herein lay the triumph of Mr. Darwin and Mr. Wallace. That triumph, however, was not celebrated in Manchester. The question was of such magnitude as to need another year's incubation, and the crucial struggle came a twelvemonth later, when the Association met at Cambridge. The victory of the new doctrine was then declared in a way that none could doubt. I have no inclination to join in the pursuit of the fugitives.

But in tracing briefly, as I am now doing, the acceptance of the teaching of Mr. Darwin and Mr. Wallace, there is one point on which I should like to dwell for a few moments, because it has, so far as I know, been very much neglected. This is the great service rendered to the new theory by one who was its most determined opponent, by one of whom I wish to speak with the utmost respect, by one who was thoroughly a philosophical naturalist, and yet pushed his philosophy to overstep the verge of—I fear I must say—absurdity. I mean the late Prof. Louiz Agassiz, whose labours in so many ways deserve far higher praise than it is in my power to bestow. There must be many here present who will recollect the time when the question "What is a 'Species'?" was always coming up to plague the mind of every zoologist and botanist. That question never received a definite answer, and yet every zoologist and botanist of those days felt that an answer ought to be given to it; for without one they knew that they were sailing on an unknown sea, and that theirs was likely to be lost labour. The chief reason why no answer was given lay in the fact that hardly any two zoologists or botanists could agree as to the kind of reply which should be made, for hardly any two of them could agree as to how a "Species" was constituted. It will be enough for me to say now that Louis Agassiz pinned his faith on every "Species" being not merely the result of a single direct act of creation, but, when he found that physical barriers interposed (as they often do) between two or more parts of the area which the "Species" occupied, he did not hesitate to declare that a "Species" might have been created directly in several places, at sundry times, and even in vast numbers. If the same Species of freshwater fish, for instance, was found in several rivers which had no intercommunication, it had been, he asserted, separately created in each. Before his time people had been content to talk of each Species having had a single birthplace—its own "Centre of Creation"—but he maintained that many Species must have had *several* Centres of Creation, and creation was in his mind no figurative expression. He meant by it, just as Linnæus before him had meant, a direct act of God; in other words, his belief was that there had been going on around us a series of mysterious performances, not one of which had ever been consciously witnessed by a human eye, but each of which had for its object the independent formation of a new living being, animal, or plant. That is to say, that there had been going on from time indefinite a continuous series of operations which could only be termed miraculous, since there was no known natural law by means of which they could be produced. Though the author of this theory was, in the country of his adoption, regarded as the especial champion of opinions that are commonly termed orthodox, it is not surprising that many minds revolted from such a conclusion as it required—a conclusion which they not unfitly deemed a *reductio ad absurdum*. Yet the position of Prof. Agassiz was perfectly logical when once his premises were admitted; and, more than that, it became obvious to all clear-seeing men that one of these alternatives must be adopted—either Agassiz's logical doctrine of centres of creation, or the theory of the transmutation of species, which had been so long condemned because no reasonable explanation of its *modus operandi* was known.

I have called these alternative opinions because I believe that no third course had been suggested by any naturalist, and yet it is hard to say which of them was most unpalatable to the world at large. On the one hand, people were called upon to believe that man was in some inexplicable and unaccountable way produced from a monad. On the other hand, they were called upon to believe that the inhabitants, vegetable and animal, whether

[1] "Tous les animaux sont perpétuellement en guerre; chaque espèce est née pour en dévorer une autre. Il n'y a pas jusqu'aux moutons et aux colombes qui n'avalent une quantité prodigieuse d'animaux imperceptibles. Les mâles de la même espèce se font la guerre pour les femelles, comme Ménélas et Paris. L'air, la terre, et les eaux sont des champs de destruction."—Voltaire, "Questions sur l'Encyclopédie par des Amateurs," article "Guerre."

bestial or human, of nearly every group of islands in the Pacific Ocean were the result of innumerable special acts of creation entirely performed within the limits of almost each cluster of coral reefs. The natural consequence of this was that most people, and even most biologists, remained in an apathetic if not an unthinking condition on this subject, and went on as their fathers had done, not caring to trouble themselves in this matter. It was only a few—an extremely few—among them who ever gave the question any consideration at all, and these few were not so much the men who had confined their labours to museums, libraries, or laboratories, but they were, with scarcely an exception, men who had studied Nature in the field, and had seen her works under varied aspects in the most distant and diverse climes. They were men who had personally compared the geological formations of the Old World and the New, men who had circumnavigated the globe, who had surveyed Antarctic volcanoes or Himalayan snows, who had dredged the depths of Australian oceans or had explored Amazonian forests. Out of the abundance of their observation and reflection these men—to this audience I need not name them—in due time delivered their verdict, and when it was delivered its effect was crushing. The position of the supporters of the doctrine of "Centres of Creation," logical as it had seemed, was swept away—not of course without a gallant struggle on the part of its defenders—and the theory of the "Transmutation of Species," fanciful and unreasonable as it had been thought, was under a new name established, the very fact of its success being an additional proof of, to use Mr. Herbert Spencer's happy phrase, the "Survival of the Fittest."

But perhaps some of you have been thinking or whispering to your neighbours, "Why should our President be taking up our time by making us listen to all these platitudes, this old story with which we are all familiar?" and if you have been so doing you will have some excuse, but I trust you will think that I also have some excuse in thus recurring to what may be almost deemed a portion of ancient history when I state that in my belief this year 1887 will in future be remembered as that in which "The Life and Letters" of our great biologist, Charles Darwin, appeared; and I hope that in a few minutes you will admit that in accordance with the fitness of things it is meet and right that this should be so. There can be little doubt that before the end of this year that work which all naturalists have been expecting with so much anxiety will be published, and published, moreover, in three languages. It can hardly fail to be accounted by biologists as the chief event of the year. By favour of its author, Mr. Francis Darwin, I have been allowed to see some of his proof-sheets, and I am sanguine that it will not disappoint the expectations of its readers. On one point I venture to speak with some certainty. The noble character of the man will be made manifest to the world in words and deeds that cannot be spoken against, and we may feel assured that in future

> "Whatever record leap to light,
> He never shall be shamed."

He is unsparing of his own mistakes or shortcomings; and, while admitting with the utmost generosity the assistance he received from others, the dignified way in which he thought of and expressed himself toward the many who attacked him, often unscrupulously and in a manner which he could not but deeply feel, will ever redound to his credit, and prove him to have been that great philosopher which all his friends and adherents would wish to believe him. Do not mistake me, however, in one respect; there where times when he "did well to be angry"; but his anger was slowly excited, and his occasional vehemence soon subsided into his wonted calm. More than all this, you will find that the childlike simplicity of his mind and the single-heartedness of his devotion to the study of Nature which characterized the beginning of his scientific career endured unto the end. His admission at the outset of "utter ignorance whether I note the right facts"; his confession that he was "nothing more than a lions' provider"; his unfeigned astonishment at discovering that his early observations were of any worth—are all of a piece with the humility he subsequently displayed when his success was declared. As he found, one after another, many of his contemporaries and still more of the younger generation of naturalists adopting his views, his joy was great; but that joy was not alloyed by any feeling of pride. He did not care for making a convert to "Darwinism"—his exultation was that the strength of truth, of reason, and of observation had prevailed. In the same lowly spirit he, when at the height of his fame, expressed his gratitude to those, whosoever they might be, that helped him in his labours; and, if I might be critical on this point, I should say that his inherent goodness of heart often caused him to exaggerate the importance of the help they gave. Not a spark of jealousy was kindled in his mind; and at what may be considered the most trying moment of all, when the theory he had for twenty years been testing by every means in his power, the theory on which he built all his hopes of future recognition, the theory which he not unnaturally believed to be his peculiar possession—when this theory, I say, was independently conceived by another naturalist, his conduct was emphatically that of a man of honour. It pained him acutely to think that this naturalist, a trusted correspondent, an esteemed philosophical observer, and at the very time a wanderer far from home, should be deprived of the full glory of his ingenuity; and, but for the counsel of judicious friends (whose good advice on this occasion is indisputable), Mr. Darwin would have withdrawn every claim of his own to this great discovery, and left it entirely to Mr. Wallace! In the history of science and invention I think there are few cases like this. When you come to read the book you will find that though he unreservedly placed the matter in the hands of Sir Charles Lyell and of Sir Joseph Hooker, it was some time before he could reconcile himself to the notion that they were not unduly favouring him at the expense of his competitor. Such was the man! Though you are doubtless all aware of the fact, it would be wrong in me if I omitted to remind you that Mr. Wallace's conduct under these circumstances—sufficiently disappointing, as all must admit, to him—was in every way worthy of Mr. Darwin's. If in future you should meet with any cynic who may point the finger of scorn at the petty quarrels in which naturalists unfortunately at times engage, particularly in regard to the priority of their discoveries, you can always refer him to this greatest of all cases, where scientific rivalry not only did not interfere with, but even strengthened, the good-feeling which existed between two of the most original investigators.

I said but a few minutes since that it was fitting that the Memoir of Mr. Darwin should appear this year—this year of jubilee—and a very remarkable anniversary I now have to point out to you. I learn from the Memoir that Mr. Darwin's pocket-book for 1837—just fifty years ago—has this entry:—

"In July opened first note-book on transmutation of species. Had been greatly struck from about the month of previous March on character of South American fossils, and species on Galapagos Archipelago. These facts (especially latter), origin of all my views."

Other passages in his already published works confirm this memorandum; but we had not hitherto known with certainty when the views originated. We may now, therefore, celebrate among other jubilees that of Mr. Darwin's adopting the theory of the Origin of Species by Natural Selection, though I am bound to tell you that it was not until a few months later—about the beginning of 1838—that, after reading Malthus's work, the full conviction of the truth and sure ground of his speculative views came upon him.

I would not have my audience disperse with the impression that my business here is merely to point out what has been done by the genius of the great man of whose character and labours I have just been speaking. Enormous as are the strides which he has enabled us to make, you will all admit that it behoves us to follow in the paths he has indicated. We may depend upon it that what we know bears a very small proportion to that which we do not know, and I venture to recall your attention to that subject, which, as I have just said, was the origin of all his views. That subject is the Geographical Distribution of Animals and Plants, not only at the present time, but in bygone ages. As regards botany, I do not dare in the presence of so many distinguished authorities to say more than this—that I believe the greatest and most important results of their labours in this direction are inadequately known to zoologists, while in zoology I am certain that there are many large groups of whose distribution we are almost entirely ignorant.[1] That excellent work has been done in some groups all will admit, and in regard to the difficulties which have precluded the investigation of the subject in other groups I am well aware. But not only do we

[1] I say this after having studied Prof. Heilprin's recent work, "The Geographical and Geological Distribution of Animals" (International Scientific Series, 1887)—in many respects the fullest on the subject—and also Mr. Hemsley's admirable Introduction to the Botany of the "Biologia Centrali-Americana," which will shortly appear. The opportunity of reading the latter I owe to the kindness of Mr. Salvin.

need further investigation in regard to them, we want much more correlation of results than we yet possess, and still more a comparison of the results obtained by botanical and zoological inquirers. Here there is a wide field, and a field worthy of cultivation. I do not know that a more competent body of cultivators can be found than within this Section of the British Association, and if they can be persuaded to make common cause, the study of biology will be much advanced. We have been told that it is as useless to investigate the origin of life as the origin of matter. That may be true or it may not; but it seems to me that to learn the way in which life has spread over the globe ought to be within the capacity of man, and we can hardly learn that way except by far more intercommunication of special knowledge than has hitherto been made. It is evident that with the existing minute subdivision of biological research the subject is beyond the power of any one man; but I should rejoice if anything I could say on this occasion might put in train some alliance between botanists and zoologists for the object I have just suggested. It may be said that we have not sufficient information as to certain parts of the world to enable such an alliance to effect its work satisfactorily. If that be the case I am sure you will join with me in thinking that these insufficiently known parts of the world should be subjected to a thorough biological exploration. For many years past I have been accustomed to hear an adage that "Property has its duties as well as its rights." If I am strongly in favour of the rights of property, I am no less prepared to exact from it its duties. Various events have given to this nation rights of property in many parts of the globe. I think we ought to justify those rights, and there is no better way of doing this than by performing the corresponding duties. It is incontestable that among the dependencies of the British Crown there are innumerable places—islands, large and small, territories the limits of which no geographer or diplomatist can define, and so forth—of which the fauna and flora have never been scientifically investigated. It is right, of course, that I should recognize the successful efforts made in many instances by the directorate of the Royal Gardens at Kew, and to a less extent by private persons. But why should not a properly organized biological investigation of all the portions of the Empire be made? You will, I think, all agree that it is our duty to carry out investigations of this kind. Whether they would be better performed under the superintendence of Her Majesty's Government or not is a point on which I reserve my opinion, only mentioning that the success which has attended those instituted by the botanical authorities at Kew leads me to suppose that an extension of the method there followed might produce results as satisfactory; but, if this be the course adopted, I must point out that the organization of a corresponding zoological and geological directorate will be needed. This matter I merely throw out for your consideration; but I would add that if anything is to be done no time is to be lost.

When on a former occasion (at Glasgow in 1876) I had the honour of addressing a Department of this Section, I pointed out the enormous changes that were swiftly and inevitably coming upon the fauna of many of our colonies. The fears I then expressed have been fully realized. I am told by Sir Walter Buller that in New Zealand one may now live for weeks and months without seeing a single example of its indigenous birds, all of which, in the more settled districts, have been supplanted by the aliens that have been imported; while further inland these last are daily extending their range at the cost of the endemic forms. A letter I have lately received from Sir James Hector wholly confirms this statement, and I would ask you to bear in mind that these indigenous species are, with scarcely an exception, peculiar to that country, and, from every scientific point of view, of the most instructive character. They supply a link with the past that once lost can never be recovered. It is therefore incumbent upon us to know all we can about them before they vanish. I have particularly instanced birds because I happen to have studied them most; but pray do not imagine that the same process of extirpation is not extending to all other classes of animals, or that I take less interest in their fate. The forms that we are allowing to be killed off, being almost without exception ancient forms, are just those that will teach us more of the way in which life has spread over the globe than any other recent forms, and for the sake of posterity, as well as to escape its reproach, we ought to learn all we can about them before they go hence and are no more seen.

I have just now applied to these expiring forms of New Zealand the epithet ancient, and in connexion therewith I would, by way of conclusion, offer a few remarks on the aspect which the subject of Geographical Distribution presents to me. Some of us zoologists—I am conscious of having myself been guilty of what I am about to condemn—have been apt to speak of Zoological Regions as if they were, and always had been, fixed areas. I am persuaded that if we do this we fall into an error as grievous as that of our predecessors, who venerated the fixity of species. One of the best tests of a biologist is his being able to talk or write of 'Species' without believing that the term is more than a convenient counter for the exchange of ideas. In the same way I hold that a good biologist should talk or write of "Zoological Regions." The expression no doubt arose out of the belief, now scouted by all, in Centres of Creation; and, as sometimes used, the vice of its birth still clings to it. To my mind the true meaning of the phrase "Zoological Region" is that of an area inhabited by a fauna which is, so to speak, a "function" of the period of its development and prevalence over a great part of the habitable globe, but at any rate of the period of its reaching the portion of the earth's surface where we now find it. One great thing to guard against is the presumption that the fauna originated within its present area and has been always contained therein. Thus I take it that the fauna which characterizes the New-Zealand Region—for I follow Prof. Huxley in holding that a region it is fully entitled to be called—is the comparatively little changed relic and representative of an early fauna of much wider range; that the characteristic fauna of the Australian Region exhibits in the same way that of a later period; and that of the Neotropical Region of one later still. But while the first two regions have each been so long isolated that a large proportion of their fauna remains essentially unaltered, the last has never been so completely severed, and has received, doubtless from the north, an infusion of more recent and therefore stronger forms; while, perhaps impelled by the rivalry of these stronger forms, the weaker have blossomed, as it were, into the richness and variety which so eminently characterize the animal products of Central and South America. I make no attempt to connect these changes with geological events, but they will doubtless one day be explained geologically. It is not difficult to conceive that North America was once inhabited by the ancestors of a large proportion of the present Neotropical fauna, and that the latter was wholly, or almost wholly, thrust forth—perhaps by glacial action, perhaps by the incursion of stronger forms from Asia. The small admixture of Neotropical forms that now occur in North America may have been survivors of this period of stress, or they may be the descendants of the more ancient forms resuming their lost inheritance. Beyond the fact that these few Neotropical forms continue to exist in North America, its fauna seems to be in a broad sense inseparable from that of the Palæarctic area, and, in my belief, is not to be separated from it. The most difficult problems are those connected with the Ethiopian and Indian (which Mr. Wallace calls the Oriental) areas; but I suppose we must regard them as offshoots from a somewhat earlier condition of the great northern or "Holarctic" fauna, and as such to represent a state of things that once existed in Europe and the greater part of Asia. To pursue this subject—one of most pleasing speculation—would now be impossible. I pray you to pardon my prolixity, and I have done.

SECTION E.

GEOGRAPHY.

Opening Address by Colonel Sir Charles Warren, R.E., G.C.M.G., F.R.S., F.R.G.S., President of the Section.

"The geographer should therefore chiefly devote himself to what is practically important."—Strabo, c. i. § 19.

My predecessors in former years have used their discretion in the opening address either to generalize on the science of geography or to lay stress upon those particular subjects to which they considered it desirable to call attention. I propose on this occasion to refer to matters which have long been of importance to those who are desirous of the spread of the knowledge of geography, and in which I trust the public generally are acquiring an interest. I refer to the teaching of geography in our schools and the economy and advantage to the State which would result from a more perfect and skilful system of instruction.

The term geography covers a very wide area, and while limiting its use to-day to the more restricted sense generally accorded to it in modern times, I must protest against its being applied only to a dry digest of names of places and record of statistics, rendering it a bugbear in the instruction of youth instead of allowing it to cover all those interesting and engrossing subjects which truly belong to it, and without the knowledge of which the mind of youth cannot be trained and expanded in the direction to which the science tends.

As the geographer Strabo points out, our science embraces astronomy, natural history, and is closely connected with meteorology and geometry, the arts, history, and fable; but since his day so much progress has been made in the arts and sciences that the branches of geography have become specialities to be taught separately, and the old root geography has been almost laid aside and treated with contempt, though it is only by a thorough acquaintance with it, the knowledge of common things, that the branches which depend upon it can be thoroughly comprehended. We may take geography, then, to embrace all that knowledge of common things connected with the surface of the earth, including the seas and the atmosphere, which it is necessary for every human being to be acquainted with in order that progress in other knowledge may be acquired and acquaintance with the world be made which will fit man for life in any capacity, whether as occupying the highest position even to the most humble. Indeed, it is difficult to say in what capacity in life this knowledge is most required. No man can do practical work without it, and to the theorist it is absolutely essential.

The science may be divided under two heads; that which we learn from others, that which we acquire from our own observation and researches. All experience tells us that the information is most valuble which we acquire by our own exertion, and therefore every effort should be made by those interested in the welfare of mankind to endeavour that each one should learn everything that can be learned from his own observation properly directed.

Year by year, as the surface of the earth becomes better known, the districts in which explorations of an adventurous nature can be made diminish more and more, and as scientific research takes the place of that of a ruder nature the chances of excitement grow perceptibly less. Indeed, when we look upon the knowledge possessed by the ancients and study their cosmogony we cannot but feel the loss we have sustained in approaching the truth. The poetic halo with which everything was encircled, the deep shadows and gloom, have gradually been dispersed and dispelled, together with all the distant and uncertain light which gave so much scope to the imagination, and we now view the hard stern realities of fact, brilliant and gay in their colouring, but leaving no room for fancy, or for a change of ideas—always the same vivid rigidity of outline which admits of no two opinions. It is like the change of scenery from our own beautiful cloudy island, where the tints and shades change from hour to hour, and where the grey and purple distances leave so much to the imagination, to the bright scenes of the Mediterranean shores, where everything is bathed in intense sunlight, and distinctness of outline reigns supreme, where there is no possibility as to doubt.

In each case we may balance the advantages and disadvantages; but as we have gained in knowledge so we are losing in understanding. We are fast losing our human nature and are becoming machines, and we call it being civilized. We are drifting into a condition in which we learn nothing of ourselves or by our own individual efforts; we are coming to a time when, as we know more about science, and are better educated in arts, we know less about mankind, and are the less able to assist in gaining knowledge of the world; all power of doing so is day by day becoming vested in the hands of a few scientific men, on whose word we have to rely. In this progress we are losing all we used to hold most dear; the desire of living for others is departing, and with it hospitality, chivalry, enthusiasm, unselfishness, and because we are unable to exercise the talents given to us they rust and corrode. No doubt we are able to seek other channels for our energies of mind, but how are we to exert our physical powers for the benefit of man? In days of yore it was open to any man of spirit and strength and activity to set out in quest of adventures of the unknown for the assistance of his fellow-men, to relieve the world of its monsters, to risk everything for others. But those days of daring are now gone by; the doubt, uncertainty, and mystery attached to unknown danger are no longer to be met with, and though the same chances are always presented to human nature to practise self-denial, they are now, though more difficult perhaps, of a passive instead of an active nature, and do not so distinctly belong to the domain of geography as they did in olden times.

As the people of olden times are to those of the present day, so may we consider the child to the man; and we adults in this assembly must recollect that, however strong may be our emotions and passions at the present time, they are but of a mild and vapid nature when compared with the aspirations and feelings of youth. Each prosaic-looking child is full of poetic and romantic feeling, to which as a rule utterance is never given, but which, nevertheless, cannot be rudely shattered without injury to the mind, and which, if taken advantage of, may assist greatly in training the mind and developing a love of geography.

It should be a matter of great interest to those who instruct in geography to study its gradual development from the earliest date and to watch the progress it has made. And this is not a matter of very great difficulty, for as geography is the knowledge of common things, and the ancients were more experienced observers than ever we may hope to be, the earliest records we possess are full of geographical accounts. In the books of Moses, three thousand years ago, we obtain our first recorded view of the cosmogony of the ancients, at which time the world is supposed to be a flat disk with water surrounding the land, and this idea pervades later books, and is dwelt upon in the Psalms of David. Homer also held a similar view, and to him is accorded by Strabo the honour of being the founder of geographical science, because he excelled in the sublimity of his poetry and his experience of social life; and a reason why he excelled is carefully related. He could not have accomplished it had he not exerted himself to become not only acquainted with historical facts, but also with the various regions of the inhabited land and sea, some intimately, others in a more general manner. "For otherwise he would not have reached the utmost limits of the earth, traversing it in his imagination." Herodotus, to whom we are indebted for furnishing us with the *earliest known* system of geography, also held the same view concerning the earth; but it is worthy of remark that he speaks in his day (450 B.C.) of there being another view, as to the world being round, which he considers to be exceedingly ridiculous, and therefore it may be surmised that even at that early period there were minds that had arrived generally at the conclusion which now obtains as to the shape of the world. The idea that the sun, moon, stars, and planets revolved round the earth was the view in early days, and continued up to quite a recent period, and even now we are unable to prove that the generally received system is correct, and only use it as being more convenient than that which makes the earth the centre of the universe.

When we come, however, to consider the progress of discoveries on the surface of the earth itself, the strides in later years appear to be enormous, but yet we must not forget that there is an ebb and flow constantly going on. Discoveries are made and lost sight of, and again are brought forward as new. Sometimes after an account of discoveries has been published a second account differs most materially from the first, and the public have to wait for further examination. Cases have occurred, as in the early Portuguese discoveries in Central Africa, in which the plans and accounts have been laid on one side and forgotten, and the territories rediscovered and surveyed years afterwards. Again, sketches of new countries have been made, and the surveyor has omitted to show what is conjecture and what is from actual observation, and his plans throughout have been discredited. In some cases these mistakes have retarded discovery, in some they have directly led up to it—as, for example, in the gigantic geographical error in placing on the globes of the fifteenth century the eastern extremity of Asia no less than 150° of longitude too far east, which prompted Columbus to endeavour to reach Asia from the west, and thus led to his discovery of America.

In gauging the progress of our knowledge of geography we must not, however, simply take into account what has been made by ourselves, but by the known world generally; for example, although the Portuguese circumnavigated the Cape and proved that it was practicable to do so, it is still a moot question whether they were attempting what was known or unknown. At any rate it seems certain that in the thirteenth century—not to go back earlier—the Arabians were aware of the fact that

Africa on the south was surrounded by the ocean, and the geography of Abulfeda clearly points this out.

It is, then, a difficult matter to decide what is a discovery in geography. We may possess an exact description of a town and know its position, and yet it may never have been visited by a traveller from what we term civilized Europe.

What we require, however, is precise and accurate information of the earth's surface, however it may be obtained, and to train the minds of our youth in the powers of observation sufficient to enable them to obtain this information; and if in so doing our countrymen continue to be stimulated to deeds of daring, to enterprise and adventures, to self denial and hardships, it will assisting in preserving the manhood of our country, which is more and more endangered year by year in consequence of our endeavour to keep peace within our borders and to stave off strife with our neighbours.

Probably many of us here to-day of mature age, on looking back at our early acquaintance with geography, will recollect little but a confused list of proper names and statistics, learnt by rote, and only imperfectly carried in the mind, so that only a few portions stand out still visible, and those probably connected with pleasurable and, in some cases, painful accessories; perhaps those particular lessons which we may have assisted some school friend to master still remain as clear as ever; or, again, those learnt under the terror of the rod.

Taking schools and subjects all round, nothing probably has ever been worse taught than geography was only a few years ago, and very little progress towards a good system has even yet been introduced into higher-class schools, though in the schools of the people an effort has been made to render the subject more palatable and instructive.

The faults, however, of the system hitherto in use are now fully recognized, and objections are general that the study has been made too painful a grind, and that the whole process has been of too severe a character. If this were the only fault to be found in the old method, I for one would be inclined to adhere to it, assured, as I am, that no training of the mind can take place without great denial and sacrifice in learning self-control. But the real question is as to the practical results of the old system. Are they of such a character with all or the majority of minds (of all classes and conditions) that they have become stored with useful knowledge and at the same time trained to take a pleasure in increasing it in the future? It the results are short of this we cannot but pronounce the old system to be a failure, as the knowledge of geography is the knowledge of common things inseparably connected with the life of each one of us, and there is no better medium through which the mind can be trained to be always in a condition for acquiring knowledge without making too great an effort.

Unfortunately for the prospects of introducing a complete and perfect system of teaching geography (suitable to most minds), the reaction that has set in recently is likely to lead to evil results if not carefully curbed. It seems now to be desired to promote the acquirement of knowledge at the earliest age without effort and without hard work; but this appears to be directed towards alleviating the toils of the instructor as much as the instructed, and we have now, as a result, children taught common things without any effort to strengthen their memories, and then a system of cramming introduced at a later period, when the memory has ceased to be capable of responding to the efforts made, and consequently all the information crammed in is dropped again in a few months.

The memory of youth is like a cup swinging freely on a pin thrust horizontally through its sides. If the pin is below a certain line, the cup will tilt over and lose its contents when filled up beyond a given level; but if the pin is near the upper edge the cup can be filled with more and more security. By careful training in the earliest years the cup may be constantly kept full in later years; but by the training at present in use the cup tilts over far too soon.

It seems to me that the remedy recently adopted is worse than the disease it was to eradicate, and that however injurious it was to attempt to store the mind with mere names, yet the memory was trained thereby to retain something definite; and it is still worse to attempt to store the mind with mere ideas without the connexion of names, and leave the memory to rust.

There is obviously a middle course which may rid us of the errors of the past without leading us into still greater difficulties. And if we keep the object to be gained always in view, we cannot fail to take a direct line. We want first to lead the memory to constant exertion of such a nature that it grows stronger day by day, but is not overstrained or wearied; at the same time it must be stored with useful facts, which may be quite above the capacity of the mind to comprehend at the time, but which will be required all through life: this can readily be done by means of verses or rhymes set to simple airs and committed to memory by song. There are facts of the greatest importance which can be learnt in this manner with very little effort, and which, if not fixed in the mind at a very early age, the want of them may be felt throughout life.

As, for example, the directions in which latitude and longitude are reckoned, in which the sun rises and sets, the relations of the east and west respectively to the north and south, and many other matters which appear to be of a trivial character, but which require to be as rigidly committed to memory by rote as does the multiplication table.

These very small matters are the foundations of everything we require to know, and if we do not have these foundations firmly and securely fixed, we shall be the sufferers all our lives. Too much attention cannot be paid to them, as it is the early lessons which remain most clearly fixed in our minds.

A point connected with this subject, which admits of much discussion, is as to *how* such verses should be learnt, whether with the assistance of books, pictures, or metaphor. Should they come to the memory through the eye, or the ear, or through both? As a beginning, I think that geography should not be learnt from books, but from the teacher, who may use diagrams and pictures, but at the same time text-books should not be done away with, as is so constantly advocated; on the contrary, they should be adhered to most rigidly. There are few teachers who could improve on a good text-book, but these books should be for the teachers, and not for the children. But the teacher should not use the text-book when teaching.

Children have a remarkable capacity for making pictures for their mind's eye of every thing they think of, which is dulled gradually as books are taken into use; this faculty, if made right use of, may be developed, and will greatly assist the study of geography, and will lead to a "picture memory," which will be most useful in regard to maps, drawing, and spelling. This faculty can, of course, be over cultivated, but there is not the remotest danger of this occurring at present in any of our schools. When highly developed, we find it employed by novelists, who can bring their characters up before them and picture them enacting their parts, and also by artists, who sometimes lose the power of discriminating between that which they actually see and that which their picture memories call up.

Although it seems to me absolutely essential to cultivate and develop the memory, so often called the "parrot memory," of young children, this is by no means all that is necessary. At the same time must be taught the proper use of the powers of observation with reference to Nature, which in towns is so difficult a matter, placing the bulk of our population at so great a disadvantage. One of the first points neglected by teachers generally is to explain to children what the object or result of the lesson is to be. In most minds it is very difficult to pay real attention unless it is known what is to be the general drift of the conversation, for otherwise the mind will be directed to points quite irrelevant. Children should be first told in a few words the line the lesson is going to take; this will greatly tend to secure the attention of what are termed dull children, who often, if properly treated, would turn out the cleverest, but who cannot grasp a subject until they see it from all sides, and know it thoroughly, while the "clever children" are satisfied with a view of one side only. The foundation should be laid slowly, the progress being governed by that of the "dull children," who often will most amply repay the teaching. The clever child will not be hurt by having the subject impressed upon his mind over and over again, so long as it is made interesting.

Great care must be taken in the method of presenting maps at an early age before children, and a distinct idea should be given of the difference between a map and a picture.

It must be recollected that from the moment geography is taught, children will make maps or pictures in their mind's eye, whether they are actually presented to them or not.

For example, if a house or a garden is mentioned, both the teacher and the child must view it from the outside and from a certain distance, for it is impracticable for most minds to look all round and behind at one time. To have a full view of what is mentioned, it is necessary to get outside and beyond it. Children will differ among themselves in their method of viewing what is spoken of, but the teacher can readily ascertain what mental pictures they have formed, and can make use of this faculty in the first use of maps. Children should first be instructed in maps of the village or town in which they live. It is remarkable how readily uneducated natives in uncivilized countries can understand plans from their constant observation of Nature. Most intelligent Bedouins are able to make a rough plan or diagram in the sand with a stick of the district they know, and will also take care that the orientation is correct. Kaffirs can do the same, and can point out the direction of a cattle post fifty or sixty miles distant with unerring sagacity.

It is of vital importance that children in our island, who cannot under ordinary circumstances have sufficient opportunities for using, cultivating, and developing their powers of observation to any purpose, should have the use of maps put before them in such a manner that they will not be led into error. Otherwise they will have fixed in their minds factors of discord which the teacher may know nothing of, and which will trouble them through life, and which if they do get rid of with great labour in after years, will constantly return at unseasonable moments.

It is very common for children to mistake east for west, north for south, and even to make still more ridiculous errors which appear on reflection to be quite impossible. Yet these errors remain often unobserved until the youth is eighteen or nineteen years old, when he begins to think the matter out for himself, from finding that he is continually making absurd mistakes, but then it is too late for him to do more than know that he is liable to the error, for on an emergency it will crop up in spite of himself.

I am aware of one instance in which an educated surveyor when thinking of London invariably placed the portions about Regent Street and Charing Cross in an inverted position while picturing all the rest correctly, and it was only by an effort that he could turn this portion upside down into its place. Another, when thinking suddenly of Paris, always placed it to the north of London; and another always thought of the west end of London as being towards the eastern coast.

Out of thirty cases of well-instructed men at an age between eighteen and twenty, I have found that about eighteen were under the impression that while the sun rises in the east, the stars rise in the west, from having learned that the sun has a proper motion among the stars.

I fancy there are few educated men who have not grown up with some curious errors with reference to geographical facts which have bothered them all their lives, and which they have found it impossible to get rid of even when they have discovered where the errors lay; and I believe that many of the numerous blunders and accidents which constantly occur on railways, with shipping, machinery, &c., and the causes of which cannot be accounted for, are really to be ascribed to some early error in learning geography or the knowledge of common things, errors which, when attention and watch over self is suddenly withdrawn, influence the actions in a contrary direction to that which is right.

As an instance of the natural liability to error, even apart from those which may be ingrained while under instruction, I may allude to the feeling when the eyes are shut when travelling by rail or carriage that the vehicle is going in an opposite direction to that in which it actually moves, to the impression when approaching or leaving land in a boat or balloon that the earth is moving and that oneself is stationary; even when on horseback under excessive fatigue in the dark the traveller has been known to imagine that the horse was moving rapidly backwards. The effect of excessive fatigue from physical exertion has somewhat the same result as a want of self-control from bad training of the mind, and perhaps those who have ridden for many miles on horseback or in a coach may have noticed how in the dark a fixed lamp may be seen to make various fantastic signals due to the motion of the horse or coach transferred by the eye to the lamp. As another instance of the difficulty of self-control I may mention a case in which a man well instructed in taking astronomical observations and in the rudiments of astronomy could not divest himself of the idea, which he had gained as a child, that the moon shines with light of her own, and that her phases are due to the earth getting between her and the sun, this error continually interfering with his mental astronomical pictures, though when his attention was specially called to the subject he was aware of the error which intruded itself so constantly in his views of the heavenly bodies. The difficulties regarding east and west, north and south, probably arise from a multiplicity of causes, such as the southern side of the Mediterranean being the northern coast of Africa, or the southern view of a house being obtained by looking towards it in a northerly direction, and these difficulties as to orientation do not only occur in modern times, but are to be found in ancient writings. Another constant source of error is inverting names unconsciously, such as speaking of Jupiter's rings and Saturn's belts. As an instance of this I mention a case in which, a lecture being given on the Franco-Prussian war, the lecturer inadvertently in the middle of his lecture used the word "Prussian" for "French," and *vice versâ* continually throughout, and though he was quite aware of some anomaly every now and then, he could not ascertain where he was in error until near the end of his lecture. Another source of error which cannot be too carefully guarded against results from placing the celestial globe by the side of the terrestrial globe and treating them as though they are of the same character; this is certain to confuse east and west with most children, as one has to be looked at from the outside and the other from the inside in actual fact. Again, as some star charts are made that they may be looked at from above and others from below, causing the east and west points to differ, there is sure to arise confusion. I venture to say that there are few young minds which are not absolutely and hopelessly confused by the use of celestial globes and charts. I believe it to be essential that, until the mind is fully trained and developed, the stars should be looked at from within and not from without, and it appears to me that all the information which a child can require, apart from practical observation, concerning the phenomena of day and night, the seasons and months, the circles and zones, the phases of the moon and eclipses, can be imparted by the use of a lamp with a reflector and two globes, though a good orrery placed in the school for children to examine and observe for themselves would often enable the dull ones to keep up with the rest more easily.

It will be interesting to note whether the class of error alluded to does not arise principally among those bred in towns, and who have not had an opportunity of developing their observation in the country; as with those who do use their observation a habit is required of unconsciously working out questions which arise, and the mind arrives at a correct conclusion. This end should be the great aim and object in instructing in geography, for as there is no royal road to knowledge divested of grind and pain, there is yet the path which provides the greatest amount of result with the least amount of grind, in which all the labour expended is productive, and in which after a time labour even becomes a pleasure.

It seems very desirable that the first maps presented to a child, viz. those of the school grounds and the parish, should be placed on the floor and properly orientated; this will go far to fix the correct positions of east and west, north and south, and will prevent the idea of the north necessarily being *up* and the south *down*. It is to be observed that if the child looks up to a map it is almost equivalent to looking at the map when lying on the back, in which case the east and west are inverted. The motion of the sun over the map might with advantage be pointed out at various times of the day, and if the position of the rays of the sun on the floor when on the meridian could be shown each day when practicable on the line drawn north and south, it would do much to fix in the mind the fact that the sun is in the meridian at apparent noon each day. A sun-dial should also be available in every school-yard to which children may have access.

The map of the district round the school should only be made use of in order to clear the way to understand what a map is, for reference in describing other maps, and for practical purposes in giving the child useful information as to the places in the neighbourhood. While this is going on, the child should be taught to point out the actual directions in space of the principal towns, &c., in the county and island, and then an outline map of the British Isles with the principal places and features marked on it should be brought under review. Too much detail should not be crammed into the early lessons; a good firm foundation is required, something to start upon before the great test of faith is made in teaching, viz. that the world is round.

Children should be taught, as far as is practicable, to make this discovery for themselves, and many will arrive at it one way or another, or think they do so, which is equally important. It is far better they should grasp truths themselves than have them drummed into them; it gives them confidence in their own deductions, and leads to further observation of Nature. In introducing the world as round, a *blackboard* globe should be used, about 3 feet in diameter, on which the continents are outlined boldly in red, with some meridians and parallels of latitude in white. It would be well if a portion of this globe could be taken to pieces to show how a horizontal sun-dial for the particular latitude is constructed, and for other matters of interest. It is material to show that the earth revolves on a fixed axis from day to day, and in one direction. All the great difficulties in learning geography are at the threshold of the science for those who have not observed Nature; the more abstruse subjects are comparatively easy to teach.

The first difficulty common to all is that with reference to latitude and longitude, regarding which there are so many elements of error. It is so difficult for the child to recollect which term means length and which breadth, and then to get the restive imagination to grasp the fact that the length is sideways and not up and down, as it apparently should be; for even if the earth is shown to be an oblate spheroid, there is nothing to lead a child to see that there is a greater circumference round the equator than round the poles, and the time has not arrived to perplex the child with the views of the ancients on the subject. Then, again, if the child does recollect that the meridians of longitude run from north to south, and the parallels of latitude from east to west, it is probable that he may measure the longitude in degrees along the meridian and the latitude along the parallels; a very common and recurring error, difficult to deal with. The only practicable method is to put the facts of the case into amusing verse and commit it to the memory by song. At this stage, also, some easy standards of measurement put into verse and to music should be learnt by rote, to enable the child readily to recollect the relative measurements of the earth, sun, and moon, and the radii of their orbits and times of progression.

I lay great stress upon these matters at the beginning, because they are really all in all to those who wish to succeed in the science in after-life, and I have viewed the matter from the stand-point of what will be required at the age of eighteen to twenty, when the mind ought to be capable of taking up any subject, instead of considering what show of learning the child should be able to produce in an examination at an early age. The stock-in-trade of knowledge for each young person need be very slender, but it must be of the right sort and best quality. No doubt there are many children badly trained who can gradually work out matters correctly for themselves, but these are the few with originality of mind, and even they would be benefited by not having to spend a portion of their lives in unlearning.

Once the preliminary difficulties are over and the power of observation and reflection is acquired, even in a small degree, the study of geography becomes but a simple matter, for it is the learning of common things, matters of every-day life, which we may, if in the country, acquire to a partial extent of our own experience; but though so simple it requires continuous application and attention.

In each calling or trade a man may become an experienced geographer to a limited degree. The pilot, for example, is an expert in the geography of the seas he works on, for he not only knows the ports, the coast lines, and the sunken rocks and sandbanks, but he also knows the tides, the winds, he studies the clouds and the currents, and has an intimate knowledge of the contours of the shallows; moreover, he knows the shipping of various countries, the merchandise they carry, and the produce shipped from each port. In the same manner, by hunting, shooting, fishing, bicycling, birdsnesting, &c., we acquire a knowledge of natural history and topography which aid us most materially in the study of geography, and which in a limited degree *is* the study of geography.

Even in large towns it is practicable to learn lessons in geography from actual experience and observation, for if the markets and railway produce are examined, it can soon be ascertained from whence the articles come and from what ports, and with careful attention most valuable lessons in political economy can be gained.

The bulk, however, of our children are cooped up in towns and walled playgrounds, and even when in the country are too often confined to one field; they have few opportunities of insensibly studying the wonders of Nature, and therefore, in order to develop their powers of observation and to understand geography, artificial means must be made use of. Great efforts are now being made under the new Code to produce these artificial means, by raised models and water and other devices, and it is to be trusted that, if these schemes can be carried out, the habit of observation will be induced; but the memory also must be at the same time actively exercised and stored with fresh facts day by day.

The knowledge of geography thus, even in its restricted sense, embraces the life of an Englishman of every class and occupation, and its study is of the greatest importance to every man who has an occupation; it is singular that so little comparatively is thought of cultivating the science, and how small interest the State has hitherto taken in fostering this class of education.

But while the Board and other schools for the people are gradually taking up the work, and endeavouring to work out a good system of education, it is mortifying to find how little progress has been made in the higher-class schools where such heavy fees are charged; and the question arises whether in these schools the teachers of geography really understand the subject they teach, and would pass an examination before a Government inspector.

The boys of the wealthy classes are put to the greatest disadvantage with regard to the study of geography. The son of a labourer will hear the price of provisions and clothing constantly discussed, so also with the son of a mechanic and tradesman, and will learn much about geography on the subjects with which the parents are connected, and will also in some measure learn to exercise his observation; but the son of wealthy parents is too often carefully kept from hearing all that might teach him geography, and he is seldom obliged to exert himself to use his observations in any essential matters of daily life; this is reserved for the playground, where nothing of real importance is at stake, and must have the most deleterious and detrimental effect on many young minds, and naturally results in so large a proportion becoming useless for any occupation.

It is apparent that, as education throughout the country progresses, the sons of the wealthy classes, if they are to compete successfully with others, must have some better mental training than they obtain at present, otherwise they will in a few years be distanced by the sons of the labourers, artisans, and shopkeepers. What an Englishman asks for is a fair field and no favour, and it seems hard upon a parent who struggles through life to make money to be enabled to give his children the best and most expensive education the country affords, that with it he must risk a training of the mind which is inferior to that in the less expensive schools of the people. As we are behind the Continental States and our colonies in so many of our institutions and land laws, so we are behind them in our training of the mind in our upper-class schools; by neglecting by artificial means to develop the power of observation among boys, who until they are put out in the world are never accustomed to do anything that will tend directly to any practical and useful result, we are putting them to the greatest disadvantage, and handicapping them in the race of life.

We omit to train the memory in early years, to lay a foundation of facts in the mind, and to develop any power of observation; we carefully prevent their doing anything useful, and bring them up in a moral atmosphere in which the idea of anything but amusement is practically excluded, and then in later years we attempt to adjust all our errors by cramming, when the memory is incapable of being crammed, and the mind has ceased to desire to acquire information; the result is that many young men are deliberately rendered unfit for work in life, and those who have sufficient courage and energy to look their prospects in the face find the enormous disadvantages to which their teaching has subjected them, and lose precious years in unlearning and learning again.

More unfortunately still, the best and choicest of our minds cannot be crammed; and thus drop out at our examinations many minds of the class that for practical purposes would be most useful to the State. I allude more particularly to the minds endowed with reflective faculties, which tend to originality and research; these minds cannot be successfully trained unless combined with the teaching there is something useful to do. It is often observable that an indolent, inert, and lazy boy suddenly becomes filled with enthusiasm and emulation, both at studies and in the playground, when subjected to a change of training. I venture to assert that every year at our public examinations many men are rejected who are of the most superior class of mind for all practical purposes, who are physically most capable, who are so constituted that they cannot cram, and who, though retarded by want of proper training, are beginning to train their minds for themselves, and who if brought up under a good system in early years would take the highest places in examination. We are thus losing year by year from our front rank the men who would be of the greatest service to the State.

The pleas given for the study of geography by Strabo are worth bringing before the mind of youth, for he points out that while the success resulting from knowledge in the execution of great undertakings is great, the consequences of ignorance are disastrous, and he refers, among other instances, to the shameful retreat of the fleet of Agamemnon when ravaging Mysia, and to bring it more home to our every-day life he says:—"Even if we descend to such trivial matters as hunting, the case is still the same; for he will be most successful in the chase who is acquainted with the size and nature of the wood; and one familiar with the locality will be the most competent to superintend an encampment, an ambush, or a march."

He further calls attention to "the importance of geography in a political view. For the sea and the earth on which we dwell furnish theatres for action; limited, for limited action; vast, for grander deeds; but that which contains them all, and is the scene of the greatest undertakings, constitutes what we term the habitable earth; and they are the greatest generals who, subduing nations and kingdoms under one sceptre and one political administration, have acquired dominion over land and sea. It is clear, then, that geography is essential to all the transactions of the statesman, informing us as it does of the positions of the continents, seas, and oceans of the habitable earth."

Of all persons who require a knowledge of geography stand first those who are most concerned in the government of our Empire, and yet, as has been mentioned, they have for the most part been brought up at schools where the mental training for geography is most defective. Our statesmen as a rule have neither theoretical teaching nor practical experience in the science, and it is perhaps not too much to say that, putting on one side those who are merchants and sailors, there are no more ignorant persons with regard to geography than our law-givers. This ignorance endangers the safety of the country, for the people are continually perceiving, with regard to matters of every-day life and practical experience, that their law-givers are more ignorant than themselves, and are consequently continually interfering and giving advice in the details of the administration of the Empire.

The progress and development of a free country depend upon the characteristics of the inhabitants, but these again depend in great measure upon the natural resources of the country—the soil, climate, mineral wealth, navigation, mountain ranges, risks and dangers from natural causes, and we must not omit the position of the country both with reference to commerce and war.

It is not usually the country too greatly favoured by Nature which develops most rapidly, neither is it necessarily a long term of peace which favours progress; on the contrary, all experience shows that man requires a certain amount of opposition to bring out his energies and stimulate him to exertion, and though we are constantly talking in our country of the blessings of peace and horrors of war, we must generally acknowledge that our present foremost place among nations is due in a great degree to the keeping up of our innate energies by incessant turmoils and differences of opinion within and little wars and commercial rivalry without. It is not, then, to a reign of peace in which our energies would stagnate and become effete, but to a continuance of political excitement, which keeps the people on the alert, that we should be indebted for progress, and our statesmen should be sufficiently well educated and trained to take advantage of every time of excitement in furthering the welfare of the Empire.

We owe the benefit (before railways) in the improvement of our great northern roads for military purposes to the rebellion of 1745, leading to our being able to run coaches between London and Manchester in 1754, and between London and Edinburgh in 1763. Scotland and Ireland are both indebted to war and disorder for the first roads, constructed for purely military purposes.

But while the duty of taking advantage of each fitting opportunity for developing a country lies with the statesman, his prospect of success depends in great measure upon his geographical knowledge. His work may serve but for the purposes of the moment, and never benefit posterity, if he has no knowledge or foresight, no originality of purpose and perception of the fitness of things.

The measures that can be taken may be divided into two classes—domestic and international: the former designed to benefit the country or Empire directly; the latter to shield the land from hostilities from without, and in which the consideration of geographical position has a most all-important bearing. In this latter class a complete knowledge of geography is an absolute necessity, as the question arises so often as to whether the acquisition of geographical positions will weaken or strengthen a kingdom. For example, were Ireland 2° further to the west, it is probable that all our views as to the method of connecting it for administrative purposes with Great Britain would be greatly modified. Again, the particular points at which our coaling stations may be situated about the world may depend upon a variety of circumstances, changing from year to year. Thus Gibraltar, from its geographical position, was an absolute necessity to us thirty years ago, but, owing to various changes, it is not now of equal value, either as a coaling station, for protecting our commerce, or as a depot for our wares, and the question is arising with some geographers whether it might not with advantage be exchanged for Ceuta on the opposite coast.

It is possible that a more full geographical knowledge of Egypt and the Suez Canal might have materially modified our present occupation of Egypt. The Canal could not be held without a fresh-water supply, and the possession of Cairo and the Nile is the key to the fresh-water canal supplying Ismailia and Suez. Had it been known that a plentiful supply of water could be obtained close to the maritime canal, independent of the Nile water, it is questionable how far any occupation of Egypt would have been necessary.

In such cases it is not sufficient that the Government subordinates should have a knowledge of geography, for, even if they are fully conversant with what they ought to know, it would be almost impracticable for them to convey to statesmen knowledge which their untrained minds render them incapable of retaining or making use of.

In settling political boundaries it may appear at first sight that they should coincide with certain geographical features, forming natural boundaries, not only in international matters, but also in cases of provincial, county, town, and parish boundaries, and also in accordance with historical associations; but we must do our statesmen the justice to admit that the deviations they adopt may not always be the result of ignorance, but arise from an astute perception that it may be necessary in the future to have a cause for further modification, or even for raising the whole question anew. It is difficult, however, to see how this can, with any propriety, arise in domestic matters, and, apart from the doubtful political morality involved, it would only occur in international matters on the assumption that our Empire is paramount, and can quarrel when it chooses; and, moreover, in such a case could only be justified by being carried out with so perfect a knowledge of geography that in any reopening of the question our country should be in the right; whereas bitter experience has shown us that our statesmen have almost invariably placed us in the wrong.

It is fatal in domestic matters to ignore the physical features within a country, and attempt to obliterate its historical and topographical associations, as the French Revolutionists attempted, by substituting their departments for the old provinces. This has only led to an artificial division, which has not taken root among the people, and French geographers are still calling attention to the absurdity of present divisions. In such cases we must keep alive to what are the ostensible and what the actual reasons for such changes, and if the so-called simplicity introduced by lawyer statesmen leads to increased law expenses, we may reasonably look with suspicion on such an interference with the economical administration of the affairs of the nation. In our own country geography is intimately connected with all kinds of divisions of land, which are dealt with by the administration. A simplification of the arbitrary political divisions, and a modification and synchronization of boundaries may lead directly to simplification of administrative machinery, and saving of expenses in salaries, &c. London itself is a glaring instance of the waste of money and friction of departments, from the extraordinary overlapping of boundaries—political, magisterial, petty-sessional, police, statistical, postal, public works, &c. Probably a great portion of the time and energies of the superior officers in the various departments is occupied in waging war on one another, keeping the peace, or temporizing with or watching each other; and this not from their own desire to quarrel, but from the fault of the system which overlaps duties as well as boundaries, and often gives one and the same duties to be performed by distinct departments. Perhaps, in some instances, this friction may call out latent energy, but it at least most successfully prevents departmental superiors from looking into their own departmental affairs, and developing and perfecting the local administration, and keeping up to the times.

With regard to international boundaries, too little attention is usually paid to the changes which are caused by the advance of civilization. For example, a natural boundary may, in time, become merely conventional owing to development of communications.

At one time the Rhine was a natural boundary, but it has now become a channel of communication. Again, the Zambesi is at present a national boundary, completely separating distinct tribes; the time may come when it also will be a great channel of communication. The usual natural international boundaries are broad or rapid rivers and arms of the sea, mountain ranges, deserts, and swamps; but the highlands and lowlands of a country are also naturally separated, as they usually are inhabited by people of different nationality.

In Europe we find natural boundaries gradually losing their efficiency as political boundaries. The Rhine, for example, throughout a great portion of its length has ceased altogether to be a political boundary, for though it is still a military line of great strength, each large town on either bank has its suburb on the opposite side, and the population has become so assimilated that the river has ceased to be a practical political line. Consequently the line of the Vosges is deemed by many to have become the natural boundary between France and Germany, on account of its coinciding with the linguistic barrier. But, again, linguistic boundaries are no tests of the limits of nationalities or national feeling. When a foreign language is forced upon an unwilling people, they may for many generations be acutely opposed to the nation whose language they have adopted. On the Lower Danube, however, the physical, linguistic, and political divisions all coincide, and the river has become neutralized, and is a natural boundary.

In Central Europe we find the highlands of the Alps forming the natural and political boundary, though the people speak three different languages; but in these cases the people probably will not be found to be of the same race as those speaking the same language in the plains below.

Again, in the Pyrenees we find a natural, political, and linguistic barrier coinciding, assisted by the fact that the mountain people are a different race from those in the plains to the north and south.

In our own country we have a curious instance of language being no proof of the nationality of the people, as the Iberians in Wales speak Celtic, and the Celts in Western Britain speak Anglo-Saxon. Again, in South Africa we have the people of French extraction speaking Dutch, and still feeling resentment to the Government on account of its having forced a foreign language upon them, although the British have succeeded the Dutch.

Among Asiatic and African territories boundaries are often very ill-defined and uncertain. Frequently it happens that between two powerful States there is a large tract of country which owes a double allegiance, paying tribute to each, and yet in some respects remaining independent, probably consisting of lands which are easily ravaged and are comparatively speaking unprotected by Nature.

When we look into the subject of boundaries among pastoral tribes, we find curious anomalies. The land belongs in many instances to the tribe and not to the individual, and cannot be alienated. In the desert of Arabia a tribe in one part will have an interest in the date palms or corn lands of a tribe in another part, and this system is rather fostered than discountenanced, so that when evil befalls an individual in one part he may go and live with his tribal friends elsewhere. It is a knowledge of the intricate connexions of these tribes and the topographic divisions of their lands which admits of any control being kept over these warlike people. A mistake arising out of a misunderstanding of this Bedouin system nearly led to a disastrous result in the Egyptian campaign of 1882, owing to an outlying branch of one of the most powerful tribes in Arabia being supposed to be a petty independent tribe of no consequence.

In many instances the cattle posts of tribes during peace time by mutual consent intermingle and overlap, yet are kept separate and distinct, so that no geographical boundary is practicable; in fact among such people it is the tribe before the territory which is under the control of the chief. Thus it is quite practicable to conceive instances of a tribe living on lands within the area occupied by another tribe and yet governed by its own laws. Many of the difficulties the British have encountered in South Africa have arisen from a complete ignorance of, or wilfully ignoring, the native land laws. Under the tribal system even the chiefs in council have not the power of disposing of any portion of the land they use; it belongs to every individual of the tribe, and of the tribal branches, and to their children's children. Thus, when a chief gives over his territory, it does not follow that he gives over the land for disposal as Crown lands, but only the government of the people. It is on this account that the offer of Khama and other chiefs of the Bechuanaland territory was of so great value. They proposed by agreement in council in their respective territories to hand over to Great Britain their territories, keeping for themselves the lands they used, and offering for emigration purposes their vast extent of hunting lands, which are not now of the same value for hunting purposes as they were in former days.

But this proposal has not been accepted, and a parallel of latitude has been proclaimed without consent of the Bechuana chiefs as the northern limit of the British Protectorate, dividing Khama's territory into two parts, and cutting a portion of Matabeleland off from Lobongolo's territory; so that the Boers of the Transvaal cannot raid upon the Matabeles without violating the British Protectorate, and *vice versâ*, while we have no means of securing its protection. Again, the Matabeles when making their annual raid upon Lake Ngami will violate the portion of the State of Khama without the Protectorate, and he, if he wishes to oppose them, must do so from his capital within the Protectorate. This will bring us into conflict with the Matabeles, or else will practically deprive Khama of part of his territory.

It is difficult to conceive any arrangement more likely to lead to complications in the future. The Protectorate, based on geographical principles, should extend as far as the Zambesi, taking in all Khama's certain territory, and as much of the neutral territory as might be necessary to provide a natural boundary to east and west.

In East Africa, again, the definition of spheres of action recently is anomalous. A boundary ten miles from the coast for the Zanzibar dominions can of course have only a tentative character, and the exact definition in the future cannot fail to lead to conflicts. Far worse, however, is the adoption of the River Tana as the northern boundary of the British sphere of influence—a river occupied on both banks by the same agricultural tribes. It is not clear for what reason the Commissioners have left this difficulty for the future.

It would not be difficult to give many recent instances in which those charged with diplomatic definitions of international boundaries have failed in their duty owing to a want of geographical knowledge of the localities with which they had to deal.

For example, the boundary treaty of 1783 with the United States was incapable of being carried into effect, as the geographical features did not correspond with the assumption of the Commissioners. This led to a dispute lasting thirty years, resulting in the boundary treaty of August 9, 1843. The ignorance of the geography of the country in this case led to very inconvenient and even disastrous results.

Again with the San Juan controversy. Historical and geographical knowledge and ordinary care for the future development of Canada might have led to such measures having been taken in the first instance as would have prevented cession of valuable positions to the United States in 1846.

In India, again, our want of knowledge of the country to the north of the Afghan boundary has led to a series of unnecessary concessions to Russia. Had the slightest encouragement been given in former years by the Indian Government to enable officers to acquire information as to the territories beyond our Indian Empire, no doubt we should now be in a more secure position.

But, fortunately for the British Empire, foreign politicians have also much to answer for to their respective countries on account of their ignorance of geography.

For many years past Germany has been increasing the population of the United States and our own colonies without assisting to further the influence of the German Empire; whereas, had her statesmen been able to look forward, a German colony might have been established. Many Germans as far back as 1866 were desirous of establishing a colony in the Transvaal. But Germany now has to cast about for unoccupied territory, and has chosen a piece of useless territory on the western coast of South Africa, whereas with a little foresight Prince Bismarck might have obtained on easy terms the whole of the French colonies in the Gulf of Guinea and north of the Congo, which France had actually abandoned as worthless. Germany would thus probably have held the position of France with reference to the reversion of the Congo State.

By the Treaty of Frankfort it was intended that all German-speaking villages were to be ceded to Germany, but the boundary as originally laid down, for want of geographical knowledge on the part of German *employés*, left several German villages near Metz in possession of France, and it was necessary subsequently to rectify the error.

As a Section of the British Association we are interested in the development of geographical knowledge in the world generally, but more particularly in our own Empire, and it is only by unceasingly calling attention to our shortcomings with regard to the science which causes us to meet here to-day that we may hope for that progress to be made which will enable us to maintain the proud position we at present hold among nations owing to our practical skill and energy. Hitherto we have possessed so many other advantages that we have been able to dispense with a good system of instruction, but owing to many causes other nations are gaining upon us in various ways, and we in our turn should use every effort to successfully grapple with a subject which if properly taught must affect our welfare as a nation so deeply.

SECTION G.

MECHANICAL SCIENCE.

OPENING ADDRESS BY PROF. OSBORNE REYNOLDS, M.A., LL.D., F.R.S., M.INST.C.E., PRESIDENT OF THE SECTION.

AT a meeting of the British Association in Manchester the subjects of interest to the members of this Section are sure to be numerous, and the attendance of those members whose opinions on the various subjects presented the Section will like to hear is sure to be such that every moment of the time at the disposal of the Section will be well occupied. It is also particularly undesirable to prolong the sittings, and so reduce the opportunities of visiting the Exhibition and numerous works which abound with things which cannot fail to be of intense interest to members of this Section.

For these reasons I feel extremely unwilling to occupy the time of the Section with more than the briefest remarks by way of an address. Indeed, were it not that when in this chair in 1872 Sir Frederick Bramwell laid down the rule that for the President to break the custom of an address would be to show disrespect to the Section, I should have felt justified in consulting my inclination and proceeding at once with the regular work which lies before us.

It is now twenty-six years since the last meeting of this Section was held in Manchester, and it certainly seems fitting that in an address on this occasion something should be said as to the achievements in mechanical science accomplished in the interval. I wish sincerely that the task had fallen to some of you, gentlemen, whose far greater experience and power of expression would have enabled you to do justice to the subject. But under the circumstances I can only ask you to take it as a mark of my extreme respect for the Section, and proof of the appreciation in which I hold the honour conferred upon me in placing me in this chair, that I venture as a matter of duty to make a few remarks, of the inadequacy of which I am only too conscious.

It is always difficult to arrive at a just appreciation of the relative importance of the events of our own time; and in any endeavour to review or take stock of the mechanical advance of the last quarter of a century, during which time things mechanical have divided the attention of the civilized world with matters political, it seems very necessary to remember that as the mechanical age gets older its relative activity is not to be gauged by the relative number and importance of such epoch-marking mechanical departures as compared with those which have distinguished past periods.

If you recall—and again, to quote Sir Frederick Bramwell, the only purpose of an address is to force you to recall what you already know—in 1861 not only had we railways, ocean steam-ships, including the *Great Eastern*, still the giant of the tribe, a complete system of machinery for cotton and textile fabrics, besides the steam hammer, Armstrong's accumulator, and types of all machine tools, but also one attempt had been made to lay an Atlantic cable; the Suez Canal was in course of construction; if not perfected, the Bessemer process was in use; as were steam ploughs, steam threshing-machines, reaping-machines, and other agricultural machinery; we had also monster ironclads and rifled ordnance.

As new departures since 1861 which have already established themselves we have the telephone, the incandescent electric light, the dynamo and the secondary battery, the gas-engine and sewing-machine, not to mention the bicycle. We have also the tin can and freezing-machine and roller mills, as well as the machine gun and Whitehead torpedo.

One of these departures, the telephone, both from its usefulness and from the scientific interests which surround it, as affording, like the telescope, a means of directly increasing the power and range of one of our senses, must for ever remain recognized as a step in mechanical science for the introduction of which this period will be distinguished.

The sewing-machine, too, though little calculated to attract notice, in its influence on the welfare and appearance of all grades of society yields in importance to few, if any, previous mechanical steps. While the process of preserving food by means of the tin can and its more striking contemporary, the freezing-machine, direct results of the discoveries of Pasteur, have already opened up the food-producing resources of the whole world for the supply of the few chosen spots, and in doing so created a most welcome demand for further advance in the application of steam.

Great things have been and still are hoped from the electric departures which have interested us so much during the last few years; also of the gas-engine, which has most usefully occupied ground for which the steam-engine is not well adapted; and as to the importance of machine guns and torpedoes many will think the less the better.

However high or low an estimate we may form of the probable future importance of some of these inventions, and however much disappointment we may feel at the non-success which has attended some of the boldest and apparently most promising departures, such as the Crampton process for substituting a blast of coal-dust for the ordinary furnace, or Sir Henry Bessemer's endeavours to prevent distressing motion at sea, there is still no ground for discouragement.

For whether or not this period be henceforth remarkable for what, to borrow language from Section D, may be called the origination of new mechanical species, is a small matter compared with the fact that it has undoubtedly been remarkable for unprecedented achievements in the development of higher states of organization in those mechanical species which were already in existence.

There has never been a time in which mechanical revolutions have followed one another with such rapidity. In all the main departments of practical mechanics progress has been so rapid that appliances have been superseded long before reaching the term of their natural existence. There are some steamboats like the steel mail-boats between Dover and Calais still on the same service as in 1861, but very few, and only such as were then much in advance of their time. The Atlantic fleet of Royal mail-steamers has twice undergone complete revolution. Not only have the paddle-boats which constituted the Cunard line in 1861, and which included the *Scotia*, then new, entirely disappeared off the line, but the iron screw-steamers which displaced them have given place to the steel boats with compound engines—*Servia*, *Aurania*, *Etruria*, and *Umbria*.

In railway appliances the iron road has given place to that of steel, iron tires and locomotives to steel, the block system has become general, as have continuous brakes; while the carriages in which members have spent four hours and a quarter on their way from London to this meeting, although mostly still of the English plan, are very different in size and ease from those in which five hours and a half were spent in 1861.

In the works and mills the change is not less complete. It is, indeed, the change here that has not only rendered possible, but forced on, the revolution in our means of communication. The great step in the production of steel was already taken in 1861, and great results were then anticipated; there were, however, doubts and difficulties, and it was not for some years that sufficient mastery was obtained over the detail of the manufacture and use of the new material to bring about the general revolution which has therefore only reached its height during the last few years, if indeed it is yet reached—certainly it is yet far from complete.

To turn for one moment to the last year. Since the last meeting of this Section in Birmingham, the second Tay Bridge has been completed, over two miles long, having occupied only five years in construction.

The Severn Tunnel, one of the most difficult pieces of engineering ever attempted, has been completed and opened for passenger traffic.

The Forth Bridge, that structure the very thought of which causes those who have seen the place to hold their breaths, and of which the relative size may be best realized from the fact that, held out in arms an eighth part of a mile long, at a height of 200 feet above the sea, as a mother might hold out an infant, are structures no less than the single spans of the Britannia Bridge, 400 feet long. This gigantic structure, the progress of which Section G has watched since the meeting at Southampton, has now attained its full height of 360 feet, although otherwise not by any means fully formed.

Nor, as you well know, is it by the completion and progress only of great undertakings that this year is marked in the annals of engineering. It will be memorable, particularly in this district, as the year of the commencement of the Manchester Ship Canal. This undertaking, for which there is no precedent in this country, has excited so much interest that it cannot be otherwise than a matter of congratulation that a paper descriptive of this work is to be read before this meeting by the engineer, Mr. Leader Williams.

The completion of the Tay Bridge, the Severn Tunnel, the progress of the Forth Bridge, and the commencement of the Manchester Ship Canal in one year and in one country is sufficient assurance that, as yet, there is no lack of enterprise or sign of falling-off in heroic undertakings; nor are these by any means the only signs of great mechanical activity, notwithstanding the continual complaints of commercial depression.

In one direction, in particular, after many years of progress, so slow as to be something like stagnation, there has been a decided advance. The steam-engine is such a familiar institution, and has been for so long looked upon as the prime mover of our entire mechanical system, that anything which affects its welfare excites a deeper interest than would a mere mechanical advance. It was therefore with anything but a feeling of pure exultation that we heard and felt the force of predictions a very few years ago that the days of the supremacy of the steam-engine were numbered, that it would soon be a thing of the past, only to be found in the museum, a relic like Newcomen's engine and the stone implements by which our children would gauge the depth of mechanical barbarism of the age from which they had emerged. If sentiment be allowed in relation to anything mechanical, it must be with a sense of relief that it is now perceived how, so far from succumbing in the competition with what threatened to be formidable rivals, the only effect has been to bring about an important step in that internal development of the steam-engine which has been long looked for, but the accomplishment of which had for so long baffled the utmost efforts to bring it to a practical issue that it was almost despaired of—at least until it should be brought about by that circumstance which we all dread, the scarcity of coal.

The uppermost step of this advance yet reached is represented by the triple and quadruple expansion engines. These engines, of which the first seem to have been the triple engines of the *Propontis* in 1874, designed by Mr. Kirk, the next those of the steam yacht *Isa*, by Messrs. Douglas and Grant in 1878, and the third those of the *Aberdeen*, again by Mr. Kirk, in 1881, rapidly sprang into favour for cargo steamers, in which they have already proved of such advantage as to more than threaten the necessity of another revolution in steamships almost before the last is complete. Each week brings the announcement of some new accomplishment in the use of higher ratios of expansion and higher pressures of steam, so that while 60 or 70 pounds was the maximum three years ago, we now hear of 130, 150, and 175 pounds; and it is impossible to say to what they have not been carried at the present moment, and with commercial success.

There can be no doubt but that this latest step, as well as those of the surface-condenser, high-pressure boilers, and compound engines which led up to it have been the immediate results of the premium on economy of coal offered by the opening up of the long steam routes, first through the Suez Canal and recently round the Cape. But these steps must none the less be considered as the results of the unprecedented attention and labour, theoretical and practical, which has been devoted to this object during the last fifty years. They have been a result of the theoretical work of Carnot and Regnault, crowned by the great discoveries of Joule and Meyer, and the subsequent work of Rankine, Thomson, Clausens, and Hern, besides others, which, about the commencement of the period I am speaking of, accomplished that complete exposition of the principles underlying the internal economy of all heat-engines which have since furnished incitation and guidance to practical efforts. And not less have they been a result of the many practical attempts which have in the meantime been made to introduce similar and equally effective developments in the steam-engine without waiting till they were called forth by circumstances; as notable amongst which I may instance the labours and successes of Mr. Perkins, who has experimentally developed the organization of the steam-engine beyond any point it has commercially reached. Each and all of these efforts has undoubtedly taken part in that readiness to take the forward step, as soon as circumstances were favourable, which is as necessary to development as are the favourable circumstances themselves. The fact that a great advance has been made in the use of higher-class steam-engines, while it is the most gratifying circumstance one could have to record, affords the greatest encouragement to all those numerous workers for mechanical advance whose work is good, yet who do not see its immediate effect. It also emphasizes the lesson that the most perfect machine is that which is most perfectly adapted to the circumstances under which it has to work; and amongst these circumstances is efficient attendance, which involves sufficient knowledge of its requirements and familiarity with its detail on the part of those who have it in charge; and while in a process of gradual development this education is insured, in the case of a sudden step it is generally wanting.

How far the present advance towards the limits to economy which are theoretically evident may extend in the immediate future it would be dangerous to predict. The present rate is immense, and not by any means confined to the marine engine, although I am not aware of any other class of engine in which triple expansion has yet been adopted as a system. The recent compound pumping-engines have attained to a very high organization; and even in those classes of engines where economy of coal is more a matter of morality than of proved commercial importance, as mill engines and locomotives, great activity is evident in adapting and substituting compound engines, so as to allow of the use of greater pressures and higher degrees of expansion. The slow-breathing compound locomotive of Mr. Webb has drawn many members of this Association on their way to this meeting. Nor is the portable engine behind, as has been shown by the recent trials of the Royal Agricultural Society at York. The result of these trials cannot but offer the greatest encouragement to engine-makers of all kinds in their attempt at higher organization. It is indeed difficult to say which has been the most gratifying—the high state of economy which these trials have shown to be realized, or the reinstitution of the trials themselves after a lapse of twenty years, during which interval their non-continuance has called forth but one expression—that of regret.

These almost sudden steps towards the realization of efforts now extending over a century, to bring higher developments of the steam-engine into practical use, have not passed without notice. The interest and excitement amongst those more directly acquainted and concerned with the steam-engine and the use of steam are probably such as have not existed since the very early days of the railway. It is not, therefore, as something likely to be new to the members of this Section that I have dwelt upon it. Remembering that there was another subject other than actual mechanical achievements on which I was, as it were, in duty bound to say something, it seemed hopeless for me to attempt to touch on all the many advances towards a higher degree of organization in mechanics which constitute the mechanical feature of our era. I therefore have chosen this decided movement of the prime mover as the most significant and most gratifying, besides being of a kind the full importance of which is not so likely to be generally apprehended until pointed out, as the importance of advances such as the electrical and metallurgical, involving some new departure or novel application.

That the character and rate of recent mechanical advance are both exactly such as would be expected to follow as the result of a deeper and broader knowledge of scientific methods and the principles involved, seems to be the very best proof of advance in that other side of mechanical science in which this Section takes interest, or, more correctly, for which it exists—the increase and spread of mechanical knowledge.

It is as impossible as it is unnecessary for me to comment on the *furore* to which the movement first for popular scientific and now for technical instruction has reached—bringing into existence, by means of South Kensington, a complete system of sensibly free elementary scientific education over the country; then the City and Guilds Technical Schools, with a general system of examination; and culminating in a Parliamentary Commission on Technical Education, with the prospect of seeing its labours result in an Act of Parliament providing for absolutely free technical instruction.

Elementary education, whatever may be its subjects, must of necessity depend for its permanent existence on some source of higher knowledge in those subjects. Without raising such questions as whether there exist at present means of training efficient teachers in all the branches for which technical education is promised, or whether such means will be forthcoming as a result of the demand for teachers, I would recall to your attention the recent progress made towards a higher training in that branch of science which most directly relates to mechanical progress, and which, according to no less an authority than the late Prof. Rankine, received its first impulse from the institution of Section G.

So long ago as 1855, Rankine, in his characteristically concise address, dwelt upon the good work which this Section was doing in making it known that the application of the laws and principles of abstract mechanics to the purposes of practical mechanics constitutes a science of itself; a science the knowledge of which is essential before a knowledge of mathematics and abstract science can be of use to the practical engineer or mechanic; and for this science he then and there claimed the name of Applied Mechanics. As a proof of the influence of Section G in making known the usefulness of this science he instanced the apparent increase in the desire to profit by the lectures of the late Prof. Lewis Gordon which had taken place since the Section was instituted.

Prof. Gordon, who held the Chair of Mechanics in Glasgow University, was the first in this country to collect and embody in his lectures, and subsequently in a text-book, the important though scattered results of individual efforts to found the laws of practical mechanics on exact science. And at the time Rankine was speaking, this chair, to which Rankine himself was called the same year, was the only chair in this country from which such lectures were given.

Since that time the appreciation of that science has steadily increased; other colleges took up the subject mostly as forming part of courses entitled engineering or naval science. Amongst these was Owens College, in which, not till after the last meeting in Manchester of this Association, the leading engineers founded and endowed, which is more important, the chair which it has been my fortune to occupy for nineteen years.

During the earlier part of this time both teachers and students were labouring under the disadvantage arising from the novelty of the subject—the former having to make an almost arbitrary selection of what they would teach, and the latter not knowing exactly what it was they were going to learn. Gradually, however, by the help of experience from the somewhat earlier French schools and with the admirable works of Rankine as a foundation, the lectures or theoretical courses have become clear and distinct, while the advantage to be gained has become so generally recognized that of late years there has been almost a scramble to found new colleges to teach engineering or to introduce such teaching into existing colleges; and most satisfactory to those engaged in the introduction of this subject is the fact that it is from the engineers themselves that the interest and funds necessary for this work have come. Since 1867 the Owens College has received gifts and bequests from engineers, including those of highest standing in the neighbourhood, of upwards of £150,000. In the same way at Sheffield and at Leeds, where, as is well known, an engineering school has just been founded by Sir John Hawkshaw and the engineers of the town, and again at Liverpool.

It cannot for one moment be doubted that this movement has been brought about by the conviction of the necessity of an education which, in its subjects and methods of teaching, is much more closely related than was the older system of the Universities to the actual work which the students may eventually be called upon to undertake. That it is in fact evidence of the appreciation, by those having the greatest experience, of the necessity of higher scientific training for engineers. This is what engineering schools during their struggle for existence have endeavoured to supply. And in spite of the danger which seems to beset all schools as they become older, to fall into the academic or pure—not because it is the most desirable to be learnt, but because it is by far the easiest to teach—in spite of this danger, such in this case is the pressure from without, that it may be hoped the schools of engineering and applied science may be kept up to the mark, both in extending our knowledge of the laws and principles which more immediately underlie the results of practical experience in art, and in teaching the methods of most useful application; and that while encouraged to offer every inducement to the attainment of a sound knowledge of the principles, they will not be allowed to fall into the fatally easy errors of carrying the abstractions of this science outside all possible application, or blocking the way by the insistence on impossible preliminary attainments in mathematics and pure science.

To be hailed as one of the greatest inducements to keeping alive in engineering schools a real scientific interest in the practical work which is going on around them is the introduction of what are now called engineering laboratories, in which students may familiarize themselves with the actual subjects for which the theoretical work is undertaken, and have placed before them in their most naked forms the data and mechanical actions on which practical achievements depend, as well as being taught the use of all those instruments and methods of measurement which it is one of the first objects of these laboratories to extend and to perfect, and which measurements are now, as the result of a better knowledge of principles, rapidly displacing the older methods of arriving at conclusions in engineering.

It is to our Continental neighbours that we principally owe the origination of these laboratories as a means of research, but, as a system of instruction distinct from a workshop it owes much to Prof. Kennedy, who was, I believe, the first to introduce the testing machine and regular engine trials as part of the regular course of instruction for students in engineering, under the title of a laboratory course. The want of such a course must, however, it would seem, have been severely felt, to judge by the rapidity with which Prof. Kennedy's example has been followed in almost all the engineering schools in the country.

It is true that as adjuncts to academic institutions these laboratories can hardly be said to have passed the experimental stage, and it evidently remains to be seen whether when the present arrears of outstanding questions in engineering science are worked up, and the courses of instruction become stereotyped, sufficient variety of work will be found to justify the expense which, both as regards qualified instructors and maintenance of apparatus, must, as compared with the number of students receiving instruction, be greater than is general with academic instruction. At present, however, thanks to the liberality of engineers and their friends, there seems no ground for fear, each new laboratory being furnished with more complete and expensive apparatus than the last. During the erection and fitting of the Whitworth Laboratory in Owens College, which is only now on the verge of completion, it has been very impressing to see the goodwill shown toward the work by everybody who has had to do with it; the ready help of engineers of the greatest experience, like Mr. Rambottom and Mr. Robinson, who have spared neither time nor trouble in giving it the benefit of their experience; also by those who have undertaken the construction of the appliances, particularly Mr. William Mather, of Salford Iron Works, where neither trouble nor money has been considered in the efforts made to render the engines for the laboratory as perfectly adapted as possible to the very novel and numerous requirements. Taking this particular instance as evidence not only of the general feeling in favour of this movement, but also of the solid support it is to receive, one cannot help concluding that there is a great future before it; and that at last a method has been found of extending and spreading the higher knowledge of mechanical science which commends itself alike to the practical and theoretical.

Everyone who has paid attention to the history of mechanical progress must have been impressed by the smallness in number of recorded attempts to decide the broader questions in engineering by systematic experiments, as well as by the great results which in the long run have apparently followed as the effect of these few researches. I say apparently, because it is certain that there have been other researches which probably, on account of failure to attain some immediate object, have not been recorded, although they may have yielded valuable experience which, though not put on record, has, before it was forgotten, led to other attempts. But even discounting such lost researches, it is very evident that mechanical science was in the past very much hampered by the want of sufficient inducement to the undertaking of experiments to settle questions of the utmost importance to general advance, but which have not promised pecuniary returns—scientific questions which involved a greater sacrifice of time and money than individuals could afford. In recent periods the aid and encouragement which it has been one of the first objects of the British Association to afford such rosearches has led to many results of the greatest importance, both directly and indirectly, by the effect of example in calling forth aid from other institutions—that of mechanical engineers, for instance, which recently induced Mr. Tower to carry out his already celebrated research on "The Friction of Lubricated Journals," the results of which research certainly claim notice as constituting one of the most important of recent steps in mechanical science. Such investigations it is now the function as well as the interest of mechanical laboratories to undertake, and thus what has hitherto been a great obstacle in the path of mechanical progress seems in a fair way to be removed and steady advance to be insured.

To what all this may lead us it is no part of my undertaking to consider, but I venture to end this imperfect address on the progress of mechanical science during the past twenty-six years by what appears to me the most satisfactory conclusion—viz. that to such mechanical progress there is apparently no end: for, as in the past so in the future, each step in any direction will remove limits and carry us past barriers which have till then blocked the way in other directions; and so what for the time may appear to be a visible end or practical limit will turn out but a bend in the road.

NOTES.

DR. JOHANNES SKALWEIT, the well-known chemist, has died at Hanover, of heart disease. The deceased was in the prime of life, and enjoyed a high reputation all over the Continent. He was, according to the *Times*, President of the German Union of Analytical Chemists—whose annual conference has been postponed in consequence of his death—and editor of the *Repertorium für Analytische Chemie.* A large number of essays and other short works on questions of sanitary science, State medicine, and chemical analysis have issued from his pen. Among the most important may be mentioned "Ueber Fette im Polarisirten Licht" (Hanover, 1879); "Ueber die Titration der Phosphorsaüre mit Uran" (Hanover, 1880); "In wie weit ist der heutige Kampf gegen die Lebensmittelfälschung gerechtfertigt?" (Hanover, 1880); "Ueber die Beziehungen zwischen Bauordnung und Oeffentlicher Gesundheitspflege" (Magdeburg, 1885). Dr. Skalweit was an authority on milk and butter analysis.

SIR WILLIAM GROVE, F.R.S., has resigned his seat on the Bench as Judge of the High Court of Justice.

THE Secretary of State for India has sanctioned the appointment of a scientific assistant in the Revenue and Agricultural Department of the Government of India, and Dr. Watt, C.I.E., has been selected for the office.

MR. G. BROWN GOODE has been appointed United States Commissioner of Fish and Fisheries in succession to the late Prof. Spencer Baird. *Science* approves highly of the appointment, observing that it meets at once the requirements of an exacting office and the exceptional provisions of the law creating it. "Prof. Goode was intimately acquainted with the methods of Commissioner Baird, whose scientific zeal and knowledge he shared, and his experience and attainments in practical fish culture and in the science of ichthyology made him easily first among those whose qualifications the President has been called upon to consider." At the same time it regards the provisions of the law under which the appointment was made as sadly in need of amendment, for under them the Fish Commissioner is not paid a salary commensurate with the importance of his office, and discharges the duties of two offices for the pay of one.

ACCORDING to a Reuter's telegram, dated September 9, from St. Paul de Loanda, Major Barttelot, who was left at the camp at Yambunga at the foot of the Aruwhimi Rapids with a garrison of about 100 men, has forwarded the following information to Leopoldville concerning Mr. H. M. Stanley's Expedition:—"Major Barttelot received news from Mr. Stanley, despatched about July 12, after he had made a ten days' march from Yambunga towards the interior. Mr. Stanley was at that date still proceeding up the Aruwhimi, which he had found to be navigable up to a certain distance above the rapids. Here he launched a steel whale-boat which he had brought with him, as well as several rafts manufactured by the Expedition, and which had been utilized for conveying the heavy baggage. All the members of the Expedition were in good health, and provisions were easily procured in the large villages near the river. The country through which the Expedition was passing showed a gradual rise towards some high table-lands. Another caravan of 480 men was following the Expedition on the left bank of the Aruwhimi, the advanced guard, consisting of forty Zanzibaris, under the command of Lieut. Stairs, being composed of men lightly burdened, whose duty was to search for provisions. Mr. Stanley hoped to arrive about July 22 in the centre of the Mabodi district, and expected to reach Wadelai in the middle of August, or even before. The advance had been so peaceably accomplished that Mr. Stanley had instructed Major Barttelot that, should it continue so, he would shortly send him orders to follow the Expedition by the same route at the head of the 100 men left at Yambunga." Major Barttelot had paid a visit to the Falls, accompanied by Tippoo Tib, and had left a detachment of twenty men there. Tippoo Tib arrived at the Falls Station on June 16.

IN moving the second reading of the Coal Mines Regulation Bill in the House of Lords on the evening of the 7th instant, Viscount Cross said he took that opportunity of tendering the thanks of the Government to the Royal Society for the trouble they had taken in the matter of coal-mines. In the year 1879 he had asked the Society to join, or send some of their members to assist, the Royal Commission which was then appointed for the purpose of seeing how accidents in mines could best be prevented. The Royal Society, he said, met the appeal in the most handsome way, and several of their most distinguished members served on the Commission. The labours of the Commission lasted for a period of six years; they went minutely into a long series of experiments, and while he was quite sure the results of those experiments would tend greatly to the safety of life and the prevention of accidents, it was satisfactory to know that they had also added very much to their own scientific knowledge, because he believed the members of the Commission all candidly admitted that in the course of their investigations they made several discoveries about gases and other matters that were absolutely unknown to them before. The result had been that a great many of their recommendations had been embodied in this Bill.

THE International Medical Congress at Washington held its final sitting on the 10th inst. The meeting for 1890 will be held at Berlin, with Prof. Virchow as President.

THE Technical Schools (Scotland) Bill was read a third time in the House of Commons on Friday night last, and a second time in the House of Lords on Tuesday night.

THE Japanese Minister of Education has invited the Seismological Society, the Institute of Architects, the Association of Engineers, and the Physical and Mathematical Society to appoint a joint committee to examine and report upon the type of buildings best calculated to resist the effects of earthquake shocks, as well as the methods by which these effects may best be mitigated. Formerly Japanese houses were built wholly of wood; but of late years brick and stone are being largely employed. Many public edifices are now constructed of masonry, and almost all public buildings will in future be of this character. The Minister of Education accordingly thinks it well that the subject of the protection of these from the results of earthquake shocks should be carefully studied by competent persons.

THE meeting of the Astronomische Gesellshaft at Kiel, under the presidency of M. Auwers, terminated on the 31st ult. The meeting for 1889 will be held at Brussels. Amongst the papers read was one by M. Peters, of Kiel, on the causes of error in marine chronometers, based on observations in the German marine. One source of error was found to be variations similar to those caused by differences of temperature, and produced by the great humidity of the air at sea. Prof. Gylden described a new and simple method of maintaining chronometers at a constant temperature. There were some discussions on the orbits of comets. Prof. Shœrer read an historical paper on sunspots, based on a manuscript of Stolberg, commenced in 1749 and concluded in 1799. From this it appears that the periods observed in 1700 were quite different from those noticed in our own day. Between 1645 and 1670 the spots were very much fewer. Other circumstances also show that these phenomena are subject to curious vicissitudes. At the final meeting on August 31 the Congress was mainly occupied with photography. M. Hartwig presented a plan of the Bamberg Observatory, and M. Hertz described the new Observatory at Vienna.

WE have received the programme of Technological Examinations for the coming year of the City and Guilds of London Institute. Amongst the alterations and additions for the year are the following: the grant made to teachers on account of students who are awarded the full technological certificate in the honours grade of any subject is £3 for a first class, and £2 for a second class certificate; the examination in subject 29 is divided into two parts; the syllabus of 16B has been reconstructed; the syllabus in subject 2 and also that in 3A are new, and in many other cases the syllabuses have been revised and altered.

WE have received the Calendar of the University College, Dundee, for the coming academical year. The volume also contains the report of the Principal for the session 1886-87. The latter exhibits very satisfactory progress in every direction.

MR. ALVAN CLARK, of Cambridgeport, Mass., U.S.A., who died the other day at the age of eighty-three, had made for himself a splendid reputation as an optician. Mr. Clark was what is called a self-made man, as his education was neglected, and at the age of seventeen he was thrown on his own resources. For some time he supported himself by engraving for calico-printing at Lowell. Afterwards he became a painter of miniature portraits on ivory. In 1835 he opened a studio at Boston, but in the following year he removed with his wife and family to Cambridgeport, where he ever afterwards resided. His attention was attracted to the making of telescopes almost by accident, and he began his labours in this department with telescopes of small sizes. His first success was a 4½-inch instrument, with which he discovered that the star 8 Sextantis is double. Other discoveries soon made his lenses well known; and he may be said to have established his reputation by the production of the 18-inch refractor ordered for the University of the Mississippi. The use of this glass, before the completion of the telescope, led to the discovery of the companion of Sirius—a discovery which was rewarded with the Lalande medal of the Paris Academy of Sciences. Since that time some of the largest telescopes in existence have been made by Mr. Clark and his sons. The telescope constructed by them for the Naval Observatory at Washington is of 26 inches aperture, and the magnificent instrument now being made for the Lick Observatory, Mount Hamilton, California, is of 36 inches aperture. Clark telescopes are known in every part of the world where astronomy is seriously studied.

THE inaugural address at St. Thomas's Hospital Medical School, at the commencement of the session 1887–88, will be delivered in the theatre of the hospital on October 1 at 3 p.m. by Mr. R. W. Reid, F.R.C.S.

A NOTE was presented by M. Ch. V. Zenger, at the meeting of the Paris Academy of Sciences on September 5, on a possible relation between the periodical showers of shooting-stars and the occurrence of fires of unknown origin. From a study of the statistics for several years, he infers that such coincidences are extremely frequent, the fires usually breaking out in woods, farmsteads, barns, mills, and also in villages and even in large towns. He points out that during the period from August 1 to August 18, 1887, violent storms, rich meteoric displays, and conflagrations were of frequent occurrence.

PROF. DITTMAR, of the Glasgow and West of Scotland Technical College, is about to publish a series of exercises in quantitative chemical analysis, with a treatise on gas analysis. The publishers are Hodge and Co., Glasgow.

A NEW gaseous oxide of manganese, of the composition MnO_4, has been discovered by Dr. Franke (*Journ. für praktische Chemie*, 1887, No. 14, p. 166). This new gas, which possesses a dark blue colour, is readily obtained by passing a current of carbonic acid gas, saturated at 40° to 50° C. with aqueous vapour over the new oxysulphate of manganese, $(MnO_3)_2SO_4$, recently described by Dr. Franke; the issuing mixture of gases is afterwards passed through two U-tubes, in the first of which the less volatile MnO_3 condenses out, while the MnO_4 may be condensed at a lower temperature in the second to a bluish-violet amorphous body, which after long shaking dissolves in water with evolution of oxygen and formation of a bright red solution which is found to contain manganic acid. The properties of the tetroxide MnO_4 are sharply distinguished both from those of the trioxide MnO_3 and those of the heptoxide Mn_2O_7, in fact it is possible, owing to its small affinity for water, to collect the gas at the pneumatic trough. The decomposition of the oxysulphate by aqueous vapour probably occurs as follows: $(MnO_3)_2SO_4 + H_2O = MnO_4 + MnO_2 + H_2SO_4$.

IN addition to this most interesting gaseous oxide, Dr. Franke has also prepared a crystalline oxide of the composition Mn_5O_8, by treating a new double sulphate of manganese and potassium, $2Mn_5(SO_4)_8 . 5K_2SO_4$, with a large quantity of water, when brilliant yellow tabular crystals of Mn_5O_8 fall out, and may be isolated by removal of the supernatant acid liquid by decantation. After washing successively with water, alcohol, and ether, and drying at 80° to 100°, the oxide has the appearance of a brownish-black mass, which on closer examination is found to consist of small yellowish metallic-looking plates. Dilute sulphuric acid decomposes it with formation of two molecules of $MnSO_4$, and one molecule of the hydrate of manganese dioxide, $3MnO_2 . 2H_2O$; hence this new oxide may be considered to have the constitution $3MnO_2 . 2MnO$.

THE fauna of the Kirghiz Steppe is the subject of a short but suggestive paper by M. Nazaroff, in the Bulletin of the Moscow Naturalists (vol. lxii., No. 4). The zoological sketch is preceded by a very clear summary of the orography and geology of the region, and by a picture of its present vegetation; a map shows the limits of the forest tracts, the meadow-land with scattered, mostly deciduous, forests, the steppe-land covered with *Stipa*, the *Artemisia* steppe, and the deserts. The animal inhabitants of these different sub-regions—mammals and birds—are described and tabulated, and very interesting conclusions are arrived at. It appears that when most of Russia was covered with the immense Scandinavian and Finnish ice-cap, and the Aral-Caspian Sea covered the steppes, the Southern Urals—as already pointed out by M. Menzbier in his "Géographie Ornithologique"—remained a refuge for many animal species. Owing to the greater moisture, the vegetation of the region was much richer than now, and thus it provided plenty of food for many animals, some of which were immigrants from the north, while others came from the south. It is for this reason that the fauna of the Southern Urals offers now such a great variety of forms. Many species have abandoned the region recently. In the last century the *Equus onager* and the wild horse were numerous in the Southern Urals, while the castor was common in the land of the Bashkirs, bears and *Cervus alces* were frequent in the steppe, and tigers reached Turgai. The corsac fox went as far as the 51st degree of latitude, and the sub-polar *Arctomys bobac* was widely spread in the forest region, while the reindeer, now found only beyond the 53rd degree, reached the southern parts of the Urals. Immense herds of antelopes abandoned the region only about thirty years ago. Notwithstanding this notable diminution of species inhabiting the Kirghiz Steppe, its fauna is still remarkably varied. As a whole the paper of M. Nazaroff, revised by M. Menzbier, will be most welcome to zoo-geographers. It is one of those partial, but not narrowly conceived descriptions of a limited region which are most needed now, when materials are so rapidly accumulating.

THE additions to the Zoological Society's Gardens during the past week include a Larger Hill Mynah (*Gracula religiosa*) from India, presented by Mr. P. Wilmot Bennitt, F.Z.S.; a Peaceful Dove (*Geopelia tranquilla*) from Australia, presented by Mr. R. O. Law Ogilby; a Green Bittern (*Butorides virescens*) from the West Indies, presented by Miss Mayrick; a Mexican Crocodile (*Crocodilus rhombifer*) from the West Indies, presented by Capt. J. Smith, s.s. *Godiva*; seven Angulated Tortoises (*Chersina angulata*), two Hoary Snakes (*Coronella cana*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; ten Short-nosed Sea-horses (*Hippocampus antiquorum*) from European coasts, presented by Prof. W. H. Flower, C.B., F.R.S.; a ——— Flying Squirrel (*Pteromys* ———) from Szechuen, China, presented by Mr. Percy Montgomery; a ——— Frog (*Discoglossus pictus*) from Sardinia, presented by Mr. Alban Doran, F.R.C.S.; an Oyster-catcher (*Hæmatopus ostralegus*) British, nine Smaller Rattlesnakes (*Crotalus miliarius*), four Testaceous Snakes (*Ptyas testacea*), two Alleghany Snakes (*Coluber alleghaniensis*), seven Milk Snakes (*Coluber eximius*), a Seven-banded Snake (*Tropidonotus leberis*), a Striped Snake (*Tropidonotus sirtalis*) from North America, deposited; two Common Squirrels (*Sciurus vulgaris*) British, a White-eyebrowed Guan (*Penelope superciliaris*) from North-East Brazil, two Smaller Rattlesnakes (*Crotalus miliarius*), a Testaceous Snake (*Ptyas testacea*), two Milk Snakes (*Coluber eximius*), a Copper-bellied Snake (*Tropidonotus erythrogaster*) from North America, purchased.

OUR ASTRONOMICAL COLUMN.

THE NEUCHÂTEL OBSERVATORY.—Dr. Hirsch, the Director of the Neuchâtel Observatory, has published his Annual Report for the year 1886, dated June 21, 1887. On the whole, Dr. Hirsch reports that, as far as his Observatory is concerned, the year 1886 was somewhat more favourable for astronomical observations than was 1885. In 1886 there were 154 nights on which observations were made, and 124 days on which no observations were possible, the longest interval without observations having been 7 days; whilst in 1885 the number of observing nights was 150, the number of days without observations 135, and the longest interval without observations 20 days. The meridian observations made during the year comprise 192 observations of the sun, 16 of planets, 1401 of fundamental stars, and 909 of stars contained in M. Lœwy's Catalogue of Moon-Culminating and Longitude Stars. The equatorial telescope has been employed in the observation of planets and comets with the ring micrometer, the position micrometer not being available until the small incandescent lamps, which are to be provided for purposes of illumination, have been supplied. Dr. Hirsch gives some interesting particulars with regard to the azimuthal movements of the meridian circle, as well as of the distant marks used for determination of azimuth error. The maximum easterly azimuth (+ 3·03s.) of the meridian circle during the year was observed on March 11, whilst the maximum westerly azimuth (− 1·02s.) was observed on September 1; the total range throughout the year 1886 was therefore 4·05s., the corresponding mean value for 22 years being 5·20s. It appears, however, that the three meridian marks (two to the north and one to the south) do not participate in this movement. The azimuth of one of the north marks, situated at a distance of 100 m. from the Observatory, varied during 1886 between + 0·24s. on May 19 and − 0·25s. on August 29, thus showing a total range of only 0·49s. The other more distant north mark showed a range of 0·42s., the extremes being + 0·18s. on April 16 and − 0·24s. on August 7; whilst the south mark at about 10 km. distance varied from + 0·40s. on May 14 to − 0·01s. on July 3, the range being therefore 0·41s. The marks are consequently well adapted for determining the azimuth of the meridian circle, the mean of the three giving this element, according to Dr. Hirsch's estimation, to ± 0·012s. nearly.

THE WEDGE PHOTOMETER.—Prof. Pickering has recently published two papers relating to the wedge photometer employed by Prof. Pritchard in the formation of his Oxford Uranometria. The first of these, forming No. 2 of the papers comprising vol. xviii. of the Annals of the Harvard College Observatory, consists in a detailed comparison of the Oxford magnitudes with those of Wolff's second Catalogue, and of the Harvard Photometry. The latter comparison appears to show the existence of some real, though small, systematic error, since the mean difference of magnitude between the two catalogues changes with the brightness of the star; the Oxford magnitude being on the average less than the Harvard magnitude for stars down to the third magnitude, but greater for the fourth and fifth, and less again for stars below the sixth magnitude. If stars below the sixth magnitude be put aside as influenced by some special cause not yet ascertained, the results would seem to suggest the use of a different constant in the reduction of the observations made by means of the wedge from that actually employed. The comparison of the three catalogues gives the following result: mean deviation of Wolff's catalogue from that of the Harvard College, 0·140; Oxford from Harvard, 0·146; Oxford from Wolff's, 0·191. On the whole, therefore, the mean outstanding differences are but small, but will evidently repay further and detailed investigation.

The second paper, which was presented to the American Academy of Arts and Sciences last November, is an investigation into the behaviour of a wedge photometer similar to those used by Prof. Pritchard. Its first portion contains some trial observations made by Prof. Young with a wedge photometer attached to the great Princeton refractor on the stars in the "region following γ Pegasi" of the American Association star magnitude charts. The result was decidedly favourable to the wedge, as, though all the stars observed were faint, the magnitudes ranging from 10 to 13, the probable error of the magnitude determined from four nights' observations was ± 0·04. The second portion of the paper contains a very full and interesting examination by Prof. Langley, by means of his bolometer, of the coefficients of transmission of the wedge. Here again the result of the examination is, on the whole, favourable to the wedge within the limits that Prof. Pritchard employed it; but it appears that there is a remarkable variation in the coefficient of transmission for the different rays of the spectrum. Speaking broadly, the transmissibility always increases from the violet towards the red, increasing very greatly in the infra-red. Within the visible portion of the spectrum the change in the transmissibility only becomes great as the red is approached, and as Prof. Pritchard had always recognized the inapplicability of the wedge photometer to deeply-coloured stars, this selective absorption will probably have but little affected its practical value in the work on which it was actually employed.

BROOKS'S COMET.—Dr. Franz gives in the *Dun Echt Circular*, No. 151, the following elements and ephemeris for this object, based upon observations obtained with the Königsberg heliometer on August 27, 28, and 29:—

$$T = 1887 \text{ October } 13{\cdot}6623 \text{ Berlin M.T.}$$

$$\left.\begin{array}{l} \pi - \Omega = 72^\circ\ 9'{\cdot}7 \\ \Omega = 85\ 29{\cdot}2 \\ \iota = 45\ 49{\cdot}0 \end{array}\right\} \text{Mean Eq. } 1887{\cdot}0.$$

$$\log q = 0{\cdot}08481$$

For Berlin Midnight.

1887.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Sept. 16	10 16 6	29 55·3 N.	0·0861	0·2843	1·5
20	10 36 30	29 31·3	0·0785	0·2779	1·6
24	10 57 10	28 56·5	0·0719	0·2725	1·7
24	11 17 57	28 10·4 N.	0·0663	0·2682	1·8

The brightness on August 27 is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 SEPTEMBER 18–24.

(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 18

Sun rises, 5h. 41m.; souths, 11h. 54m. 8·9s.; sets, 18h. 7m.; decl. on meridian, 1° 54′ N.: Sidereal Time at Sunset, 17h. 56m.

Moon (at First Quarter Sept. 24, 5h.) rises, 6h. 30m.; souths, 12h. 50m.; sets, 18h. 55m.; decl. on meridian, 0° 11′ N.

Planet	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	6 15	12 20	18 25	0 15 N.
Venus	6 36	11 59	17 22	8 2 S.
Mars	1 42	9 20	16 58	17 45 N.
Jupiter	9 27	14 26	19 25	12 28 S.
Saturn	0 44	8 35	16 26	19 38 N.

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. ° °
24	29 Sagittarii	6	22 43	23 24	180 264

Sept.	h.	
20	6	Jupiter in conjunction with and 4° 18′ south of the Moon.
21	16	Venus in inferior conjunction with the Sun.
23	9	Sun in equator.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
λ Tauri	3 54·4	12 10 N.	Sept. 19,	0 51	*m*
			,, 22,	23 43	*m*
R Canis Minoris	7 2·5	10 12 N.	,, 21,		*M*
U Monocerotis	7 25·4	9 33 S.	,, 24,		*M*
S Cancri	8 37·5	19 26 N.	,, 18,	3 39	*m*
δ Libræ	14 54·9	8 4 S.	,, 19,	3 39	*m*
			,, 23,	19 22	*m*
U Coronæ	15 13·6	32 4 N.	,, 23,	0 17	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 21,	3 59	*m*
			and at intervals of	20 8	
X Sagittarii	17 40·5	27 47 S.	Sept. 21,	23 0	*m*
			,, 24,	20 0	*M*
W Sagittarii	17 57·8	29 35 S.	,, 19,	19 0	*m*
β Lyræ	18 45·9	33 14 N.	,, 21,	22 0	*M*
η Aquilæ	19 46·7	0 43 N.	,, 18,	23 0	*m*
δ Cephei	22 25·0	57 50 N.	,, 22,	20 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near α Arietis	31°	20° N.	
,, η Aurigæ	74	41 N.	Swift; streaks.
,, δ Draconis	290	70 N.	Swift.

GEOGRAPHICAL NOTES.

To the September number of *Petermann's Mitteilungen* Dr. G. Gürich contributes a useful sketch of the geological structure of the African continent. Much of his article is devoted to the Atlas and Cape regions, and that naturally, since on these regions the material is most abundant, only scanty notes for the most part being available for Central Africa, where, however, the prominence of granite Dr. Gürich thinks specially noteworthy. To the same number Dr. Baumann, the companion of Dr. Lenz, contributes a fairly detailed study of the physical geography of Fernando Po, where he stayed for some time on his return from the Congo. Both papers are illustrated by maps, that of Fernando Po being a specially good one on a large scale. Dr. Radde continues his preliminary account of his journeys in 1886 into the Transcaspian region and North Khorassan.

HERR ED. GLASER contemplates a third journey into Southern Arabia, and will attempt to explore the northern and eastern part of the old Sabæan kingdom, which in his first two journeys he was not able to reach. If political conditions permit, he will also cross the Serat Mountains into a part of Hadramaut hitherto almost unexplored.

AN original communication of some value on the aboriginal Indian races of Vera Cruz, Mexico, by Consul A. Baker, will be found in the September number of the Proceedings of the Royal Geographical Society.

IN the course of a short exploratory visit of six weeks' duration, in March and April last, to the delta region known as Aird River, New Guinea, Mr. Theodore Bevan made the important discovery of two large rivers, flowing from the interior highlands, at a distance apart of about 60 miles, into that part of the Gulf of Papua. One of these, the Douglas, enters the head of the Aird delta, and the other discharges at Bald Head; both rivers were navigated for about 100 miles.

WITH reference to the recent Russian expedition to the New Siberian Islands, we learn further that Von Soll made a special exploration of the mountain in New Siberia known to travellers of the beginning of the present century as the "Wood Mountain," which was found to be a beautiful Tertiary profile with carbonized tree trunks, and a rich collection of leaf impressions and fruits, corresponding exactly with the Tertiary flora of Greenland and Spitzbergen, as described by Heer. He made a complete circuit of Kotelnoy Island in forty days, obtaining from the northern point a view of the still untrodden land of Ssanikow, 100 miles distant. The northern part of Kotelnoy is Devonian, and the southern Trias. On Liakov Island, Dr. Bunge found that, with the exception of some granite peaks, the prevailing formation is Quaternary. The ice blocks are covered with loamy deposits, in which are found fossil bones. Besides the fossil remains of the mammoth, rhinoceros, and musk-ox, Dr. Bunge discovered the remains of two species of oxen, deer, horses, and some smaller animals. About seventy Phanerogams were collected. Both birds and insects are poorly represented.

THE India-rubber, Gutta-percha, and Telegraph Works Company have issued tables of the soundings taken by their vessels in 1885–87 in the two Havanna expeditions, the second West African expedition, and the Congo repairs expedition.

GRANTS FOR SCIENCE AND ART INSTRUCTION.

A SUMMARY of grants made by the Department of Science and Art is printed in the new number of the "Directory," issued by the Department, containing regulations for establishing and conducting Science and Art schools and classes. The summary is as follows:—

Science.	Art.
1. Payments to the Local Committees of Schools and Classes on the results of instruction, as tested by Examination, of Students of the Industrial Classes.	
(*a*) £2 and £1 for a 1st and 2nd class respectively in the Elementary and Advanced Stage of each subject. (*b*) £4 and £2 for a 1st and 2nd class respectively in Honours. (*c*) In Practical Chemistry and Practical Metallurgy £2 and £1 for a 1st and 2nd class respectively in the Elementary Stage, £3 and £2 in the Advanced Stage, and £4 and £3 in Honours.	(*a*) £1 and 10*s*. for a 1st and 2nd class respectively in each subject of the 2nd Grade Examination, *including Modelling*. (*b*) £1 10*s*. and £3 for a 1st and 2nd class respectively in 3rd Grade Examination. (*c*) £2 or less per student for works executed in local classes. (*d*) £3 each on account of Free Students (being artisans) under certain conditions. (*e*) £15 each for not more than two Art Pupil Teachers. (*f*) £5 for each student who obtains a National Scholarship or who obtains admission to Training Class.
2. Prizes and medals are awarded to candidates.	
(*a*) Prizes to students obtaining a 1st Class in the Advanced Stage of each subject, and Bronze Medals to those obtaining a 1st Class in Honours. Certificates or cards to all successful candidates.	(*a*) Prizes of books or instruments, to the value of 8*s*. and 12*s*., to students obtaining the mark "excellent" in the 2nd and 3rd Grade Examinations, respectively; and Gold, Silver, and Bronze Medals, and other prizes of Books, for the best works submitted in the National Competition of works of all the Schools of Art and Art Classes.

3. Science and Art Scholarships for Students of the Industrial Class, held locally, £4, £7, and £10, for the 1st, 2nd, and 3rd year respectively, on condition that a local contribution of £5 a year is made.

4. Local Exhibitions, to be held by Students of the Industrial Classes at the Normal School of Science and Royal School of Mines, London, the Royal College of Science, Dublin, or at an approved Provincial Science College, £25 to meet an equal sum locally raised.

5. Grants for Buildings, Fittings, and Apparatus.

Science.	Art.
(*a*) Not exceeding 2*s*. 6*d*. per square foot of internal area up to a maximum of £500 for buildings. (*b*) Grants towards the purchase of fittings, apparatus, examples, &c., not exceeding 50 per cent. of their cost within certain limits.	(*a*) Not exceeding 2*s*. 6*d*. per square foot of internal area up to a maximum of £500 for buildings. (*b*) Grants towards the purchase of fittings, apparatus, examples, &c., not exceeding 50 per cent. of their cost and within certain limits.

6. Special Grants to Organized Science Schools in addition to the foregoing. 10*s*. and 5*s*. respectively for each student who attends a day or an evening school not less than 250 or 75 times in the year.

7. Aid to Students in attending the Normal School of Science and Royal School of Mines, London, the National Art Training School, London, and the Royal College of Science, Dublin.

Science.	Art.
(*a*) 21 Royal Exhibitions (seven awarded each year) with maintenance allowance of £50 a year tenable for three years. (*b*) 36 National Scholarships (twelve awarded each year) with maintenance allowance of 30*s*. a week for 40 weeks in the year tenable for three years. (*c*) 18 Free Studentships (six awarded each year) tenable for three years, at Normal School of Science and Royal School of Mines, London.	(*a*) National Scholarships tenable for not more than three years at National Art Training School with maintenance allowance of 20*s*. a week. (*b*) Free Studentships in National Art Training School.

8. Aid to teachers and persons preparing to become teachers in attending the Normal School of Science and Royal School of Mines, London, the National Art Training School, London, the Royal College of Science, Dublin, and Provincial Colleges at which advanced instruction in Science is given.

Science.	Art.
(*a*) Grants of £2 each with travelling expenses to local teachers selected to attend short courses of instruction at Normal School of Science and Royal School of Mines. (*b*) Grants of 21*s*. a week each with travelling expenses to teachers in training selected to attend the sessional courses of the Normal School of Science and Royal School of Mines. (*c*) Grants in aid of fees to local teachers selected to attend Provincial Science Colleges. (*d*) Free admission (subject to payment of examination fee) to courses of lectures at Normal School of Science and Royal School of Mines and Royal College of Science, to Science teachers.	(*a*) Grants to enable masters and students to visit various metropolitan Art Institutions, and, in special cases, foreign towns, schools, and galleries. (*b*) Grants of from 10*s*. to 35*s*. a week with travelling expenses to teachers in training selected to attend the National Art Training School.

9. Grants to Local Museums and Loans of works of Science and Art, Books, and specimen sets of teaching Apparatus, to Science and Art Schools.

10. Aid to Training Colleges for Instruction in Science and Art.

Science.	Art.
(*a*) Grants of £3 and £1 10*s*. respectively for each 1st and 2nd Class obtained at the Annual Examination. In Practical Chemistry £3 and £2. (*b*) Grants not exceeding 50 per cent. of the cost for apparatus and fittings.	(*a*) Grants of 10*s*. in respect of each subject of examination in which a resident student passes. (*b*) Grants of 50 per cent. towards the cost of examples.

11. Aid to Elementary Schools for Instruction in Drawing.

Science.	Art.
	(*a*) Grants of 1*s*., 1*s*. 6*d*., or 2*s*. on average attendance of Schools examined in Drawing. (*b*) Grants of 10*s*. for each pass in 2nd Grade Examinations.

12. Aid towards expenses of Examinations.

(*a*) Grants of 50 per cent. towards the fees of Special Local Secretaries and their Assistants for conducting annual examinations of Science and Art Schools and Classes.

SCIENTIFIC SERIALS.

American Journal of Science, July.—The viscosity of steel and its relations to temperature, by Carl Barus. In this paper the author's studies are mainly restricted to a discussion of the relation between torsional viscosity and temperature as observed with steel in different states of hardness. Reference is also made to the effect of stress on the amount of viscous motion in solids, and to a more general method by which the instantaneous deformation and the gradual deformation produced by stress may be co-ordinated. It is shown that imperceptible gradations lead from the purely viscous deformation which follows strains within the elastic limits to the sudden permanent set which follows strains beyond those limits.—Kilauea in 1880, by William T. Brigham. A detailed account is given of the results of the outbreak of May 1, 1880, with a description of the changes that had taken place since the author's previous visit in 1865. The trigonometrical survey then made was found to be already antiquated, the whole boundary perceptibly changed, and Kilauea apparently 5 per cent. larger than eighteen years previously.—Recent explorations in the Wappinger Valley lime stone of Dutchess County, New York (continued), by W. B.

Dwight. In this paper (No. 6 of the series) the author deals with the discovery of additional fossiliferous Potsdam strata and pre-Potsdam strata of the Olenellus group near Poughkeepsie. This review of the latest palæontological facts makes it evident that the strata in Dutchess County are simply the continuation of the strata characterizing the Taconic and adjoining series lying northward. But while proving a grand unity, they indicate also an interesting and unexpected variety of rock structure.—Image transference, by M. Carey Lea. By image transference are here denoted curious effects produced on sensitive films, and specially interesting in connexion with the subjects of papers which appeared in the May and June numbers of the journal. In supplement to those papers the possibility is here shown of developing on a film of silver haloid a complete image, a print from a negative for example, without either exposing the silver haloid to light, or to the action of hypophosphite, or subjecting it to any treatment whatever, between the moment of its formation and that of its development. The film of silver haloid comes into existence with the image already impressed upon it.—The theory of the wind vane, by George E. Curtis. The author's theoretical studies lead to the inference that the oscillations of both spread and straight vanes are smaller as the vanes are longer and larger; that the spread is always more stable than the straight vane; and that this advantage in stability is greater for long than for short vanes, and is independent of the wind velocity.—On the manner of deposit of the glacial drift, by O. P. Hay. The author's studies of this great geological problem lead to the following conclusions: (1) an ice-sheet moving over a nearly level surface would possess far less abrading power than it would have while descending at a higher angle; (2) through subsidence of the glacial mass by the earth's heat and other causes a constantly increasing proportion of inert matter would collect in the lower-layers of the moving ice; (3) this accumulated material would tend to retard and finally arrest the motion of the lower portions of the glacier, and a permanent deposit would then be gradually made; (4) other detritus might accumulate at the foot of the glacier as a terminal moraine, and still other masses on the top of the already formed deposit when the glacier finally melted.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 5.—M. Hervé Mangon in the chair.—Photochronography applied to the dynamic problem of the flight of birds, by M. Marey. Having in a previous note shown that the kinematics of flight may be completely illustrated by photochronography, the author here proves that the same process contains all the elements necessary for solving the dynamic problem of flight; that is to say, for measuring the muscular forces and the work performed by the bird. Here is applied the mechanical principle that, if the mass of a body and the movements animating it be known, it is possible to deduce the value of the forces by which those movements are produced. On the photochronograph are measured all the displacements of the mass of the bird on the wing, together with the velocities of these movements. On the other hand the weight, that is, one of the forces to which the mass is submitted, is also known, while the resistance of the air, another of these forces, may be determined experimentally. Consequently the unknown quantity to be eliminated will be the muscular force of the bird with its momentum of action, and the value of its two components, one acting vertically against the weight, the other horizontally against the inert resistance of the mass and of the air. In these experiments the displacements of the bird are successively measured according to these two vertical and horizontal elements.—Measurement of luminous sensations in function of the quantities of light, by M. Ph. Breton. Since the invention of Bouguer's photometer it is known that if a dull white surface be disposed in contiguous zones receiving equi-different quantities of light, the perceptible contrasts between such zones are very far from being equal. To explain this phenomenon it has been suggested that the eye perceives the *relation* between two contiguous lighted surfaces. But the law (attributed to Fechner and Weber) based on this assumption—to the effect that, if several contiguous luminous surfaces are in geometrical progression, the sensations of the contrasts are equal—is shown to be incorrect by the experiment here described.—Observations of Brooks's new comet, made at the Observatory of Algiers with the 0·50-metre telescope, by MM. Trépied, Rambaud, and Sy.—Observations of the same comet made at the Observatory of Lyons with the 6-inch Brunner equatorial, by M. Le Cadet. The positions of this comet for August 29 and 30 and September 1 are also given from measurements taken by M. Gruey at the Observatory of Besançon. Its brightness is that of a star of the tenth magnitude.—Differential formulas for the variation of the elements of an orbit, by M. R. Radau. To correct a provisional system of elements it is often preferable to have recourse to the equations supplied by the ephemerides, rather than repeat the direct calculation of the elements. But the method is somewhat laborious, as the equations generally include six unknown quantities. The author, however, here shows that it is possible to give them a form in which the number of unknown quantities will be diminished without causing any complication in the calculation of the coefficients.—Note on M. Bertrand's problem, by M. Désiré André. A direct solution is given of this problem, followed by some remarks by M. Bertrand himself, pointing out its application to the question of chances in games of hazard as treated by Huygens, Moivre, Laplace, Lagrange, and Ampère. He offers a fresh solution of the problem: if a player stake the *n*th part of his fortune and continue the game indefinitely, what is the probability of his being ruined within a given number of rounds?

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Revised Currency System: H. Bull (Hamilton).—Laws and Definitions connected with Chemistry and Heat: R. G. Durrant (Rivingtons).—Educational Ends: S. Bryant (Longmans).—*Challenger* Report, Zoology, vol. xx. (Eyre and Spottiswoode).—A Short Introduction to the Study of Logic: L. Johnstone (Longmans).—The Instability of Gold as a Standard of Value: H. Bull (Hamilton).—The Eruption of Tarawera, N.Z.: S. P. Smith (Wellington).—The Icerya or Fluted Scale (Washington).—U.S. Department of Agriculture, Division of Entomology, Bulletins No. 13 and 14 (Washington).—Kryptogamen-Flora von Schlesien, iii. Band, 3 Liefg. (Kern, Breslau).—Beiblätter zu den Annalen der Physik und Chemie, 1887, No. 8 (Barth, Leipzig).

CONTENTS. PAGE

THURSDAY, SEPTEMBER 22, 1887.

TWO RECENT WORKS ON MICROSCOPICAL TECHNOLOGY.

Elementary Microscopical Technology. Part I. The Technical History of a Slide, from the Crude Materials to the Finished Mount. By Frank L. James, Ph.D., M.D. (St. Louis: *Medical and Surgical Journal* Company, 1887.)

A Course of Elementary Practical Histology. By W. Fearnley. (London: Macmillan and Co., 1887.)

THE above are the most recent of the numerous works now before the public on this somewhat restricted subject. The title of the second-named volume is somewhat of a misnomer, as the author deals with pure technology. Both works are for the most part expositions of those well-tried methods which now constitute the basis of the study; as such they differ, as might be expected, but little from their predecessors, except in matters of detail, in methods of treatment, and in literary style: but while this is the case, each volume has, nevertheless, a marked individuality.

Both works contain well-chosen woodcuts, illustrative of the leading apparatus described.

The first manual is a pleasing volume of 106 pages, being the first of a series which the author has found it necessary to prepare for the especial use of his own students. He has aimed at producing a work "in which nothing should be taken for granted, no previous acquaintance, on the part of the student, with the subject-matter presupposed," and we are pleased to admit that he has succeeded in his endeavour. The volume embraces a certain amount of botanical as well as zoological technique, and the most striking feature of it, apart from its general novelty, is the manner in which the several subjects are introduced. Thus, on p. 25, for example, we find the microtome first defined as "a receptacle for holding the material to be cut, a screw or other apparatus for feeding it to the knife, and a razor or knife with a very keen edge." The book is subdivided into fourteen chapters, each abounding in sound sense, and the product of great labour. Easy reading such as this is hard writing, and the author shows throughout a keen appreciation of the precise difficulties which beset a beginner. He guards against laborious idleness (that pitfall of the histologist) by giving the *rationale* of most of the complicated processes which he adopts; and his work leaves on the mind the impression that the various methods are "to be learned from experience and practice only, aided by the experience of others in similar cases," and that "experience, after all, is the great teacher, and the knowledge that is to guide one in doubtful cases is rarely to be obtained from text-books and manuals, no matter how elaborate, practical, and complete they be. They can only point the way, but individual experiment alone can make the successful worker." We shall look with interest for the continuation of this excellent work.

The last-named work is the most recent of the "Manuals for Students," which are so familiar in our laboratories and class-rooms; and it is, in many respects, a most remarkable book. There are in all 360 pages, and the author subdivides the whole into two parts, with an appendix. The first part is devoted to a consideration of apparatus and methods; it contains all that is customary and much that is useful, and it is by no means destitute of originality. The author has set down his experiences in a conscientious and painstaking manner; he states that his work "is intended as much for junior practitioners working in a private laboratory of their own as for medical students so called;" and from a declaration in his preface it is clear that he writes as a private medical man for medical men. Although there is a want of that system to which we are accustomed in text-books by recognized teachers, the book may be useful in extending beyond the usual boundaries the kind of work now universally imparted in our leading schools. It gives evidence of a large amount of honest labour, and there is incorporated within it much sound advice, notably that concerning the choice of a microscope; and it will be no fault of the author's if the student should go astray in the use of a high power. In discussing the immersion-lens an unnecessarily long disquisition is given upon the history and optics of the subject. The remarks offered might be advantageously condensed and re-placed in a footnote, giving references to the authorities cited; whereas, on the other hand, descriptions such as those given of the sub-stage condenser (p. 5) and of the camera lucida (pp. 24–25) are wholly insufficient, when it is considered that the author professes to write "for those who know little or nothing of the instrument." We see no reason why 1 per cent. solution of silver nitrate in distilled water need be placed in a bottle surrounded by black paper.

The second part is unique in construction, since it consists, in the main, of 145 pages of thick paper for the most part nearly blank. It is devoted to the enumeration of title-heads of those objects which the student is directed to examine, together with scant directions for so doing. By far the greater part, however, is given up to a muster of technical words, which under the head of "definitions of terms," the author would presumably have the student fill in, in school-boy fashion, for committal to memory. Novelty this, unexpected but unwarrantable, as it leads to a waste of valuable time and good paper. In giving directions for drawing under the microscope, the author advises (p. 28) that the typical parts be filled in—after getting from the text-book "all information about these." We have here something akin to an inversion of the order of procedure which experience and common-sense alike dictate. Whatever may be the success of this volume, it will remain memorable for its striking originality of style. To begin with, the inventor of a method or of a reagent is exalted to the dignity of a discoverer; and, to proceed, we read (pp. 10–11) that "if the student hesitates as to choice [of dry lenses recommended] he had better adopt the street urchin's mode of settling the matter by tossing up one of the coins of the realm"; while, by way of a novelty, we are informed (p. 61) that "this almost universal desideratum of the physiologist [anæsthetizing] is carefully concealed by professional anti-vivisectionists who obtain their livelihood by harrowing the feelings of the public." Perusal of almost every page of this very remarkable book furnishes similar eccentricities; but it must be remembered that it is intended "for students of all denominations who can command the means and have the wish to construct for themselves a histological cabinet," and that it has been produced "between the numerous and unavoidable interruptions of a family medical practice." G. B. H.

OUR BOOK SHELF.

Precious Stones in Nature, Art, and Literature. By S. M. Burnham. (Boston: Bradlee Whidden. London: Trübner and Co.)

MR. BURNHAM is the author of a work on limestones and marbles published a few years ago in which he indicated the resources of the United States and other countries in stone for decorative purposes. In the present volume he treats of precious stones in that exhaustive and thorough fashion which we are accustomed to regard as a special characteristic of German writers. He begins by describing, as far as is known, the origin, properties, classification, localities, imitations, and antiquity, of precious stones (antiquity here applies of course to their use as ornaments), and then proceeds to treat of their prices, the trade in them, the sumptuary laws relating to them, those of remarkable size, and notorious jewel robberies. This chapter is followed by a description of various notable collections, and of the Crown jewels of different countries, from which the author passes on to some very interesting chapters on the secular uses of precious stones, the different kinds of ornaments, and their sacred uses. A chapter on precious stones in literature, and their mystical properties, is succeeded by one on the curious art of engraving on precious stones, and then commences a series of chapters on the various stones. First, of course, comes an account of the diamond, its home, and of historical and remarkable diamonds, which is followed by descriptions of all the precious stones at present known, from the sapphire, emerald, and ruby, to coral, amber, jet, cat's-eye, and rock-crystal, to the number of about one hundred. The appendices give the sizes of large and remarkable diamonds, a classification of precious stones according to their principal constituents, the hardness and specific gravity of precious stones, their relative hardness, relative specific gravity, and finally a list of the localities in the United States in which gem-minerals have been found. It will be perceived from this very brief indication of the contents of the book that the work is perfectly encyclopædic in its treatment of its subject; nothing relating to precious stones is strange to or disregarded by Mr. Burnham. Of the value of the book to the gem collector, expert, or mineralogist, it is needless to speak, but we can answer for it that it is highly interesting to the general reader, or at least to all who like to hear about those rare and beautiful products of Nature to which man in all ages and in every country has attached a high value.

Hydrophobia: An Account of M. Pasteur's System. By Renaud Suzor. (London: Chatto and Windus, 1887.)

DR. RENAUD SUZOR is the delegate commissioned by the Government of the colony of Mauritius to come to Europe to study M. Pasteur's treatment of hydrophobia, and this volume is the result of his mission. It is greatly to the credit of Sir John Pope Hennessy, the Governor of that colony, and of the members of the Legislative Council, that they perceived the value to science and humanity of adequately studying M. Pasteur's recent discoveries on the subject of hydrophobia, and that they "unanimously voted" the appointment of a delegate to proceed to Paris to work under the distinguished discoverer. It is to be hoped that other and more prominent colonies may be led to follow this excellent example. This little volume amply justifies the selection of Dr. Suzor as delegate. It opens with an historical account of hydrophobia and its treatment from the earliest times—for this dreadful disease has been known and studied for more than 2000 years—down to the end of 1880. The second and principal part of the volume is occupied by translations of all M. Pasteur's communications on the subject to the Academy of Sciences, beginning with his first note in January 1881, and ending with a lengthy paper presented in November 1886. Finally, there is a description of the technique of M. Pasteur's method. The book is valuable as a clear and comparatively untechnical exposition of the Pasteur method; but it is still more valuable as an example of the manner in which Pasteur's wonderful discovery should be met and treated by Governments and others in authority, who are responsible for the prevention, as far as possible, of disease amongst the populations which they govern. The Governor of Mauritius has taken care that the neglect of this primary duty, in relation at least to hydrophobia, cannot be laid to his charge.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

A Monstrous Foxglove.

A SOLITARY specimen of *Digitalis purpurea* was found last month in a damp wood near Old Colwyn, North Wales, which exhibited the following curious abnormalities in the structure of its flowers. In only one out of the six opened flowers of the raceme was the calyx normal (*i.e.* consisting of four broad and one narrow segment); in all the others it was divided almost to the base into five equal linear segments. The corolla in four out of the six flowers consisted of merely two narrow petals with long claws, placed at opposite points on the receptacle; in one flower these two distinct floral leaves were deeply divided into two and three lobes respectively, thus forming a perfect two-lipped flower, the lips, however, being quite separate from one another. In the only other flower the upper lip was altogether wanting, the three-lobed lower one alone being present, upon which, alternating with its lobes, were inserted one long and one short stamen. This was the only flower which possessed stamens.

The form and number of the styles also was variable and abnormal. In one flower only was the usual single shortly two-cleft style met with; two other flowers possessed each a single style forked below the middle; in two others there were two, and in the remaining flower three styles, all separate and similar.

The same abnormalities were seen in the corolla and styles of two unopened buds.

I should be happy to learn if such monstrous forms are at all usual in the foxglove. F. R. TENNANT.

Longport, Staff.

The Law of Error.

DR. VENN, in a letter published in NATURE, September 1 (p. 411), adduces certain meteorological statistics which do not conform to the typical law of error or probability-curve. To discover the cause of this failure there would be required both a special knowledge of the subject-matter and the general conceptions which the calculus of probabilities supplies. The latter qualification is the only one to which the present writer can make any pretension.

The essential condition of the typical law being fulfilled is that each observation or statistical return should be made up of a great number of independent variable items. A good example is afforded by taking a great number, *e.g.* 100, digits at random from mathematical tables. The sums of that number of digits will fluctuate about the mean 450 according to a probability-curve whose "probable error" is about 19.

(1) One explanation of the failure of the law is that the requisite plurality of items is wanting. Suppose we had taken sums of *two* (instead of a hundred) digits, the grouping of these sums would be best represented by a right line, or rather two right lines. If we took three digits at a time, the resulting form would be parabolic. A variant of this class of exception is when the larger items are few or unique, while items of an inferior order congregate in great numbers. Suppose each observation to consist *either* of the digits 3 or 6, *plus* ten items taken at random from the series ·1, ·2, ... ·9. There would then be generated a curve like those in Dr. Venn's Fig. 2. If, instead of 3 and 6, we had two digits, 4 and 5, differing by very little from each other, the abnormal uniqueness of the larger items would be disguised. It is upon this principle, doubtless, that the population of a kingdom appears to conform (in respect of height or other attribute) to the law of error, while at the same time each province may present a distinct type. Suppose that the majority of our returns were, as the last-mentioned case, *either* 4 or 5 *plus* an aggregate of smaller items; but that a small proportion of the returns were governed by a widely disparate "large item," *e.g.* 8 or 9; in this case we might have the appearance presented by Dr. Venn's Fig. 1. The body of the curve would seem to be of the probability family; but there would be tacked on a tail appertaining to a different type. Dr. Charles Roberts has adduced some statistics of this species in a paper published in the *Medical Times*, February 7, 1885.

(2) We have hitherto supposed that the constituent items have no bias in one direction. Suppose, however, that instead of the digits 1, 2, ... 8, 9 being each equally eligible, 8 and 9 became inadmissible; and, whenever one of those digits was presented, we had to substitute 6 and 7 respectively. There would thus be two chances in favour of 6 and also of 7. An aggregate of 100 digits each selected according to this unsymmetrical scheme would be grouped about the mean value $10 \times (1 + 2 + 3 + 4 + 5 + 2 \times 6 + 2 \times 7)$, or 410, in a form which as to the body of the curve would be a probability-curve, but which would be unsymmetrical at the extremities. The most familiar example of this case is afforded by games of chance. If black and white balls, in an unequal proportion and immense numbers, are mixed up, then if you take at random batches of 100 (or 1000) balls the percentage of white or black balls will fluctuate in the manner described. It is quite possible that this principle should govern what Dr. Venn calls a "one-ended phenomenon," *i.e.* one in which unlimited variation is conceivable in one direction but not in the other. Dr. Venn's Fig. 1 seems fairly well to represent a biased probability-curve.

(3) We have hitherto supposed that the individual observation or return is the *sum* of the variable elements. But it may be a more complicated function. Thus it may be a product. The *logarithm* of the observations may fluctuate according to a probability-curve, while the observations themselves obey a law which has been investigated by Dr. Macalister in the Proceedings of the Philosophical Society (1879); related to the geometrical mean just as the probability-curve is to the arithmetic mean. This grouping is to be expected wherever the analogies of *Fechner's law* prevail. This may be the rationale of the fact which I have elsewhere pointed out, that fluctuations of price rise much higher above, than they fall below, the mean. But, where the principle of estimation does not come in, it is not quite clear why the geometrical curve should be more appropriate to a "one-ended phenomenon" than the biased probability-curve which has been described under our heading (2). At any rate, in the case before us, Dr. Venn's Fig. 1, the numerical statistics which he has allowed me to inspect show much too close a correspondence between the body of the figure and the probability-curve to admit of the geometrical explanation. There is also this peculiar difficulty, that the longer limb of the given curve is the lower one.

F. Y. EDGEWORTH.

King's College, London.

A Null Method in Electro-calorimetry.

BY reference to the last number of the *Electrical Review* (vol. xxi. p. 262), wherein is printed a short abstract of our paper on "A Null Method of Electro-calorimetry" read before the British Association on September 1, Mr. Huntly will find that the method of measuring specific heats suggested by him is in principle similar to that described by Mr. Gee and myself. The method has been employed for determining specific heats during the last two years, but we have delayed publication till the best working details of the method have been elaborated.

In certain practical details our method differs from Mr. Huntly's suggestion. The mass of liquid in each calorimeter is *not* the same. It is much preferable to have the masses inversely proportional to the specific heats, so that the thermal capacities of the liquids are equal. In this way it will be readily understood that the correction for radiation can be made to disappear altogether. For since the calorimeters are precisely equal, and their temperatures equal, the loss of heat by radiation must be the same from each; further, since the thermal capacities of the liquids are the same, as well as that of vessels and stirrers, it follows from the equality of the resistances that the same current will produce the same rise in temperature in each case, and conversely, since the heat radiated from each calorimeter is the same, and since the thermal capacities of the calorimeters and stirrers are equal, it follows that, if the same current traversing the equal resistances produces the same rise in temperature in each liquid, the thermal capacities of the two liquids are the same, whence the specific heat can at once be determined by determining the masses of the liquids. Virtually, then, the null method of obtaining the same rise of temperature in each calorimeter is attained by varying the mass of liquid in either or both calorimeters. In practice we approximate as nearly as possible to the condition by adding liquid to that calorimeter which rises in temperature most quickly, and then make a final adjustment by shunting a *very small fraction* of the current by means of the high resistance in the box. This is, we believe, the first time that a method for measuring specific heats has been published in which the correction for radiation and for the thermal capacity of calorimeters and stirrers has been entirely eliminated.

With the first apparatus we had made to embody these ideas, viz. that described in the *Electrical Review* (*loc. cit.*), an accuracy of at least one-tenth per cent. could be obtained from a single experiment, thoroughly confirming Mr. Huntly's anticipations as to the delicacy of the method. We have just introduced some considerable improvements in the apparatus which we hope will enable us to insure much greater accuracy than that hitherto obtained.

A few words are required in reply to some observations of Mr. Huntly. First, he suggests a bolometric method of determining the difference of temperature. We have so far preferred a thermo-electric method, which, without a specially constructed galvanometer, enables us to detect with certainty 1/2000 of a degree; the necessary corresponding variation in the resistance of a Pt wire would only be 1·6 parts in a million; besides some difficulties may arise in procuring two pieces of Pt wire which shall have the same temperature coefficient to 1 part in a million, even if they be cut from the same piece originally. Secondly, the time method described by Mr. Huntly at the end of his paper seems to me to have a fatal objection: it would be quite impossible to keep the current constant for a long time to the 1/2000 part which would be requisite to secure such accuracy as we can get with present arrangements.

WILLIAM STROUD.

Mental Development in Children.

I SHOULD like to hear the opinion of psychologists on the following circumstance:—A female child, quick and intelligent, when about fifteen months old, learned to repeat the alphabet, shortly afterwards the numerals, days of the week, month, &c., and, subsequently, scraps of nursery rhymes, English and German; then to spell words of two and three letters. All this was learned readily, eagerly indeed, and for a time she remembered apparently every word acquired, indelibly. At about two years old further teaching was for a time remitted, as she was observed to be repeating audibly in her sleep what she had learned during the day. Subsequently, tuition was resumed, under a governess, but she had not only forgotten much of what she had previously known perfectly, but learns far less readily than formerly. She is now about three and a half years old, in perfectly good health and spirits, quick, and particularly observant, but the capacity for learning by rote is materially diminished; she is remarkably imitative, but shows no faculty whatever for writing, and as little for music.

I should like to hear of any parallel cases, and what the ultimate development has been; with any opinions upon the cause of their appearances.

M A.

September 18.

FIFTY YEARS' PROGRESS IN CLOCKS AND WATCHES.[1]

II.

TO pass on to another phase of mechanical improvement, a wonderful advance in the mechanism of chronographic watch-work has been made during the period we refer to. In this department the first chronographs to be introduced were those having a kind of double hand, the lower portion of which carried a tiny vessel of ink. When an observation was requisite, the upper part of the hand passed through a small orifice in this ink-vessel, marking a dot upon the dial below. We have had of late years, however, much cleaner and more convenient arrangements. The most usual form is as follows. In addition to the ordinary minute and *centre*-seconds hands there are auxiliary hands, which always stand at zero when not moving. Pressure on the crown-piece sets them going, a second pressure stops them, and the third pressure sends them back to zero; and it is interesting to observe that they always return to zero—their normal position—the shortest way round the dial. The nature of the mechanism by which these operations are effected is briefly as follows. Pressure on the crown-piece causes a wheel carrying different sets of cams to advance step by step. These cams, which correspond to the starting, stopping, and returning of the hands, operate on springs and levers. The first motion frees the auxiliary hands, and also throws them into gear with the watch-train. The second motion throws them out of gear and clutches them so that they shall not shift. The third motion sends them back to zero, and this is effected in the case of both by what is called a heart-piece. This heart-piece, as regards the seconds-hand, is shown in outline at the centre of Fig. 8,[1] which has already appeared as Fig. 5 in the first article. It is to be mentioned

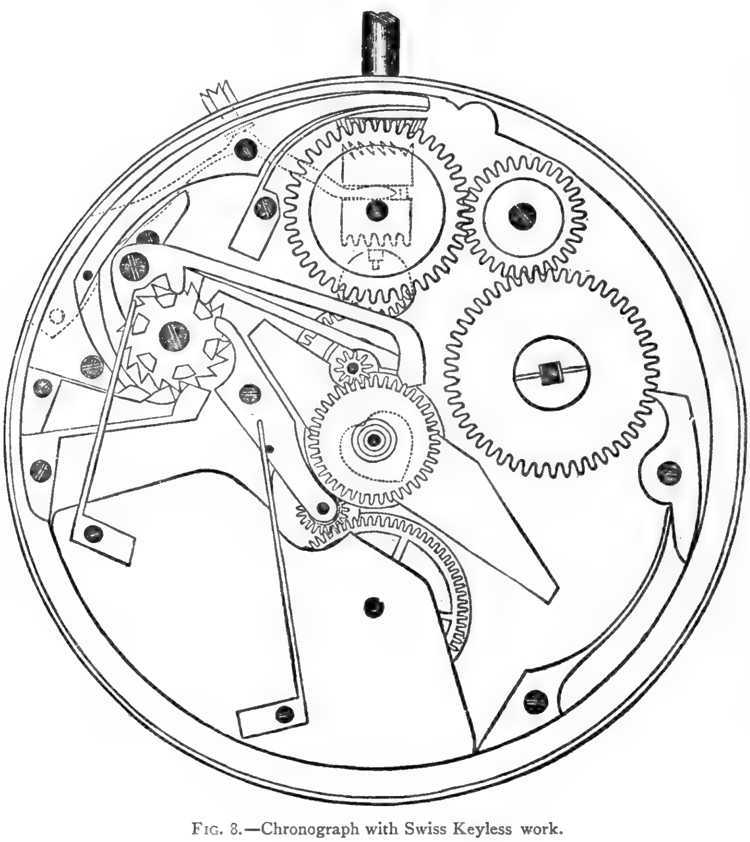


Fig. 8.—Chronograph with Swiss Keyless work.

that the heart-pieces go round with their respective hands. The third pressure releases the clutches and also causes the lever, shown above the heart, to descend upon it; the heart and hand being now free to move, the lever draws round the heart until it finds the lowest position of it, which, as is natural, is arranged to correspond to the normal position of the hand. The gearing-wheels and clutch-levers can be very well seen in Fig. 9.

In another form of chronograph a long seconds-hand is superimposed over another so as to appear as one with it. They both travel with the watch-train; until a first pressure stops one, and a second pressure the other; the interval between the two pressures can now be read off at leisure. A third pressure sends them flying to the place where they would have been if they had not been stopped at all, even should they have been kept standing for a week. They, also, always return the shortest way. The mechanical arrangements consist of a heart-piece for bringing the hands together, and they achieve the position where they would have been had they not been stopped by means of a kind of cylinder sliced through at an angle not perpendicular to its axis. Whilst the hands are travelling, the faces where the cylinder is cut are kept pressed together by springs, but they are parted when the hands are brought to rest. One half of the cylinder goes on with the watch-train, the other half (in connexion with the hands) remains suspended above it; at the third pressure of the crown-piece the top half is permitted to descend, when it naturally seeks its former position with

[1] Continued from p. 395.

[1] We are indebted to Mr. Britten for the use of Figs. 5, 9, 10, 11, 12, and 15, and to Mr. Glasgow and the Messrs. Cassell for Figs. 13 and 14. Readers who wish further insight into the details of our subject should consult both Mr. Britten's and Mr. Glasgow's books.

respect to the other, which has been permitted to go on with the watch-train.

The form of mechanism which is applied for the purpose of maintaining and showing a calendar has undergone considerable development. Calendars are now made to be perpetual, correcting themselves for everything, including leap-year. The following (Fig. 10) is the plan generally adopted in clocks, and is the invention of the late M. Brocot. M *m* is a lever which is worked by a pin, *e*, in a wheel in the clock-train going round every twenty-four hours. As *e* advances in the direction indicated by the arrow, M *m* is moved to the left, and the clicks G and H, which it carries, pass over the top of a single tooth each of the wheels A and B, the wheels being meanwhile held loosely in position by weak springs called "jumpers." As soon as *e* has passed the end of M *m*, the latter falls back by its own weight, dragging back A and propelling B each one tooth respectively. B has seven teeth, and works the days of the week; A has thirty-one, and serves for the days of the month. All months, however, have not thirty-one days, and provision is made for the difference by a supplementary thruster, N. In A there is a pin, *i*, which comes regularly below N every twenty-eighth day. The tail of N rests against a wheel, V F, which goes round once in four years. V F is graduated with notches of different depths. These notches correspond to the respective lengths of each month, and those representing the Februaries are conspicuously noticeable; that one which is the shallowest of the four identifying the leap-year. During the months of normal length, N maintains the position which is shown dotted in the diagram, and does nothing. But whenever there occurs a short month, the tail of N will enter one of the notches; in consequence, N will descend, and, engaging *i*, propel A forward a day or more, depending upon the depth of the notch. This happens whilst M *m*

FIG. 9.—Chronographic watch-work.

is travelling to the left. When M *m* falls back, the click G will act in addition, and as usual. Fig. 11 shows the dial; the hands on dials right and left are in connexion with A and B (Fig. 10). The hand upon the lowest dial shows the month of year; its progression is continuous. The hand at the top shows the equation of time, and alternates on each side of noon, + or −, as may be required. It is worked by a rack which reposes against a cam of suitable form revolving once in twelve months.

The phases of the moon are indicated (as may be seen in the diagram) by the passage of three shaded disks across a circular aperture.

Magnetism exercises the most destructive influence upon watches or chronometers, turning their balances into compass-needles, and causing the coils of their balance-springs to stick together.

In these days of large magnetic engines it has therefore been found necessary to revert to an idea of the elder Arnold, and to construct watches for the use of those having to do with such engines upon a plan which shall render them indifferent to magnetization. This result is obtained by making the balances of silver and platinum, or an alloy of iridium, or of some other non-magnetizable material, and the balance-springs of gold or palladium; and the use of steel is avoided in certain parts of the escapement. Watches carefully constructed on such plans give results little inferior, as regards time-keeping, to others.

Amongst the multifarious purposes to which clocks have recently been applied, we must not omit to mention those which are designed for registering the proper performance of a watchman's duty. The old-fashioned principle was that there should be a separate clock at each station the watchman had to visit, and by pulling a string or handle he was enabled to leave record of his presence. Now, either he carries the clock (or large watch) with him, or else it is fixed at a central station, and is operated upon by electricity. In the first case there is a revolving paper dial inside the clock, and by placing the clock within specially arranged orifices at the different stations he has to visit, he is enabled to get printed off upon the paper dial a mark or letter showing the time at which he was at the station. In the latter case the clock is provided with a large drum or cylinder, and wires lead to it from the different stations; and when a button at any station is touched, a mark follows upon the cylinder, indicating the where and when of the person who made it. At one large lunatic asylum the system is so perfect that the night superintendent, sitting in his own room, can follow the movements and whereabouts of all his men. Clocks have also been designed for registering the gross aggregate or integral of daily temperatures or barometric pressures. In the former case a watch is used, and has a balance compensated the wrong way, so that the effects of changes of temperature are magnified. In the latter case a barometer is used as the pendulum.

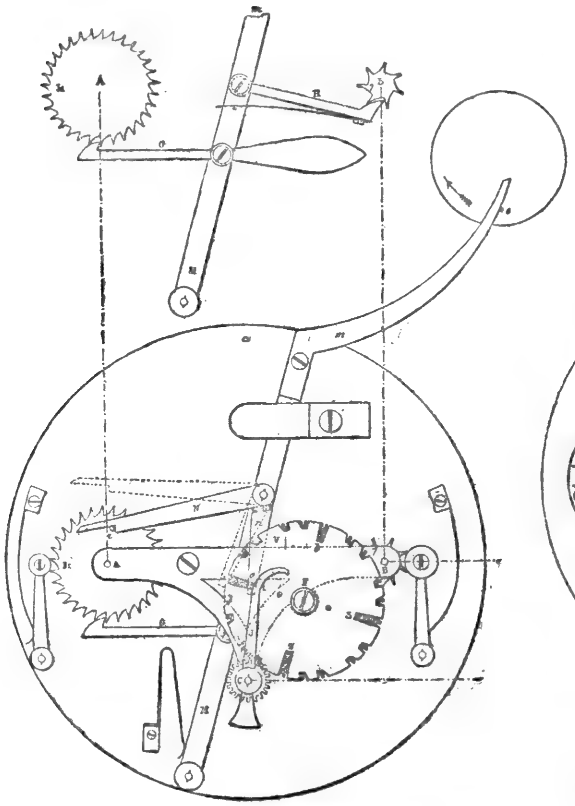


Fig. 10.—Brocot's Perpetual Calendar.

Until three years ago there was no public institution in Great Britain where a serviceable authentic trial of the performance of a watch under varying conditions as regards temperature and changes of position could be obtained. At that date, however, under the auspices of the Royal Society, a department of the Kew Observatory was established for the purpose. It satisfied a want which had long been felt, and provided with every requisite for timing both in temperatures and positions, a considerable and increasing number of watches are regularly sent there for the purpose of obtaining its certificates. In Class A (the first class) merit-marks are awarded in addition to the certificates, in the following proportions: 40 for a complete absence of variation of daily rate, 40 for absolute freedom from change of rate with change of position, and 20 for perfect compensation for effects of temperature.

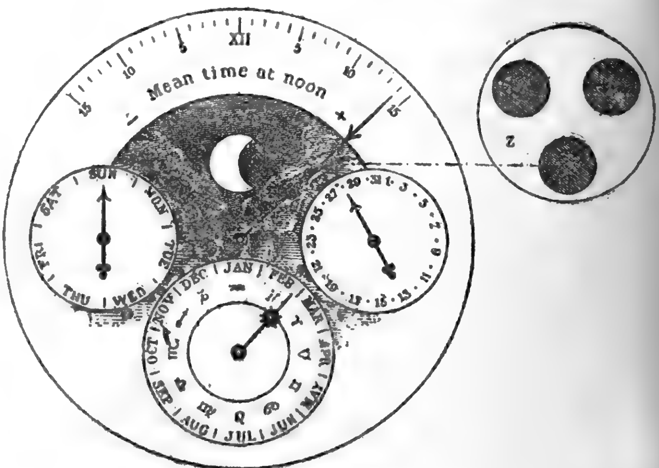


Fig. 11.—Dial of Brocot's Perpetual Calendar.

The subject of the application of the balance-spring, and the process of timing, which is subsequent, must be

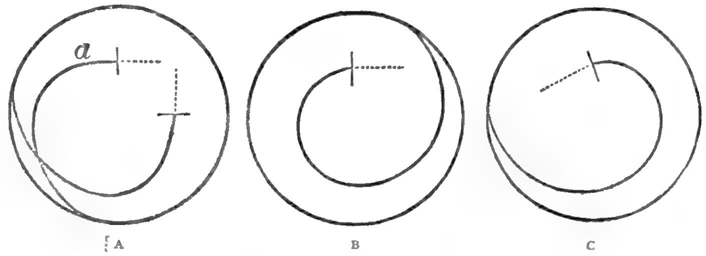


Fig. 12.—Diverse forms to which balance-springs are fashioned.

reckoned beyond the scope of the present article. But we may briefly allude to the fact that the causes which operate upon a watch to make it keep different times in different positions are generally three. For instance, the balance may be out of poise, the balance-spring may not be isochronous, and the action of the escapement generally is irregular in different positions. Putting the balance in poise is done roughly in a poising tool; the finer adjustment follows the results of trials when the watch is kept going in different positions. Isochronism is more important and more difficult of attainment. Without isochronism a watch might keep tolerably near time when placed successively 12, 3, 6, or 9 upwards, and still possess a very wide error between all these and the dial up (flat) position. Want of isochronism would also cause it to vary its rate considerably as time went on. Isochronism is obtained by a careful adjustment of the weight of the balance to the motive power; and by suiting the length, number of coils, and forms of the curves at the terminations of the balance-spring to circumstances, as may be required. For example, A, Fig. 12, shows the contour of the curves which terminate the spring of a marine chronometer; B and C, the contours of a pocket chronometer spring. It must not be supposed that all marine or pocket chronometer springs are alike. The correct form is generally arrived at after prolonged trial and patient fashioning.

Technical education has not been neglected in recent years by English watch-makers. Indeed, the necessity

Fig. 13.—Loseby's Balance.

for it has been too keenly felt to allow them to forget it. But for a long time there was nobody to help or even to advise them. Under such conditions a small party took the matter into their own hands, and founded the Horological Institute. With very little encouragement they at first worked on, but have now the satisfaction of seeing their efforts successful to an extent which they could have scarcely anticipated. Workshops, science and drawing classes are to be found at the Institution; and examinations, under the auspices of the City and Guilds of London Institute, are periodically conducted, and certificates of proficiency granted.

Before concluding we give two diagrams which may be of interest. They refer to the subject of secondary compensation, one of them, Fig. 13 (Loseby's), representing

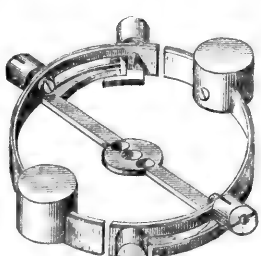


Fig. 14.—Kullberg's Balance.

one of the oldest, and the other, Fig. 14 (Kullberg's), one of the most recent forms of balance for the purpose. It will be seen that Loseby's object was effected by means of curved mercurial thermometers—the lower the temperature the more indirectly the mercury receded from the centre, checking the action of the compensation: with Kullberg's the supplementary compensation screws are checked directly.

There have been many improvements in the lever escapement. Fig. 15 shows one of the most remarkable. In this case the discharging is effected by means of two pins in the roller, and the impulse given by means of a pin in the lever working into the notch on the roller. The effect is that the unlocking takes place at about the line of centres, and the impulse is given more advantageously. Resilient escapements are those which will enable the watch-balance to make several turns in the same direction without injury to the escapement. They often save a breakage in the case of a blow or jerk; their invention is due to Mr. Cole. We ought not to close this article without mentioning the fact that the manufacture of the

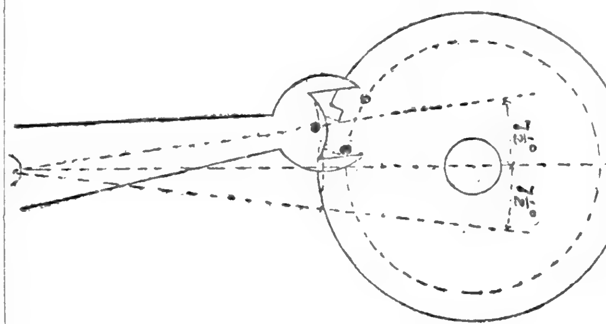


Fig. 15.—Savage's Two-pin Lever Escapement.

duplex escapement, which was at one time reckoned the very first, has been completely abandoned. Besides its liability to stop, it was found that the wear in the pivot-holes made its timing and adjustment exceedingly precarious.

HENRY DENT GARDNER.

THE BRITISH ASSOCIATION.

SECTION F.

ECONOMIC SCIENCE AND STATISTICS.

OPENING ADDRESS BY ROBERT GIFFEN, LL.D., V.P.S.S., PRESIDENT OF THE SECTION.

The Recent Rate of Material Progress in England.

IN coming before you on this occasion it has occurred to me that a suitable topic in the commercial capital of England, and at a time when there are many reasons for looking around us and taking stock of what is going on in the industrial world, will be whether there has been in recent years a change in the rate of material progress in the country as compared with the period just before. Some such question is constantly being put by individuals with regard to their own business. It is often put in political discussions as regards the country generally, with some vague idea among politicians that prosperity and adversity, good harvests and bad, in the most general sense, depend on politics. And it must always be of perennial interest. Of late years it has become specially interesting, and it still is so, because many contend that not only are we not progressing, but that we are absolutely going back in the world, while there are evident signs that it is not so easy to read in the usual statistics the evidence of undoubted growth as it was just before 1870–73. The general idea, in my mind, I have to add, is not quite new. I gave a hint of it in Staffordshire last winter, and privately I have done something to propagate it so as to lead people to think on what is really a most important subject. What I propose now to do is to discuss the topic formally and fully, and claim the widest attention for it that I possibly can.

There is much *primâ facie* evidence, then, to begin with, that the rate of the accumulation of wealth and the rate of increase of material prosperity may not have been so great of late years, say during the last ten years, as in the twenty or thirty years just before that. Our fair-trade friends have all along made a tactical mistake in their arguments. What they have attempted to prove is that England lately has not been prosperous at all, that we have been going backwards instead of advancing, and so on; statements which the simplest appeal to statistics was sufficient to disprove. But if they had been more moderate in their contentions, and limited themselves to showing that the rate of advance, though there was still advance, was different from and less than what it was, I for one should have been prepared to admit that there was a good deal of statistical evidence which seemed to point to that conclusion, as soon as a sufficient interval had elapsed to show that the statistics themselves could not be misinterpreted. There has now been ample time to allow for minor variations and fluctuations, and the statistics can be fairly construed.

I have to begin by introducing a short table dealing with some of the principal statistical facts which are usually appealed to as signs of general progress and the reverse, and I propose to go over briefly the items in that table and to discuss along with them a few broad and notorious facts which cannot conveniently be put in the same form.

Statement as to Production or Consumption of Staple Articles in the United Kingdom in the undermentioned Years, with the Rate of Increase in Different Periods compared.

	1855.	1865.	1875.	1885.	Ratio of increase per cent.		
					1855–65.	1865–75.	1875–85.
Income-tax assessments, million £	308	396	571	631	28	44	10
Production of coal, million tons	64	98	132	159	53	35	20
,, pig iron, ,,	3·2	4·8	6·4	7·4	50	33	16
Receipts from railway goods traffic per head of population	—	11*s.*[1]	18*s.*[1]	21*s.* 2*d.*[1]	—	63	18
Clearances of shipping in foreign trade, million tons	10	15	24	32	50	60	33
Consumption of tea per head, lbs.	2·3	3·3	4·4	5·0	43	33	13½
,, sugar, ,, ,,	30·6	39·8	62·7	74·3	30	58	19

[1] These figures are for 1860–64, 1870–74, and 1880–84.

The first figures are those of the income-tax assessments. What we find is that if we go back thirty years and compare the amount of income-tax assessments in the United Kingdom at ten years' intervals, there appears to be an immense progress from 1855 to 1875, the first twenty years, and since 1875 a much less progress. The total amount of the assessments themselves, stated in millions, was as follows:—

	Millions.		Millions.
1855	£308	1875	£571
1865	396	1885	631

And the rate of growth in the ten-yearly periods which these figures show is—between 1855 and 1865, 28 per cent.; between 1865 and 1875, 44 per cent.; and between 1875 and 1885, 10 per cent. only.

Making all allowance for changes in the mode of assessment by which the lower limit of the tax has been raised, for the apparent increase before 1875, which may have been due to a gradual increase of the severity of the collection, and for the like disturbing influences, I believe there is no doubt that these income-tax assessments correspond fairly well to the change in the money value of income and property in the interval. How great the change in the rate of increase is, is shown by the simple consideration that if the rate of increase in the last ten years, instead of being 10 per cent. only, had been 44 per cent., as in the ten years just before, the total of the income-tax assessments in 1885, which is actually £631,000,000, would have been £882,000,000! Something then has clearly happened in the interval to change the rate of increase.

These figures being those of money values, an obvious explanation is suggested which would account in great part for the phenomenon of a diminished rate of increase in such values without supposing a reduction of the rate of increase of real wealth, of the things represented by the money values, to correspond. This is the fall of prices of which we have heard so much of late years, and about which in some form or another we shall no doubt hear something at our present meeting. It is quite clear that, if prices fall, then income-tax assessments must also be affected. The produce of a given area of land, for instance, sells for less than it would otherwise sell; there is less gross produce, and in proportion there is even less net produce, that is, less rent; consequently the net income appearing in the Income Tax Schedules is either less than it was or does not increase as it did before. The same with mines, with railways, and with all sorts of business under Schedule D. The things themselves may increase as they did before, but as the money values do not increase, but diminish, the income-tax assessments cannot swell at the former rate. It is the same with salaries and other incomes not dependent so directly in appearance on the fall in prices. Salaries and incomes are of course related to a given range of prices of commodities, and a fall in the prices of commodities implies that the range of salaries and incomes is itself lower than it would otherwise be, assuming the real relation between the commodities and incomes to be the same after the fall in prices as it would have been if there had been no fall in prices. Hence the income-tax assessments by themselves are not a perfectly good test in a question like the present. The change implied may be nominal only, so far as the aggregate wealth and prosperity of the community are concerned, though of course there can be no great and general fall of prices without a considerable redistribution of wealth, which must have many important consequences.

This criticism, however, does not apply to the remaining figures in the short table submitted, and to various other well-known facts, which we shall now proceed to discuss.

The production of coal, then, is found to have progressed in the last thirty years as the income-tax assessments have done. The figures in millions of tons at ten years' intervals are as follows:—

	Million tons.		Million tons.
1855	64	1875	132
1865	98	1885	159

And the rate of growth in the ten-yearly periods which these figures show is between 1855 and 1865, 53 per cent.; between 1865 and 1875, 35 per cent.; and between 1875 and 1885, 20 per cent. only. The rate of growth in the last ten years is much less than in the twenty years just before. The percentages here, it will be observed, are higher than in the case of the income-tax assessments. The increase in the last ten years in particular is 20 per cent. as compared with an increase of 10 per cent. only in the income-tax assessments. But the direction of the movement is in both cases the same.

I need hardly say, moreover, that coal production has usually been considered a good test of general prosperity. Coal is specially an instrumental article, the fuel of the machines by which our production is carried on. Whatever the explanation may be, we have now, therefore, to take account of the fact that the rate of increase of the production of coal has been less in the last ten years than in the twenty years just before.

Then with regard to pig-iron, which is also an instrumental article, the raw material of that iron which goes to the making of the machines of industry, the table shows the following particulars of production:—

	Million tons.		Million tons.
1855	3·2	1875	6·4
1865	4·8	1885	7·4

And the rate of growth which these figures show is between 1855 and 1865, 50 per cent.; between 1865 and 1875, 33 per cent.; and between 1875 and 1885, 16 per cent. only. Whatever the explanation may be, we have thus to take account of a diminution of the rate of increase in the production of pig-iron much resembling the diminution in the rate of increase of the production of coal.

At the same time the miscellaneous mineral production of the United Kingdom has mostly diminished absolutely. On this head, not to weary you with figures, I have not thought it necessary to insert anything in the above short table; but I may refer you to the tables put in by the Board of Trade before the Royal Commission on Trade Depression. Let me only state very briefly that while the average annual amount of copper produced from British ores amounted in 1855 to over 20,000 tons, in 1865 the amount was about 12,000 tons only, in 1875 under 5000 tons, and in 1885 under 3000 tons. As regards lead, again, while the production about 1855 was 65,000 tons, and in 1865 about 67,000 tons, the amount in 1875 had been reduced to 58,000 tons, and in 1885 to less than 40,000 tons. In white tin there is an improvement up to 1865, but no improvement since, and the only set-off, a very partial one, is in zinc, which rises steadily from about 3500 tons in 1858, the earliest date for which particulars are given, to about 10,000 tons in 1885, considerably higher figures having been touched in 1881–83. There is nothing, then, in these figures as to miscellaneous mineral production to mitigate the impression of the diminution in the rate of increase in the great staples, iron and coal, in recent years.

Agricultural production, it is also notorious, has been at any rate no better, or not much better, than stationary for some years past, although down to a comparatively recent period a steady improvement seemed to be going on. Making all allowance for the change in the character of the cultivation, by which the gross produce is diminished, although the net profit is not affected to the same extent, and which might be held to argue no real decline in the rate of general growth if the population, diverted from agriculture, were more profitably employed, yet the facts, broadly looked at, taken in connexion with the other facts stated as to diminished rate of increase in other leading industries, seem to confirm the supposition that there may have been some diminution in the rate of increase generally.

It is, unfortunately, impossible to state in a simple manner the progress at different dates in the great textile industries of the country. Everything as regards these industries is thrown out by the disturbance consequent on the American War. It does not appear, however, that what has happened as regards the main textile industries, cotton and wool, would alter sensibly the conclusions above stated, drawn from the facts as to other main industries of the country. If we take the consumption of raw materials as the test, it would appear that the growth in the cotton manufacture is from a consumption of 28 lbs. per head in 1855 to about 38 lbs. per head in 1875, while in 1885 the consumption is nearly 42 lbs. per head, an increase of 4 lbs. per head in the last ten years, against 10 lbs. per head in the previous twenty. The percentage of increase in the last twenty years must therefore, on the whole, have been less than in the previous twenty, although in these twenty years the great interruption due to the American Civil War occurred. Of course the amount of raw material consumed is not here an absolute test. There may be more spinning and weaving now in proportion to the same quantity of raw material than was formerly the case. But the indications are at least not so certain and direct as when the consumption of raw material could be confidently appealed to. As regards wool the comparison is unfortunately very incomplete owing to the defect of data for the earlier years; but what we find is that the amount of wool consumed per head of the population of the United Kingdom has in the last ten years rather declined than otherwise from nearly 11 lbs. per head in the five years 1870–74 to 10 lbs. per head only in the five years 1880–84. Here, again, the explanation suggested as to cotton—viz. that there may be more spinning and weaving now in proportion to the same quantity of raw material than was formerly the case—applies. But the answer is also the same, that at any rate the indications of progress are no longer as simple as they were. The reality of the former rate of advance is not so clearly manifest.

Of course I need hardly add that in the case of another great textile, silk, there has been no progress, but the reverse, for some years; that this is also true of linen; and that the increase in the allied manufacture, jute, can only be a partial set-off.

In the textiles, then, as in other staple industries of the country, the rate of advance in the last ten years, measuring by things, and not merely by values, has been less than in the twenty years immediately before.

We pass on, then, to another set of figures included in the short table above submitted. We may look not only at leading industries of production directly, but at the broad figures of certain industries which are usually held to reflect, as in a mirror, the progress of the country generally. I refer to the railway traffics as regards the home industries of the country, and the entries and clearances of shipping in the foreign trade as regards our foreign business.

As regards railways what we find is, if we take the receipts from the goods traffic in the form in which they were summarized for the Royal Commission on Trade Depression, viz. reduced to so much per head of the population on the average of quinquennial periods, that in the five years 1860–64, which is as far back as the figures can be carried, the receipts per head were 11*s.*; ten years later, viz. in 1870–74, the receipts per head were 18*s.*; and ten years later, viz. 1880–84, the receipts per head were 21*s.* 2*d.* The rate of growth shown in the first ten years' interval is 63 per cent.; in the second ten years' interval it is only 18 per cent.; and in the last year or two, I may add, there has been no further improvement. Here the question of the value of money comes in again, but this would only modify partially the apparent change. There is also a question as to railway extension having been greater in the earlier than in the later period, so that growth took place in the earlier period because there were railways in many districts where they had not been before, and there was no room for a similar expansion in the later period. But the difference in the rate of growth it will be observed is very great indeed, and this explanation seems hardly adequate to account for all the difference. At any rate, to repeat a remark already made, the indications are no longer so simple as they were. There is something to be explained.

The figures as to the number of tons of goods carried are not in the above table; nor are such figures very good, so long as they are not reduced to show the number of tons conveyed one mile. But, *quantum valeant*, they may be quoted from the Board of Trade tables already referred to. The increase, then, in minerals conveyed between 1855 and 1865 is from about 40 million to nearly 80 million tons, or 100 per cent.; between 1865 and 1875 it is from 80 to about 140 million tons, or 75 per cent.; and in the last ten years it is from 140 to 190 million tons only, if quite so much, or about 36 per cent. only. As regards general merchandise, again, the progression in the three ten-yearly periods is in the first from about 24 to 27 million tons, or rather more than 50 per cent.; in the second from 37 to 63 million tons, or 70 per cent.; and in the third from 63 to 73 million tons, or 16 per cent. only. As far as they go there is certainly nothing in these figures to oppose the indications of a falling-off in the rate of increase in the general business already cited.

Coming to the movement of shipping in the foreign trade, the series of figures we obtain are the following, which relate to clearances only, those relating to entries being of course little more than duplicate, so that they need not be repeated: 1855, 10 million tons; 1865, 15 million tons; 1875, 24 million tons; 1885, 32 million tons. And the rate of growth thus shown is between 1855 and 1865 no less than 50 per cent.; between 1865 and 1875 no less than 60 per cent.; and between 1875 and 1885 about 33 per cent. only—again a less rate of increase in the last ten years than in the period just before. Here, too, it is to be noticed, what is unusual in shipping industry, that in the last few years the entries and clearances in the foreign trade have been practically stationary. The explanation no doubt is in part the great multiplication of lines of steamers up to a comparatively recent period, causing a remarkable growth of the movement while the multiplication of lines was itself in progress, and leaving room for less growth afterwards because a new framework had been provided within which traffic could grow. But here again it is to be remarked that the whole change can hardly, perhaps, be explained in this manner, while the remark already made again applies, that the fact of explanation being required is itself significant.

The figures of imports and exports might be treated in a similar manner, as they necessarily follow the course of the leading articles of production and the movements of shipping. But we should only by so doing get the figures we have been dealing with in another form, and repetition is of course to be avoided.

The short table contains only another set of figures, viz. those of the consumption of tea and sugar, which are again commonly appealed to as significant of general material progress. What we find as regards tea is that the consumption per head rises between 1855 and 1865 from 2·3 to 3·3 lbs., or 43 per cent.; between 1865 and 1875 from 3·3 to 4·4 lbs., or 33 per cent.; and between 1875 and 1885 from 4·4 to 5 lbs., or 13½ per cent. In sugar the progression is in the first period from

30·6 to 39·8 lbs. per head, or 30 per cent.; in the second period from 39·8 to 62·7 lbs., or 58 per cent.; and in the third period from 62·7 to 74·3 lbs., or 19 per cent. only. In the last ten years in both cases the rate of increase is less than in the twenty years before.

These facts, I need hardly say, would be strengthened by a reference to the consumption of spirits and beer, the decline in the former being especially notorious. In tobacco again in the last ten years there has been no increase of the consumption per head; which contrasts with a rapid increase in the period just before—viz. from about 1·31 lb. per head in 1865 to 1·46 lb. per head in 1875.

No doubt the observation here applies that the utmost prosperity would obviously be consistent with a slower rate of increase per head from period to period in the consumption of these articles, and with, in the end, a cessation of the rate of increase altogether. The consumption of some articles may attain a comparatively stationary state, the increased resources of the community being devoted to new articles. But here, again, we have to observe the necessity for explanation. The indications are no longer so sure and obvious in all directions as they were.

It is difficult, indeed, to resist the impression made when we put all the facts together, leaving out of sight for a moment those of values only. We are able to affirm positively—(*a*) That the production of coal, iron, and other staple articles has been at a less rate in the last ten years than formerly; (*b*) that this has taken place when agricultural production has been notoriously stationary, and when the production of other articles such as copper, lead, &c., has positively diminished; (*c*) that there has been a similar falling-off in the rate of advance in the great textile industries; (*d*) that the receipts from railway traffic and the figures of shipping in the foreign trade show a corresponding slackening in the rate of increase in the business movement; and (*e*) that the figures as to consumption of leading articles, such as tea, sugar, spirits, and tobacco, in showing a similar decline in the rate of increase, and, in some cases, a diminution, are at least not in contradiction with the other facts stated, although it may be allowed that there was no antecedent reason to expect an indefinite continuance of a former rate of increase.

From these facts, however we may qualify them—and many qualifications have already been suggested, while others could be added—it seems tolerably safe to draw the conclusion that there has probably been a falling-off in the rate of material increase generally. The income-tax assessment figures, though they could not be taken by themselves in such a question, are, at least, not in contradiction, and there is nothing the other way when we deal with these main figures only. I should not put the conclusion, however, as more than highly probable. Some general explanation of the facts may be possible on the hypothesis that there is no real decline in the rate of growth generally at all; that the usual signs for various reasons have become more difficult to read; that owing to the advance already made the real growth of the country and, to some extent, of other countries, has taken a new direction; and that the utmost caution must be used in forming final conclusions on the subject. But the conclusion of a check having occurred to the former rate of growth may be assnmed meanwhile for the purposes of discussion. The attempted explanation of the causes of change, on the hypothesis that there is a real change, may help to throw light on the question of the reality of the change itself.

Various explanations are suggested, then, not only for a decline in the rate of our progress, but for actual retrogression. Let us look at the principal of these explanations in their order, and see whether they can account for the facts: either for actual retrogression, or for a decline in the general rate of material growth equal to what some of the particular facts above cited, if they were significant of a general change in the rate of growth, imply—a decline, say, from a rate of growth amounting to 40 per cent. in ten years to one of 20 per cent. only in the same period.

One of the most common explanations, then, as we all know, is foreign competition. The explanation has been discredited because of the exaggeration of the alleged evil to be explained; but it may possibly be a good enough explanation of the actual facts when they are looked at in a proper way. In this light, then, the assertion as to foreign competition would be found to mean that foreigners are taking away from us some business we should otherwise have had, and that, consequently, although our business on the whole increases from year to year, it does not increase so fast as when foreign competition was less. Those who talk most about foreign competition have actually in their mind the unfair element in that competition, the stimulus which the Governments of some foreign countries give or attempt to give to particular industries by means, on the one hand, of high tariffs keeping out the goods we should otherwise send to such countries, and giving their home industry of the same kind a monopoly which sometimes enables them to produce a surplus they can sell ruinously cheap abroad; and by means, on the other hand, of direct bounties which enable certain industries to compete in the home market of the United Kingdom itself, as well as in foreign markets. But there is a natural foreign competition as well as a stimulated foreign competition to be considered, and it may be the more formidable of the two.

Dealing first with the stimulated competition, the most obvious criticism on this alleged explanation of the recent decline in the rate of increase of our material progress is that the stimulus given by foreign Governments in recent years has not been increasing, or at any rate not materially increasing, so as to account for the change in question. People forget very quickly; otherwise it would not be lost sight of that after 1860, as far as European nations are concerned, there was a great reduction of tariff duties—a change, therefore, in the contrary direction to that stimulus which is alleged to have lately caused a change in the rate of our own development. Since about five or six years ago the movement on the Continent seems again to have been in the direction of higher tariffs. France, Italy, Austria, Germany, and Russia have all shown protectionist leanings of a more or less pronounced kind. Some of our colonies, especially Canada, have moved in the same direction. But, on the whole, these causes as yet have been too newly in operation to affect our industry on a large scale. As a matter of fact, with one exception to be presently noticed, the period from 1860 to 1880 was one in which the effect of the operation of foreign Governments in regard to their tariffs could not be to stimulate additional competition of an injurious kind with us in the way above described, but to take away, if anything, from the stimulus previously given. The changes quite lately brought into operation, if big enough, and if really having the effects supposed, might stimulate foreign competition in the way described in the period now commencing; but, as an explanation of the past facts, it is impossible to urge that foreign competition had recently been more stimulated by additions to tariffs than before, and that in consequence of this stimulus our own rate of advance had been checked.

The one exception to notice is the United States. Immediately after 1860 the civil war in that country broke out, and that war brought with it the adoption of a very high tariff. Curiously enough, however, that tariff operated most against us in the very years, that is, the years before 1875, in which our rate of advance was greater to all appearance than it has lately been. In 1883 there was a great revision of the tariff, having for its general result a slight lowering and not an enhancement of the tariff, and it is with this reduction, that is, with a diminution of the alleged adverse stimulus, that the diminution in our own rate of advance has occurred.

Of course the explanation may be that, although Governments have not themselves been active till quite lately in adding to their tariffs, yet circumstances have occurred to make the former tariffs more injurious in recent years than they were down to 1875. For instance, it may be said that, owing to the fall of prices in recent years, the burden of specific duties has become higher than it was. The duty is nominally unchanged, but by the fall of prices its proportion to the value of the article has become higher. This is no doubt the case to a large extent. On the other hand, *ad valorem* duties have been lowered in precisely the same way. The fall of prices has brought with it a reduction of duty; and especially on articles of English manufacture, where the raw material is obtained from abroad, the reduction of duty, being applicable to the whole price, must certainly have had for effect to render more effective than before the competition of the English manufacturer. Whether on the whole the reduction of *ad valorem* duties consequent on the fall of prices has been sufficient throughout the range of our foreign trade to compensate the virtual increase of the weight of specific duties from the same cause seems to be a nice question. This being the case, it must be very difficult indeed to show that on the whole the weight of foreign tariffs, apart from the action of foreign Governments, has been increased in recent years so as to affect our own growth injuriously.

Foreign tariffs, it may be said, have become more effective for another reason. Manufacturing industry having itself developed abroad, the same amount of protection given to the foreign industry becomes more efficient than it was. But this, of course, raises the question of the effect of natural foreign competition, which will presently be discussed.

So much for the stimulus to foreign competition due to high tariffs. With regard to bounties, very little need be said. They have been the subject of much discussion and agitation for various reasons, and in what I have to say I propose not to touch on the practical question whether these bounties are injurious, and the nature of the political remedies that may or may not be possible. I limit myself strictly to the point, how far any effect which such bounties can have had would account for a diminution in the rate of material growth of the country generally in the last ten years as compared with the ten years just before. Dealing with the question in this strictly limited fashion, what I have to observe first is, that hitherto very few bounties have been complained of, except those on sugar production and refining; and next, that the whole industries of sugar production and refining, important as they are in themselves, hardly count in a question of the general history of the United Kingdom. Even if we refined all the sugar consumed in the United Kingdom and the maximum amount we have ever exported, the whole income from this source, the whole margin, would not exceed about £2,000,000 annually, not one six-hundredth part of the income of the people of the United Kingdom; and of this £2,000,000 at the worse we only lose a portion by foreign competition, while all that is really lost, it must be remembered, is not the whole income which would have been gained if a certain portion of our labour and capital had been employed in sugar refining, but only the difference between that income and the income obtained by the employment of the same labour and capital in other directions. The loss to the Empire may be greater, because our colonies are concerned in sugar production to the extent at present prices, of £5,000,000 to £6,000,000 annually, which would probably be somewhat larger but for foreign competition. But it does not seem at all certain that this figure would be increased if foreign bounties were taken away, while in any case the amounts involved are too small to raise any question of foreign bounties having checked the rate of growth of the general industry of the country.

Per contra, of course, the extra cheapness of sugar, alleged to be due to the bounties, must have been so great an advantage to the people of the United Kingdom, saving them perhaps £2,000,000 to £3,000,000 per annum, that the stimulus thereby given to other industries must apparently have far more than compensated any loss caused by the stimulus of foreign bounties to sugar production and refining abroad. But to enlarge on this point would involve the introduction of controversial matter, which I am anxious to avoid. I am content to show that nothing that can have resulted from sugar bounties could have affected seriously the general rate of material growth in the country.[1]

Mutatis mutandis, the same remarks apply to other foreign bounties, of which indeed the only ones that have been at all heard of are those on shipping. But as yet, at least, the increase of foreign shipping has not been such as to come into comparison with our own increase, while the portion of the increase that can be connected with the operation of bounties is very small. It would be useless to enter into figures on so small a point; but few figures are so well known or accessible as those relating to shipping.

In neither way, then, does there appear to be anything in the assertion that the protectionist action of foreign Governments in recent years can have caused the check alleged to the rate of growth in our industry generally, assuming such a check to have occurred. I may be dispensed, therefore, from entering on the theoretical argument, which I only notice *pour mémoire*, that in the nature of things no enhancement of foreign tariffs and no grants of foreign bounties could really check our own rate of growth, except by checking foreign growth still more, which is not the case we are considering, because the allegation is that foreign competition is increasing at our expense. That I do not insist on this argument is not to be considered as a sign that it is dropped or that I am not fully sensible of its logical completeness. It seems enough, at present, to fortify it by considerations from actual practical facts which no one can dispute.

The question of an increase of foreign competition from natural causes is more difficult. It is beyond all question, as I have pointed out elsewhere, that foreign competition in every direction from natural causes must continue to increase, and that it has increased greatly in recent years. But when the facts are examined, it does not appear that this competition has been the cause of a check to our own rate of growth. One of the facts most commonly dwelt upon in this connexion is the great increase of the imports of foreign manufactured articles into the United Kingdom. But the increase in the last ten years is not more than about £18,000,000, taking the facts as recorded in what is known as Mr. Ritchie's Return, viz. from about £37,000,000 in the quinquennial period 1870–74 to £55,000,000 in the quinquennial period 1880–84, or about 50 per cent. Out of £18,000,000 increased imports of such articles it is fair to allow that at least one-half, if not more, is the value of raw material which we should have had to import in any case; so that only £9,000,000 represents the value of English labour displaced by these increased imports. Even the whole of this £9,000,000 of course is not lost, only the difference between it and the sum which the capital and labour "displaced" earns in some other employment, which may possibly even be a *plus* and not a *minus* difference. If we add articles "partly manufactured" no difference would be made, for the increase here is only from £26,000,000 to £28,000,000 in the ten years. Such differences, it need not be said, hardly count in the general total of the industry of the country. Further, the rate of increase of these imports was just as great in the period when our own rate of growth was greater, as in the last ten years, the increase in manufactured articles between 1860–64 and 1870–74 being £19,000,000, viz. from £18,000,000 to £37,000,000, or over 100 per cent. as compared with 50 per cent. only in the last ten years, and in articles partly manufactured from £17,000,000 to £26,000,000, an increase of £9,000,000 as compared with an increase of £2,000,000 only in the last ten years. Making all allowance for the fall in prices in recent years, these figures will show a greater relative increase of imports of manufactured articles before 1875 than afterwards. It cannot, therefore, be the increased import of foreign manufactures which has caused the check to our own growth in the last ten years.

But foreigners, it is said, exclude us from their own markets and compete with us in foreign markets. Here again, however, we find that any check which may have occurred to our foreign export trade is itself so small that its effect on the general growth of the country would be almost *nil*. Take it that the check is as great as the diminution in the rate of increase in the movements of shipping, viz. from an increase of 55 per cent. to one of 33 per cent. only, that is, broadly speaking, a diminution of one-third in the rate of increase of our foreign trade, whatever that rate may have been. Assuming that rate to have been the same as the rate of increase in the movements of shipping itself, the change would be from a rate of increase equal to one-half in ten years to a rate of increase equal to about one-third only. Applying these proportions to the exports of British and Irish produce and manufactures, which represent the productive energy of the country devoted to working for foreign exchange, and assuming that ten years ago the value of British labour and industry in the produce and manufactures we exported, due deduction being made for the raw material previously imported, was about £140,000,000 (see my "Essays in Finance," first series), then it would appear that if the same range of values had continued, the check to the growth of this trade would have been such that at the end of ten years the British labour represented in it, instead of having increased 50 per cent., viz. from £140,000,000 to £210,000,000, would have increased one-third only, or from £140,000,000 to about £187,000,000. The annual difference to the energy of the country developing itself in the foreign trade would on this showing be about £23,000,000 only, an insignificant sum compared with the aggregate income of the people of the country; while the country, it must be remembered, does not lose the whole of this sum, but only the difference between it and the sum earned in those employments to which those concerned have resorted, which again may be a *plus* and not a *minus* difference. Even, therefore, if foreign competition is the cause of a check to our general growth, yet the figures we are dealing with in our foreign trade are such that any visible check to that trade which can have occurred must have been insufficient to cause that apparent diminution in the rate of our material growth generally which has to be explained.

It has to be remembered, moreover, that when the figures are

[1] See Appendix to "First Report of Royal Commission on Trade Depression," p. 130.

studied, and the fall of prices allowed for, it is not in our foreign trade that any check worth mentioning seems to have occurred at all. The diminution in the rate of increase in the movements of shipping is very largely to be accounted for in the way already explained, viz. by the fact that the increase just before 1875 was largely owing to the multiplication of lines of steamers, and that a framework had then been provided up to which the traffic has since grown. Even an increase of one-third in the movements in the last ten years may thus show as great an increase in real business as an increase of 50 or 60 per cent. in the movements in the twenty years before. Foreign competition, even from natural causes, is thus insufficient to account for the diminution in the rate of increase of our material growth in the last ten years.

These figures may be put directly another way. The increase of our foreign exports per head between 1860–64 and 1870–74 was from £4 14*s.* 11*d.* to £7 7*s.* 5*d.*, or about 55 per cent., and allowing for an average rise of prices between the two dates, may be put as having been at the extreme about 50 per cent. Between 1870–74 and 1880–84, instead of an increase, there is a decrease, viz. from £7 7*s.* 5*d.* to £6 12*s.* 9*d.*, but deducting about one-third from the former figure for the fall in prices, the real increase in the last ten years would appear to be as from £4 16*s.* 3*d.* to £6 12*s.* 9*d.*, or over 35 per cent. The difference in the rate of increase in the last ten years compared with the previous ten is thus the difference between 35 and 50 per cent. only, equal to about £21,000,000 annually on the amount of £140,000,000 assumed to represent the value of British industry in our foreign exports, deduction being made for the value of raw material included. A deduction of this sort from the annual income of the country is too small to account for such a check to the rate of our growth generally as that we are now discussing as probable, especially when we recollect that the labour is only diverted, and it is not the whole £21,000,000 that is lost, but only the difference between that sum and what is otherwise earned, which may even be a *plus* and not a *minus* difference.

To bring the matter to a point, an increase of 40 per cent. in the income of the country in ten years would, on an assumed income of 1000 millions only in 1875—and the figure must then have been more—have brought the income up to 1400 millions; an increase of 20 per cent. would have brought it up to 1200 millions only, a difference of 200 millions, which must have arisen from the alleged difference in the rate of our material growth in question if it had occurred. Clearly nothing can have happened in our foreign trade to account for anything more than the smallest fraction of such a difference. The figures are altogether too small. We may repeat again, then, that it is not the check to our foreign trade which foreign competition may have caused to which we can ascribe the recent check to our general rate of growth.

I need hardly add that in point of theory foreign competition was not likely to have the effect stated. I have set forth elsewhere in an elaborate essay ("Essays in Finance," second series, "Foreign Manufactures and English Trade") the reasons for holding this opinion; why it is, in fact, that as foreign nations grow richer we should be better off absolutely than if they were to remain poor, though relatively they might advance more than we do. But, whatever theory may say, in point of fact the check to the rate of our material growth cannot, for the reasons stated, have been due to anything which has happened to our foreign trade.

Another explanation which has been suggested, and to which I have myself been inclined to attach considerable weight as being plainly, as far as it goes, a *vera causa*, is the extent to which the hours of labour have been reduced in many employments in consequence of the improvement in the condition of the working classes in the last half-century, and the growth of a disposition to take things easier, which has been the result of the general prosperity of the country. Such causes, when they exist, and when they are brought into operation, must tend to diminish the rate of material growth in a country as compared with a period just before when they were not in operation. If we could suppose them brought into operation suddenly, all other things, such as the progress and development of invention, remaining the same, such a reduction of hours of labour and growth of a disposition to take things easy must produce a check to the former rate of growth.

After some consideration, however, although there is no doubt of the general tendency of the causes referred to, I begin to doubt whether they would explain adequately such a check to the rate of material growth generally throughout the country as is assumed to have occurred. As regards the shortening of the hours of labour, which is the more definite fact to be dealt with, it cannot but be observed that the shortening has by no means been universal. It has been conspicuous among certain trades organized into trade unions; but the unions, after all, only include about a tenth part of the labour of the country. There has been no such conspicuous shortening of the hours of labour among professional men, clerks, domestic servants, and many others whose labour is an essential part of the general sum total. Next—and this is perhaps even more important—the shortening of the hours of labour is not coincident with the beginning of the last ten years, though it has been in full operation for the whole of that period, but rather with the beginning or middle of the previous ten years, viz. 1865–75; so that it should have been fully in operation upon the production of 1875; and the check to our rate of growth if due to this cause should thus have been felt between 1865 and 1875, rather than between the latter date and the present time. The same with the general disposition to take things easy. This disposition did not spring up in a day in 1875, but was probably as effective as a cause of change in the earlier, as in the later, period. It must count for something as a cause of the annual production of the country being less at a given moment than it would otherwise be; but in comparing two periods what we have to consider is whether the growth of this disposition has been greater in one period than in another; and there are no data to support such a conclusion as regards the last ten years compared with the previous ten.

We must apparently, therefore, reject this explanation also. It is not adequate to account for the apparent change that has occurred in the rate of our growth from the year 1875 as compared with the period just before. Our progress in periods previous to 1875 took place in spite of the operation of causes of a similar kind which were then in operation, and there is no proof at all that the shortening of the hours of labour and the growth of a disposition to take things easy have been greater since 1875 as compared with the period just before than they were between 1865 and 1875 as compared with the period just before that. What is wanted is a new cause beginning to operate in or about 1875, and the shortening of the hours of labour and the growth of a disposition to take things easy do not answer that description sufficiently. Something of the apparent change may be due to an acceleration in recent years of the growth of a disposition to take things easy, but on the whole the explanation halts when we make a strict comparison.

Another cause which may properly be assigned as a *vera causa* of a check to the rate of material growth in the country is the unfavourable weather to agriculture, and the generally unprofitable conditions of that industry in recent years. *Pro tanto* such influences would make agricultural production less to-day than it would otherwise be. Employment in that industry would also be diminished comparatively, and perhaps absolutely, and a check to production generally would take place while labour was seeking new fields. But the check arising in this manner, as far as the general growth is concerned, has obviously not been very great. More land in proportion has been turned into permanent pasture, but very little land has gone out of cultivation altogether, and even the amount under the plough has not much diminished. Agricultural labour, in somewhat greater proportion than before, has been obliged to seek other employments; the flow of population from country to town has been increased somewhat; but nothing new has happened to diminish production generally to a serious extent, and it is a new cause, it must be remembered, for which we are seeking. As far as unfavourable weather is concerned, again, that is only a temporary evil. One year with another, the weather is not worse now than at any former time; the remarkably unfavourable weather which lasted from 1874 to 1880 has passed. The other conditions unfavourable to agriculture, especially foreign competition, are more enduring; but these seem much more unfavourable to rent than to production itself, which is the point now under consideration; and we do not know that they will be permanent at all when prices and wages are fully adjusted.

The disturbance to industry by the fall of prices generally is also a *vera causa* of a check to the rate of material growth. But the effect of such a cause seems to be confined within narrow limits, and it is not a new cause. It occurs in every time of depression due to discredit, being partly the effect and partly the cause of the depression itself. All that is new recently is the extreme degree of the fall, and I must express the greatest doubt whether a mere difference of degree aggravates materially the periodical disturbance of industry, tending to check production, which a fall of prices from a high to a low level causes. So far as past experience has gone, at any rate, no such cause has been known to check production to any material extent. If any such cause tended to have a serious effect we should witness the results every time there is a shrinkage of values owing to the contraction and appreciation of an inconvertible paper currency, and I am not aware of any such contraction having had the effect described on production, though the effect in producing a feeling of depression is beyond all question. The facts as to the great contraction in this country between 1815 and 1820 are on record, while the experience of the United States after the civil war is also fresh in everyone's recollection. Contraction of currency and fall of prices, though they are painful things, do not stop production materially.

Another explanation suggested is that there is in fact no antecedent reason for supposing that the rate of material growth in a community should always be at the same rate—that a community may, as it were, get "to the top" as regards its development under given conditions, and then its advance should be either less rapid than it had been or it should even become stationary. The defect of this explanation is that it assumes the very thing which would have to be proved. Is there any other sign, except the alleged check to the rate of our material growth itself, that in or about the year 1875 this country got "to the top"? It has, moreover, to be considered that on *a priori* grounds it is most unlikely a community would get to the top *per saltum*, and then so great a change should occur as the apparent change we are considering. The persistence of internal conditions in a given mass of humanity is a thing we may safely assume, and if these conditions are consistent with a given rate of development in one period of ten years, it is most unlikely that, save for an alteration of external conditions, there would be another rate of development in the succeeding ten years. Human nature and capacities do not change like that. Scientific opinion, I believe, is also to the effect that the progress of invention, and of the practical working of inventions, which have been the main cause of our material growth in the past, have been going on in the last ten years, are still going on, and are likely to go on in the near future, at as great a rate as at any time in the last fifty years. Except, as already said, the apparent check to the rate of our material growth itself, there is no sign anywhere of our having got to the top, so that a stationary condition economically, or a condition nearly approaching it, has been reached.

Last of all, it is urged that the diminution in the material of our growth, which is in question, must be due to the fact that we are losing the natural advantages of coal and iron which we formerly had in comparison with the rest of the world. This is perhaps only another way of saying that we have got to the top by comparison, though the community of nations generally has not got to the top, and another way of saying also that foreign competition affects us more than it formerly did—an argument already dealt with. But the question whether coal and iron at home are really so indispensable to our material growth as is sometimes assumed appears itself so important that I may be excused for specially discussing this question, notwithstanding that it has virtually been disposed of, as far as any explanation of past facts is concerned, by what has been already said.

The argument proceeds on the supposition—which is no doubt well founded in the abstract and as far as the past experience of mankind is concerned—that in addition to natural capacities of its own a community requires for its prosperity certain natural advantages, fertility of soil, rich and easily-worked mines, a genial climate in which labour may conveniently be carried on, and so forth. A community possessing all these things, or the like things, will flourish, but as it ceases to lose any of them its prosperity must become precarious, and population must flow to the places where they can be secured. Of course climate is not a thing which changes, as far as any practical experience is concerned; but relatively the advantage of a fertile soil may be lost, as England has lately lost it in comparison with the United States and other new countries, its soil having become inadequate for the whole population; and still more the advantage of mines, especially mines of coal and iron, on which the miscellaneous industries of a manufacturing country depend, may be lost. Hence it is said the check to our rate of growth in recent years. We have long since lost our agricultural advantages by comparison. Now we are also beginning to lose the special advantages which coal and iron have given. Our mines are becoming less rich than those of foreign countries, and the balance is turning against us. Why should not population relatively flow from England to the United States and other countries as it has passed within the limits of the United Kingdom itself from Cornwall and Sussex to Staffordshire, Lancashire, Yorkshire, and the north? In this view the coal famine of 1873 was the sign of a check such as Mr. Jevons anticipated. What has happened since is only a sequence of the like causes.

I need not repeat in opposition to this view what has already been said as to the inadequacy of any actual decline in our foreign trade to account for such a check to our general growth as is supposed to have occurred. If the loss of our natural advantages of coal and iron in addition to agriculture are having the effects supposed, we ought to witness them in our foreign trade, and in fact we do not witness them to the extent required for the production of the phenomenon in question.

What I wish now specially to urge is that in consequence of the progress of invention and the practical application of inventions in modern times the theory itself has begun to be less true generally than it has been. It is no longer so necessary, as it once was, as in fact it always has been until very lately, that people should live where their food and raw materials are grown. The industry of the world having become more and more manufacturing and, if one may say so, artistic, and less agricultural and extractive, the natural advantages of a fertile soil and rich mines are less important to a manufacturing community than they were at any former period of the world's history, because of the new cheapness of conveyance. Under the new conditions, I believe it is impossible to doubt, climate, accumulated wealth, acquired manufacturing skill, concentration of population, become more important factors than mere juxtaposition to the natural advantages of fertile soil and rich mines. The facts seem at any rate worth investigating, judging by what has happened in England and other old countries in the last half-century, and by what is still happening there.

Take first the question of food. Wheat is now conveyed from the American Far West to Liverpool and London and any other ports in the Old World for something like five shillings per quarter—equal to about half a farthing on the pound of bread, or a halfpenny on the quartern loaf. The difference between the towns of a country with fertile soil, therefore, and the towns of a country with inadequate soil is represented by this small difference in the price of bread. At about fivepence the quartern loaf the staff of life may be about 10 per cent. cheaper in the fertile country than it is in a country which does not grow its own food at all, and which may be thousands of miles away. As the staff of life only enters into the expenditure of the artisan to the extent of 20 per cent. at the outside, and into the expenditure of richer classes to a smaller extent, the difference on the whole income of a community made by their living where the staff of life would be cheaper would be less than 2 per cent.—too small to tell against other advantages which may be credited to them. What is true of wheat is even more true of meat and other more valuable articles of food, where the cost of conveyance makes a less difference in the proportionate value of the food *in situ* and its value at a distant point. The same more and more with raw materials. Cotton and such articles cost so little to transport that the manufacturing may as well go on in Lancashire or any other part of the Old World as *in situ* or nearly *in situ*; and even as regards metals or minerals, except coal and perhaps iron, the same rule applies, the cost of conveyance being as nothing in proportion to the value of the raw material itself. As regards coal and iron, moreover, there are many places where they are not in absolute juxtaposition, and if they have to be conveyed at all they may as well be conveyed to a common centre. Iron ore and iron at any rate are beginning to be articles of import into the old countries of Europe to which the cost, in fact, offers very little difficulty. The additional cost to the miscellaneous manufacturing of a country through its having to bring iron and coal from a distance may thus be quite inconsiderable, and apparently is becoming more and more inconsiderable. As regards raw materials generally it has also to be considered that, owing to their immense variety, there is an undoubted convenience in a common manufacturing centre to which they can be brought. Hitherto they may have come to England and other old countries of Europe in part because coal and iron were abundant there in juxtaposition; but the habit once set up, there seems no reason why they should not concentrate themselves on the old manufacturing centres. The ruder parts of the coal and iron industry may be attracted to other places, but the higher branches of manufacturing will be at no disadvantage if carried on at the old centres.

On the other hand, the old centres will retain the advantages, which are obviously very great, of climate, accumulated wealth, acquired skill, and concentration of population. That population under the new conditions is to go from them merely because they do not grow food which can be transported to them at the cost of a mere fraction of the aggregate income, and because they have not coal and iron in abundance and in juxtaposition, that abundance and juxtaposition, owing again to the diminished cost of conveyance, being no longer so indispensable as it was to the higher branches of manufacturing, appears certainly to be a "large order." What I have to suggest most strongly at any rate is that the advantages I have spoken of as possessed by old manufacturing centres are not unlikely to tell more and more under the new conditions, and that the indispensability of coal and iron is no longer to be spoken of as what it has been in the last century, during which apparently England owed so much of its precedence in manufacturing power to these causes.

To the same effect we may urge the specially great increase of the efficiency of coal in recent years. Cheap coal *in situ* cannot be relatively so important as it was in days when five or ten tons of coal were required to do the work which can now be done by one.

The truth is that the whole change that has been occurring is only a continuation of much larger historical changes. There has almost always in English history been some one industry that was supposed to be king. In the Middle Ages it was the growth and export of raw wool; last century it was the woollen manufacture itself; early in this century and down to a very late date cotton was king; more lately, since the beginning of the railway and steamship era, it has been coal and iron. How do we know, how can we know, that coal and iron are to reign indefinitely, any more than wool, or the woollen manufacture, or cotton themselves have done? Changes are always going on, and for that reason I believe we should attach the more importance to the increasing signs that it is no longer necessary or indispensable for prosperous communities to live where their food and raw materials are grown; that there may be advantages of climate, of accumulated wealth, of acquired skill, of concentration of population which are now, under the new conditions, overwhelmingly more important. It would be absurd to dogmatize in such a matter. I hope, however, I have said enough to those who care to reflect to satisfy them that the indispensability even of coal and iron to the continuance of our material growth is no longer to be assumed, that there are wholly new conditions to be considered.

To come back to the practical point in all this discussion. Not only is there no sign in anything that has yet happened that the apparent check to our former rate of material growth is due to the loss of natural advantages which we once possessed, but the theory of natural advantages itself requires to be revised. Equally in this way as in the other ways that have been discussed, it is impossible to account for the apparent check to the former rate of our material growth which has been observed.

Having carried matters so far, however, and having found the insufficiency of the various causes which have been assigned for the check to our former rate of material growth, because they have not produced the sort of effect in detail which they ought to have produced so as to lead to the general effect alleged, or because they existed quite as much when the rate of growth was great as in recent years when a diminution has apparently been observed, it would seem expedient to inquire whether, in spite of the accumulation of signs to that effect, the apparent check to our rate of growth may, after all, not be a real one. To some extent I think we must conclude that this is the case. There are other facts which are inconsistent with a real and permanent check such as has been in question, and a general explanation of the special phenomena of arrest seems possible without supposing any such real check.

The first broad fact that does not seem quite reconcilable with the fact of a real diminution of the kind alleged in the rate of material growth generally is the real as distinguished from the apparent growth of the income-tax assessments when allowance is made for the fall of prices which affect, as we have seen, all aggregate values. Assuming the fall of prices to be about 20 per cent., then we must add one-fourth to the assessments in 1885 to get the proper figure for comparison with 1875. The total of 631 millions for 1885 would thus become 787 millions, which is a falling-off of 35 millions, or 4 per cent. only, from the figure of 822 millions, which should have been reached if the rate of growth had been the same between 1875 and 1885 as between 1865 and 1875. Allowing for the raising of the lower limit of the income-tax in the interval, this is really no decrease at all.

Of course this comparison may be thrown out if we are to assume the difference made by the fall of prices on the income-tax assessments to be 15 or 10 per cent. only, instead of 20 per cent. But a point like this would involve a most elaborate discussion, for which this address would hardly be the occasion. I hope to find a better opportunity shortly in a continuation of my essay of ten years ago on the accumulations of capital in the United Kingdom. There is no doubt, however, that an allowance must be made for the difference of prices, and when any such allowance is made the rate of material growth would not appear to be so very much less between 1875 and 1885 than in the period just before, as it does in the above figures.

Another broad fact not easily reconcilable with the fact of a great diminution in the real rate of material growth in the last ten years is the steadiness of the increase of population and the absence of any sign, such as an increase in the proportion of pauperism, indicating that the people are less fully employed than they were. The increasing numbers must either be employed or unemployed, and if there is an increase in the proportion of the unemployed the fact should be revealed in the returns of pauperism somehow. The existence of trade unions, no doubt, prevents many workmen coming on the rates who might formerly have done so, but there are large masses of workmen, the most likely to feel the brunt of want of employment, to whom this explanation would not apply.

What we find, however, is that population has increased as follows: between 1855 and 1865 from 27,800,000 to 29,900,000, or 7½ per cent.; between 1865 and 1875 from 29,900,000 to 32,800,000, or nearly 10 per cent.; and between 1875 and 1885 from 32,800,000 to 36,300,000, or over 10 per cent. If it is considered that the figures are not fairly comparable for the early period, owing to the specially large emigration from Ireland, which took away from the apparent numbers of the United Kingdom as a whole, but still allowed of as great an increase in the manufacturing parts of the country as there has been later, then we may take the figures for England only, and what we find is—between 1855 and 1865 an increase from 18,800,000 to 21,100,000, or 12½ per cent.; between 1865 and 1875 from 21,100,000 to 24,000,000, or nearly 14 per cent.; and between 1875 and 1885 from 24,000,000 to 27,500,000, or 14½ per cent. Whether, therefore, we take the figures for the United Kingdom or for England only, what we find is a greater increase of population in the last ten years than in either of the previous decades when the rate of material growth seemed so much greater. If there had been such real diminution in the rate of material growth, ought there not to have been some increase in the want of employment and in pauperism to correspond?

It is one of the most notorious facts of the case, however, that there has been no increase, but instead a very steady decrease of pauperism, excepting in Ireland, which is so small, however, as not to affect the general result. As regards England the figures are very striking indeed. The average number of paupers and proportion to population have been as follows in quinquennial periods in England since 1885:—

	Number of Paupers.	Proportion to Population per cent.
1855–59	895,000	4·7
1860–64	948,000	4·7
1865–69	962,000	4·5
1870–74	952,000	4·2
1875–79	753,000	3·1
1880–84	787,000	3·0

Thus there has been a steady diminution in the proportion to the population all through, accompanied by a diminution in the absolute numbers between 1865–69 and 1875–79, though there has since been a slight increase. In spite of all that can be urged as to a more stringent Poor-Law administration having made all the difference, it is difficult to believe that a real falling-off of a serious kind in the rate of our material growth in late years as compared with the period just before should not have led to some real increase of pauperism. Change of administration may do much, but it cannot alter the effect of any serious increase in the want of employment in a country.

The corresponding figures as to Scotland are much the same :—

	Number of Paupers.	Proportion to Population per cent.
1855–59	123,000	4·2
1860–64	125,000	4·2
1865–69	131,000	4·3
1870–74	123,000	3·7
1875–79	103,000	2·9
1880–84	100,000	2·7

Here there is the same steady diminution in the proportion of pauperism to population all through as we have seen in the case of England, accompanied in this case by a steady diminution of the absolute number of paupers since 1865–69. The Scotch administration has been totally independent of the English, but the same results are produced.

In Ireland, as already hinted, the history has been different. There has been an increase in the pauperism accompanied by a decline of population. But Ireland is too small to affect the general result.

We are thus confronted by the fact that if there had been a real check of a serious kind to the rate of our material growth in the last ten years as compared with the ten years just before, there ought to have been some increase in the want of employment and in pauperism, but instead of there being such an increase there is a decline. The population apparently, while increasing even more rapidly in the last ten years than before, has been more fully employed than before. To make these facts consistent with a check to the rate of our material growth we must contrive some such hypothesis as that employment has been more diffused as regards numbers, but the aggregate amount of it has fallen off—another form of the hypothesis as to the effect of shorter hours of labour already discussed ; but a little reflection will show that any such hypothesis is hardly admissible. It is difficult to imagine any change in the conditions of employment in so short a time which would make it possible for larger numbers to be employed along with a diminution in the aggregate amount of employment itself.

Another fact corresponding to this decrease of pauperism is the steady increase of savings-bank deposits and depositors. These deposits are not, of course, the deposits of working classes only, technically so called. They include the smaller class of tradesmen and the lower middle classes generally. But, *quantum valeant*, the facts as to a growth of deposits and depositors should reflect the condition of the country generally in much the same way as the returns of pauperism. What we find then is, as regards deposits, that the increase between 1855 and 1865 was from £34,300,000 to £45,300,000, or about one-third ; between 1865 and 1875 from £45,300,000 to £67,600,000, or about one-half; and between 1875 and 1885 from £67,600,000 to £94,053,000, or just about 40 per cent.—a less increase than in the previous ten years, but not really less, perhaps, if allowance is made for the fall of prices in the interval, and in any case a very large increase. Then, as regards depositors, what we find is an increase between 1855 and 1865 from 1,304,000 to 2,079,000, or 59 per cent. ; between 1865 and 1875 from 2,079,000 to 3,256,000, or 56 per cent. ; and between 1875 and 1885 from 3,256,000 to over 5,000,000, or over 50 per cent. Whatever special explanations there may be, facts like these are at least not inconsistent with a fuller employment of the population in the last ten years than in the previous ten.

Yet another fact tending to the same conclusion may be referred to. The stationariness or slow growth of the income-tax assessments in general in the last ten years, as compared with the rapid increase in the ten years just before, has already been referred to as one of the signs indicating a check in the rate of advance in our material growth. But when the returns are examined in detail there is one class of assessments, more significant, perhaps, than any, of the general condition of the nation, viz. houses, which is found to exhibit as great an increase in the last ten years as in the previous decade. Between 1865 and 1875 the increase in the item of houses in the income-tax assessments in the United Kingdom was from £68,800,000 to £94,600,000, or just about 37 per cent. In the following ten years the increase was from £94,600,000 to £128,500,000, or just about 36 per cent. In "houses," then, as yet there is no sign of any check to the general rate of the material growth of the country. Allowing, in fact, for the great fall in prices in the last ten years, the real increase in houses would seem to have been more in the last ten years than in the ten years just before.

Other facts, such as the increase of Post Office business, may be referred to as tending to the same conclusion. But there is no need to multiply facts. If no hypothesis is to be accepted except one that reconciles all the facts, then these facts as to the increase of population, diminution of pauperism, increase of savings-bank deposits and depositors, increase of houses, must all be taken into account, as well as those signs as regards production and other factors, which have usually been most dwelt upon in discussing the question of the accumulation of wealth and the material growth of the people. If the signs of a check to production in some directions can be reconciled with the fact of an unchecked continuance of the former rate of growth generally, then the later facts cited as to increase of population, diminution of pauperism, and the like, may be allowed to have their natural interpretation and to be conclusive on the point.

Such a general explanation, then, of the facts as to production in leading industries and the like, referred to in the earlier part of this address, consistent with the fact that there is no serious falling-off in the rate of our material growth generally, is to be found in the supposition that industry by a natural law is becoming more and more miscellaneous, and that as populations develop the disproportionate growth of the numbers employed in such miscellaneous industries, and in what may be called incorporeal functions, that is, as teachers, artists, and the like, prevents the increase of staple products continuing at the former rate. This supposition, it will be found, has a good deal to support it in the actual facts as to industry and population in recent years.

The foreign trade shows some sign of the change that is going on. Looking through the list of export articles some remarkable developments are to be noticed. The following short table speaks for itself :—

Exports of the undermentioned Articles in the Years stated, with the Rates of Increase in 1855–65, 1865–75, *and* 1875–85 *compared.*

	Quantities exported.				Increase per cent.		
	1855.	1865.	1875.	1885.	1855–65.	1865–75.	1875–85.
Candles, million lbs.	4	4	5·3	7·8	Nil	33	47
Cordage and twine, thousand cwts.	110	168	111	177	53	−34 [2]	59
Plate glass, million sq. ft.	0·3	0·6	1·6	3·9	100	166	143
Jute yarn, million lbs.	not stated	4·9	15·9	30·7	—	224	93
Jute manufacture, million yds.	,,	15·4	102·1	215	—	563	110
Iron hoops, sheets, &c. thousand tons... ...	,,	116	204	331	—	76	62
Tinned plates, thousand tons	,,	63	138	298	—	119	116
Other wrought iron, thousand tons	,,	214	239	348	—	12	45
Oil and floor cloth, million sq. yds.	0·5	2·4	6·3	11·3	380	162	79
Paper other than hangings, thousand cwts....	106·1 [1]	145	319	733	37	120	130
Dressed skins and furs, millions	not stated	not stated	0·37	3·45	—	—	832
Soap, thousand cwts.	205	140	251	402	−32 [2]	79	60
Spirits, million gals.	3·8	2·0	1·0	2·7	−47 [2]	−50 [2]	170
Unenumerated, values, millions	—	—	£9·7	£10·6	—	—	10

[1] 1858, not separately stated before.

[2] Decrease.

Thus there are not a few articles, of which jute is a conspicuous example, in which there has been an entirely new industry established within a comparatively short period; and, though the percentage of increase may not in all be so great in the last ten years as in the previous ten just because the industry is so wholly new, yet the amount of the increase is as great or greater. In other articles, such as soap and British spirits, there is a new start in the last ten years after a decline in the previous periods. Such cases as oil and floor cloth, paper other than hangings, and plate glass are also specially noticeable as practically new trades. The list I am satisfied could be considerably extended, but I am giving it mainly by way of illustration. Finally, there is the item of other articles not separately specified—an item which is always changing in the statistical abstract because every few years one or more articles grow into sufficient importance to require separate mention, so that any extended comparison of this item for a long series of years is impossible. Still it is ever growing, and what we find in the last ten years is that, in spite of the fall of prices, the growth is from £9,700,000 to £10,600,000, or nearly 10 per cent. Many of the articles referred to, it is plain, cannot run into much money, but the indications of a tendency are none the less clear. What is happening in the foreign trade is happening, we may be sure, in the home trade as well, of which in another way the increase in the imports of foreign manufactures, already referred to in another connexion, is really a sign, as it implies the growth of miscellaneous wants among the consumers.

The census figures as to occupations tend, I believe, to confirm this observation as to the special growth of miscellaneous industries, but the discussion of the figures would require more preparation than I have had time for, and perhaps more space than can well be spared.

As to the growth of incorporeal functions, which is another fact significant of the supposed change in the direction of the employments of the people, I propose to appeal to the testimony of the census figures. I need refer on this head only to the paper read some time ago to the Statistical Society by Mr. Booth. Among those classes of population whose numbers in England and Wales in the last ten years have shown a disproportionate growth are the following :—

Numbers and Percentage of Self-supporting Population employed.

	Numbers.		Percentage.	
	1871.	1881.	1871.	1881.
Transport	524,000	654,000	4·9	5·6
Commercial Class	119,000	225,000	1·1	1·9
Art and Amusement ...	38,000	47,000	0·3	0·4
Literature and Science ...	7,000	9,000	—	0·1
Education	135,000	183,000	1·3	1·6
Indefinite	124,000	269,000	1·2	2·3
Total	947,000	1,387,000	8·8	11·9

Following the indication of these figures, whatever qualification they may be subject to, we are apparently justified in saying that an increasing part of the population has been lately applied to the creation of incorporeal products. Their employment is industrial all the same. The products are consumed as they are produced, but the production is none the less real. If a nation chooses to produce more largely in this form as it becomes more prosperous, so that there is less development than was formerly the case in what were known as staple industries, it need not be becoming poorer for that reason; all that is happening is that its wealth and income are taking a different shape.

It is quite conceivable, then, and is in truth not improbable, that a check to the former rate of material growth in certain directions may have taken place of late years without any corresponding check to the rate of material growth generally, which would seem to be inconsistent with such facts as the growth of population, diminution of pauperism, increase of houses, and the like. The truth would seem to be that with the growth of staple industries, such as cotton, wool, coal, and iron, up to a point, there being reasons for the remarkably quick development of each for many years up to 1875, there comes a growth of new wants, the satisfaction of which drafts a portion of the national energy in new directions. Just because certain staples developed themselves greatly between 1855 and 1875 the time was likely to arrive when they would grow not quite so fast. For the same reason the rapid increase for a certain period in the consumption per head of articles like sugar and tea was likely to be followed by a less rapid increase, the wants of consumers taking a new direction. Probably owing to the more and more miscellaneous character of modern industry, it will become more and more difficult to follow its development by dealing with staple articles only, while changes in aggregate values are untrustworthy as indications of real changes owing to changes in prices. Already there seems to be no doubt the staple articles are no longer a sufficient indication.

A supplementary explanation may be added which helps to explain another difficulty in the matter by which people are puzzled. I can imagine them saying that it is all very well to pooh-pooh the non-increase or slower increase of the production of staple articles and to assume that industry is becoming more and more miscellaneous; but other countries go on increasing their production of these same staple articles. The increase of the manufactures of cotton, wool, coal, and iron in Germany and the United States, they will say, has in recent years been greater in proportion than in England, which is undoubtedly true. The explanation I have to suggest, however, is that the competition with the leading manufacturing country, which England still is, is naturally in the staple articles where manufacturing has been reduced to a system, the newer and more difficult manufactures and the newer developments of industry generally falling as a rule to the older country. Even in foreign countries, however, there are signs of slower growth of recent years in the staple articles as compared with the period just before. In Germany, for instance, the production of coal increased between 1860 and 1866 (I take the years which I find available in Dr. Neumann Spallart's "Uebersichten") from 12,300,000 tons to 28,200,000, or nearly 129 per cent.; between 1866 and 1876 the increase was from the figure stated to about 50,000,000 tons, or about 77 per cent. only; between 1876 and 1885, another period of ten years, from the figure stated to 74,000,000 tons, or less than 50 per cent.—a rapidly diminishing rate of increase. In the United States of America the corresponding figures for coal are 15, 22, 50, and 103 million tons, showing a greater increase than in Germany, but still a rather less rate of increase since 1876 than in the ten years before. The experience as to the iron production would seem to be different, the increase in the United States and Germany having been enormously rapid in the last ten years; but I have not been able here to carry the figures far enough back for comparison. Still the facts as to coal in Germany are enough to show how rapidly the rate of increase of growth may fall off when a certain point is reached, and that the experience of the United Kingdom is by no means exceptional. As the staple articles develop abroad the rate of increase in such articles will diminish too, and foreign industry in turn will become more and more miscellaneous.

The conclusion would thus be that there is nothing unaccountable in the course of industry in the United Kingdom in the last ten years. In certain staple industries the rate of increase has been less than it was in the ten years just before, but there would seem to have been no increase or little increase in the want of employment generally, while there is reason to believe that certain miscellaneous industries have grown at a greater rate than the staple industries, or have grown into wholly new being, and that there has also been some diversion of industry in directions where the products are incorporeal. These facts also correspond with what is going on abroad, a tendency to decline in the rate of increase of staple articles of production being general, and industry everywhere following the law of becoming more miscellaneous. Abroad also, we may be sure, as nations increase in wealth the diversion of industry in directions where the products are incorporeal will also take place. What the whole facts seem to bring out, therefore, is a change in the direction of industry of a most interesting kind. If we are to believe that the progress of invention and of the application of invention to human wants continues and increases, no other explanation seems possible of the apparent check to the rate of material growth which seems to be so nearly demonstrated by some of the statistics most commonly appealed to in such questions.

At the same time I must apply the remark which I applied at the earlier stage to the opposite conclusion that there had been a real check to the rate of increase in our material growth. When the main statistics bearing on a particular point all indicate the same conclusion, it is not difficult to reason from them and to convince all who study them; but when the indications are apparently in conflict it would be folly to dogmatize. I have indicated frankly my own opinion, but I, for one, should like the subject to be more fully threshed out. It is a very obvious suggestion, moreover, that one may prove too much by such figures—that it is an outrage on common-sense to talk of there being no check to the rate of growth in the country when times are notoriously bad and everybody is talking of want of profit. What I should suggest finally, by way of a hypothesis reconciling all the facts, would be that probably there is some check to the rate of material growth in the last ten years, though not of the serious character implied by the first set of figures discussed; that this check may even be too small to be measured by general statistics though it is sufficient to account for no small amount of *malaise;* and that the *malaise* itself is largely accounted for, as I have suggested on a former occasion, by the mere fall of prices, whatever the cause, as it involves a great redistribution of wealth and income, and makes very many people feel poorer, including many who are not really poorer, but only seem so, and many who are really richer if they only allowed properly for the increased purchasing power of their wealth. All these facts are quite consistent with the fact of a very slight real diminution in the rate of our material growth generally, and with that change in the direction of the national industry, significant of a general change beginning throughout the world which would seem to have occurred.

To some extent also it ought to be allowed that the tendency in the very latest years seems unsatisfactory, and that the developments of the next few years should be carefully watched. Up to now there is nothing really alarming in the statistics when they are analyzed and compared. It may be the case, though I do not think it is the case, that causes are in operation to produce that great check and retrogression which have not as yet occurred, though many have talked as if they had occurred. The exact limits of the discussion should be carefully kept in mind.

Fortunately, however, there is no doubt what some of the conclusions on practical points should be. If it be the case that the hold of an old country like England on certain staple industries of the world is less firm than it was, and, as I believe, must be less and less firm from period to period, owing to the natural development of foreign countries and the room there is among ourselves for development in new directions, then we should make assurance doubly sure that the country is really developing in new directions. If our dependence must be on the new advantages that have been described, such as acquired manufacturing skill, concentration of population, and the like, then we must make sure of the skill and of the best conditions of existence for the concentrated population. If, in point of fact, shorter hours of labour and taking things easy have contributed to check our rate of progress slightly, there is all the more reason for improving the human agent in industry so as to make work in the shorter hours more efficient. Looking at the stir there now is about technical education and such matters, and the hereditary character of our population, I see no cause to doubt that the future will be even more prosperous than the past. The national life seems as fresh and vigorous as ever. The unrest and complaints of the last few years are not bad signs. But the new conditions must be fully recognized. The utmost energy, mobility, and resource must be applied in every direction if we are only to hold our own.

REPORTS.

Fourth Report of the Committee, consisting of Prof. Balfour Stewart (secretary), Profs. Stokes, Schuster, G. Johnstone Stoney, Sir H. E. Roscoe, M.P., Captain Abney, and Mr. G. J. Symons, appointed for the purpose of considering the best methods of recording the Direct Intensity of Solar Radiation.—In their last report the Committee gave a description of a copper inclosure which had been constructed by them. This consisted of a copper cube $3\frac{1}{2}$ inches square outside, the faces of which were $\frac{3}{8}$ of an inch thick. The cube was packed round with felt $\frac{9}{10}$ of an inch thick, and the whole was faced outside with thin polished brass plates. Thermometers were inserted into that side of the cube intended ultimately to face the sun, and into the opposite side, by means of which the temperature of these sides could be accurately determined. Finally, a thermometer was placed in the vacant space in the very centre of the inclosure. This last thermometer occupies the position that will ultimately be occupied by the internal thermometer, upon which the sun's rays are to fall through a hole; only at this stage the hole had not been constructed. It is obvious that when the instrument is finally in action, with a beam of solar rays (condensed by means of a lens so as to pass through the hole) falling upon the bulb, this thermometer will be subject to a heating effect from two separate causes. (*a*) It will, first of all, be subject to radiation and convection from the surrounding inclosure, which is gradually (let us suppose) getting hot through exposure to the sun. (*b*) It will, secondly, have a beam of solar rays of constant size and of constant intensity (except as to variations arising from atmospheric absorption, seasonal change in the sun's apparent diameter, or change in the sun's intrinsic radiation) continuously thrown upon it through the hole. In fine days when there is no abrupt variation of the sun's intensity the temperature of the internal thermometer will remain sensibly constant, or at least will only vary slowly with the sun's altitude; and this temperature will be such that the heat lost by radiation and convection from the internal hot thermometer will be equal to the heat which it gains from the sources (*a*) and (*b*), save as to a small correction, calculable from the slow variation of the temperature of the thermometer. Now, our object being to estimate accurately the intensity of source (*b*), we must be able, notwithstanding the gradual heating of the inclosure, to determine how much heat the internal thermometer gains from source (*a*). That is to say, we must be able to tell what would be the temperature of the internal thermometer if the instrument were still made to face the sun, but without any aperture. For the solid angle subtended by the hole at any point of the bulb is so small that we may regard it as a matter of indifference whether there be a hole or not, except as to the admission or exclusion of direct solar radiation. It was suggested by Prof. Stokes that a simple practical method of doing this would be to expose the instrument, without a hole, to an artificial source of heat, such as a fire or a stove, the intensity of which might likewise be made to vary. By this means the conditions of the instrument when facing the sun might be fairly represented. Experiments of this nature were made at Manchester by Mr. Shepherd, acting under the superintendence of Prof. Stewart, and these were reduced by Prof. Stokes. It was ascertained from these experiments that the internal thermometer represented with great exactness the temperature of the cube such as it was $3\frac{1}{2}$ minutes before; in other words, there was a lagging time of the internal thermometer equal to $3\frac{1}{2}$ minutes. We may thus find what would be the reading of the internal thermometer if the balance were perfect between the gain of heat by direct solar radiation and the loss of heat by communication to the environment; and as the latter is approximately proportional to the difference of temperature of the envelope and internal thermometer, and the deviation from exact proportionality admits of determination by laboratory experiments, we have the means of measuring the former. We must bear in mind that the lagging time of the final thermometer may be different from that of the thermometer with which the experiments were made. It was likewise ascertained that the difference between the temperature of the internal thermometer and that of the case need not exceed 20° Fahr., and that a comparatively small lens and hole would suffice for obtaining this result. In consequence of this preliminary information, we have made the following additions to the instrument described in our last report:—(1) We have had it swung like the ordinary actinometers with a motion in altitude and azimuth, and with two moderately delicate adjusting-screws, one for azimuth and another for altitude adjustments. (2) We have had a thermometer centrically placed in the interior. The graduation of the stem is very delicate, and extends from 20° to 120° Fahr., the reading being taken from one of the sides. The bulb is of green flint, and the stem of colourless glass. (3) We have also had a small plate of quartz cut and polished and mounted so as to cover the hole, and to be easily removed and replaced. The object of the plate is to prevent irregularities arising from irregular issue of heated air through the hole, entrance of cooler air blown in by wind, &c., and the choice of material was influenced by the wish to permit of frequent cleaning without risk of alteration by scratching. We ought to mention that as it would be difficult to procure the loan of a good heliostat, and expensive to make, we resolved that in the preliminary experiments the adjustments to keep the sun's image on the hole should be made by the observer. Hence the necessity for the adjusting-screws already described.

The Committee suggest that they should be reappointed, and that the sum of £10 be placed at their disposal to defray the expenses of further experiments connected with the instrument.

Report of the Electrical Standards Committee, consisting of Prof. G. Carey Foster, Sir W. Thomson, Prof. Aryton, Prof. J. Perry, Prof. W. G. Adams, Lord Rayleigh, Prof. O. J. Lodge, Dr. John Hopkinson, Dr. A. Muirhead, Mr. W. H. Preece, Mr. Herbert Taylor, Prof. Everett, Prof. Schuster, Dr. J. A. Fleming, Prof. G. F. Fitzgerald, Mr. R. T. Glazebrook (secretary), Prof. J. J. Thomson, Mr. W. N. Shaw, and Mr. J. T. Bottomley.—The Committee was appointed for the purpose of constructing and issuing practical standards for use in electrical measurements. The Committee report that the work of testing resistance coils has been continued at the Cavendish Laboratory, and a table of the values found for ten various coils is given. Of these two coils have been tested before, but, owing to the green coloration mentioned in the last report showing itself in the paraffin, the paraffin was removed and the coils refilled with ozokerit, which can be obtained more nearly free from traces of acid. This change in all cases produced an appreciable increase in resistance. Shortly after the Birmingham meeting of the Association the secretary received a letter from the Board of Trade, inclosing a copy of the general bases of a convention proposed by the French Government for the consideration of the Powers with the object of carrying out the resolution of the Paris Conference with regard to electrical standards. The convention stipulates that a legal character is to be given to (1) the legal ohm, (2) the ampere, (3) the volt, (4) the coulomb, (5) the micro-farad. These questions had been considered by the Committee at the Birmingham meeting, and the following series of resolutions, which the secretary was instructed to forward to the British Government, had been agreed to on the motion of Sir W. Thomson, seconded by Prof. W. G. Adams: (1) to adopt for a term of ten years the legal ohm of the Paris Congress as a legalized standard sufficiently near to the absolute ohm for commercial purposes; (2) that at the end of the ten years period the legal ohm should be defined to a closer approximation to the absolute ohm; (3) that the resolutions of the Paris Congress with respect to the ampere, the volt, the coulomb, and the farad be adopted; (4) that the resistance standards belonging to the Committee of the British Association on Electrical Standards now deposited at the Cavendish Laboratory at Cambridge be accepted as the English legal standards, conformable to the adopted definition of the Paris Congress. During the year the original standards of the Association have again been compared by the secretary. An account of this comparison, and of the very complete one made in the years 1879-81 by Dr. Fleming, the details of which have not been published previously, is given in the appendix. The general result of the comparison is to show that there is no evidence that any of the original coils have changed in value since the year 1876, when they were compared by Prof. Chrystal and Mr. Saunders. The Committee recommend the adoption of the watt as the unit of power. The watt is defined to be the work done per second by the ampere passing between two points between which the difference of electric potential is one volt. The Committee was also of opinion that it was highly desirable to proceed with the construction of an air condenser as a standard of capacity, and for this purpose they desire to be reappointed, with the addition of the name of Mr. Thos. Gray, and a grant of £100.

Report of the Committee on Ben Nevis Meteorological Observations.—The work of the Ben Nevis Observatory for the past year has been carried on by Mr. Omond and his assistants with the same intelligence, enthusiasm, and completeness as in previous years. With the two exceptions of October and November the temperature was every month below its normal. Atmospheric pressure at Fort William was very nearly the normal on the mean of the year, being only 0·012 inch under it. The maximum pressure for the year at the Observatory was 26·093 inch on November 24, and the minimum 23·45 inch on December 8. The maximum temperature for the year was 55°·8 in September, and the lowest 8°·4 in December, thus giving an absolute range of 47°·4. In addition to the regular work of the Observatory, Mr. Omond, superintendent, Mr. Rankin, first assistant, and Mr. Dickson, who has repeatedly relieved the regular observers at the Observatory, are engaged in carrying on original researches. The plotting of the observations of storms made at the sixty-four Scottish lighthouses is now far advanced. The results show a very large number of failures both of storms which have occurred of which no warning has been sent by the Meteorological Office, and of warnings issued with no accompanying or following storm. These failures are at present being investigated by the Ben Nevis observations in connexion with the observations at Fort William and other low-lying stations in Scotland. The directors of the Observatory have from the outset spoken with some earnestness on the absolute necessity of combining the double observation for all forecasting purposes—in other words, of combining the observations at the top of Ben Nevis with those made at the same instant at Fort William. The reason is obvious, it being by vertical gradients, and not by horizontal gradients, that the observations at high-level stations can be turned to their proper and fullest account in forecasting weather. Since none of the sea-level observations at Fort William are in the Meteorological Office, or indeed anywhere but in the office in Edinburgh, the opinion that the Ben Nevis observations are useless in forecasting falls to the ground. A recent discussion in Parliament, already referred to in NATURE, was then alluded to.

In the course of a discussion Prof. Cleveland Abbé said that the problems of meteorology demanded mathematical treatment more and more.

Final Report of the Committee, consisting of Mr. R. H. Scott (secretary), Mr. J. Norman Lockyer, Prof. G. G. Stokes, Prof. Balfour Stewart, and Mr. J. G. Symons, appointed in August 1881, *and reappointed in* 1882–83 *and* 1884 *to co-operate with the Meteorological Society of the Mauritius in the publication of Daily Synoptic Charts of the Indian Ocean for the year* 1861.—Your Committee have to report that the sum of £50 granted in 1881 has now been expended, and they inclose herewith a receipt for the amount, showing its disposition, from the Treasurer of the Mauritius Meteorological Society.

Dr. Meldrum, in a letter to the Secretary, dated June 4, 1887, says: "I am requested by the President and Council of our Meteorological Society to convey to yourself and the British Association their very best thanks, and to say that the Society will forward to the Association, through you, two copies of each of the publications that have been issued."

The following is a list of these publications:—

1. Daily Synoptic Weather Charts of the Indian Ocean for the months of January, February, and March, 1861. The charts for the remaining months of 1861, and remarks to accompany the months already published, are in preparation.

2. Tabular Statements of the number of gales experienced monthly between the parallels of 20° S. and 46° S., and the meridians of 0° and 120° E. during the last 39 years.

Dr. Meldrum further states that the following works are nearly ready for publication:—

I. Synoptic Weather Charts of the Indian Ocean for January 1860, in the course of which month a typical cyclone took place.

II. The Tracks of the Tropical Cyclones in the Indian Ocean, south of the Equator, from 1848 to 1886, as far as is known, together with the observations from which the tracks have been deduced.

III. The Mean Pressure and Temperature of the Indian Ocean for 5° square, in the months of January and July.

IV. Synoptic Charts of the Indian Ocean for each day, during the last 39 years, in which it is known that a cyclone existed.

V. The Average Limits in the Indian Ocean of the South-East Trade in each month, and of the North-West Monsoon from November to May.

Fourth Report of the Committee, consisting of Prof. Balfour Stewart (secretary), Mr. J. Knox Laughton, Mr. G. J. Symons, Mr. R. H. Scott, and Mr. G. Johnstone Stoney, appointed for the purpose of co-operating with Mr. E. J. Lowe in his project of establishing on a permanent and scientific basis a Meteorological Observatory near Chepstow.—This Committee met at 22 Albemarle Street on March 26, and passed the following resolution:—"As your Committee have heard no further results from the action referred to by Mr. Lowe in his letter quoted in their last report, and there thus appears to be an absence of local support, they see no prospect of the scheme ever being carried out. The fundamental idea presiding over the establishment of the observatory was that it should be one of permanence, and hence it is obvious that adequate endowment is essential. To provide this, and properly equip the observatory, several thousand pounds are needed; but the Committee have no assurance that anything at all approaching the necessary amount has yet been subscribed or even promised. As they have now been in existence for between three and four years with this negative result, they are of opinion that the Committee should now be dissolved." In consequence of this resolution the Committee have not drawn the £20 voted at Birmingham, and they do not now request their reappointment.

Report of the Committee on Tidal Observations in Canada.—In the absence of Prof. Johnson, Mr. Robert E. Baynes presented this report. He said that no grant had yet been obtained from the Dominion Government, for though the Hudson Bay Expedition was ended, the Canadian Government had undertaken to pay half the expenses of the re-survey of the Gulf of St. Lawrence. This survey would probably take two years, but when it was concluded there was the greatest possible expectation that a special grant might be given to the Committee. In the meantime, Lieut. Gordon, commanding one of the Dominion cruisers, had been ordered to make certain preliminary observations.

Report of the Committee on Magnetic Observations.—The Committee had met at various intervals during the year. The subject which chiefly occupied them at present was the diurnal variation of terrestrial magnetism and the reduction of the observations. The great difficulty of the Committee was the want of proper observations in the southern hemisphere. The observations which had been made went to show that the two hemispheres were pretty well symmetrical, and at present the Committee had to take for granted that it was so. They hoped in another year to be able to give a more complete report, and some definite results.

Report of the Committee on Standards of Light.—The Committee have compared the standards hitherto proposed, but have not done much. Prof. Adams has, however, presented a report of some experiments, and the Committee think that if funds are provided they will be able to settle the question of standards.

Report of the Committee on Differential Gravity Meters.—Since last report the Committee have received from Mr. Boys an account of experiments in which he is engaged. They await the result of those experiments before proceeding with the construction of an instrument.

Report of the Committee on the Translation of Foreign Scientific Memoirs.—In reply to a communication from the Committee to the Royal Society, Prof. Reinold has informed them that the Royal Society is not at present able to undertake the publication of foreign memoirs in a systematic manner, but anything of special interest would be attended to.

NOTES.

WE learn that the Government of Jamaica offers a premium of £100 for the production of the best practical elementary text-book of tropical agriculture specially applicable to Jamaica, and embodying the first principles of agriculture. It is stated that the object of the manual is to create in the mind of the young an early and intelligent interest in the soil and its products, and particular attention is to be paid to simplicity, brevity, and freedom, as far as possible, from technical terms. It is stated that the propagation and cultivation of tropical economic plants should have due prominence. Manuscripts are to be forwarded to the Government of Jamaica on or before August 1, 1888.

THE Iron and Steel Institute held their autumn meeting at Manchester last week. It was an entire success both as regards the papers and discussions and the excursions to industrial works and places of interest in the neighbourhood. We shall give a report of the proceedings in our next week's issue.

ONE point which seems to be determined by the news which has just reached Zanzibar concerning Emin Pasha is that Albert Nyanza and Muta Nzige are two distinct lakes, a point which has hitherto been doubtful. It is stated that in the recent campaign between Mwanga, King of Uganda, and his neighbours the whole country between these two lakes has been laid waste. Doubtless we shall soon have full details as to this, as well as to the results of the recent explorations, from Emin Pasha himself.

MR. RICHARD QUAIN, F.R.S., Surgeon Extraordinary to the Queen, died on Thursday at his residence, 32 Cavendish Square, at the age of eighty-seven. He began his career in 1828, and speedily rose to high distinction. He wrote many books on medical subjects, such as "Anatomy of the Arteries of the Human Body," and was Honorary Fellow of the Medical and Surgical Society of Edinburgh, Emeritus Professor of Clinical Surgery in University College, Consulting Surgeon at University College Hospital, and President of the Royal College of Surgeons.

THE Annual Congress of the Sanitary Institute of Great Britain was opened on Tuesday at Bolton. Lord Basing delivered the Presidential Address, in which he reviewed what has been done for the protection of public health since the importance of the question was brought home to the minds of legislators. An exhibition of sanitary appliances and apparatus was opened at the same time in the Drill Hall at Bolton.

THE University College of Bristol has recently been enabled by the generosity of local firms to make a notable advance in the matter of engineering education. At a meeting held in the early part of the present year the desirability of instituting engineering scholarships was considered. The practical result of this meeting was that most of the firms of the neighbourhood agreed to institute bursaries, or scholarships, at their works. The holders of these are to be nominated by the College authorities. Some will be awarded on the results of the annual examinations, while others will be reserved for deserving students who may be unable to pay the usual premiums required on entrance into works. The educational scheme adopted at Bristol does not include any attempt to impart practical workshop instruction within the College walls, but the students will spend six months (April to October) in each year acquiring practical experience in the works and drawing offices of the engineers of the west of England. This system is found to answer so well that Messrs. Stothert and Pitt, of Bath, and the Bristol Wagon Works Company propose to make it obligatory on all their pupils to attend the College courses in the winter months for the first three years of their pupilage. Several firms have also signified their willingness to take College students for short periods, so that civil engineering and electrical engineering pupils may spend one or two terms of six months in works, while at the same time mechanical engineers may have experience in two or three different establishments during their College career. In return for these concessions the Council of the College has decided to permit deserving apprentices or artisans, nominated by the local engineers, to attend the College courses at reduced rates. It is expected that about nine first-class scholarships, and a larger number of second-class ones, will be available during the coming session.

MESSRS. CROSBY LOCKWOOD AND CO. will publish during the forthcoming season the following scientific and technical works:—"Flour Manufacture: a Treatise on Milling Science and Practice," by Frederick Kick, translated by H. H. P. Powles, illustrated; "A Dictionary of Terms used in the Practice of Mechanical Engineering"; "Practical Surveying," by George Wm. Usill; "The Mechanical Engineer's Office Book," by Nelson Foley (second edition); "British Mining: a Treatise on the History, Discovery, Practical Development, and Future Prospects of the Metalliferous Mines in the United Kingdom," by Robert Hunt, F.R.S. (second edition); "The Watchmaker's Hand-book," from the French of Claudius Saunier, translated and enlarged by Julien Tripplin; "Our Granite Industries," by Geo. F. Harris; "Marble and Marble-Workers," by Arthur Lee; "Tables, Memoranda, and Calculated Results for Mechanics, Engineers, Architects, Builders, Surveyors, &c.," by Francis Smith (fourth edition). Also the following new volumes in Lockwood's Series of Handy-books for Handicrafts: "The Mechanic's Workshop Handy-book," "The Model Engineer's Handy-book," "The Cabinet-Worker's Handy-book," "The Clock-Jobber's Handy-book," all by Paul N. Hasluck. Also the following new editions in Weale's Rudimentary Scientific Series: "A Treatise on Mathematical Instruments," by J. F. Heather; "The Mineral Surveyor's and Valuer's Complete Guide," by Wm. Lintern (second edition).

THE English edition of Naegeli and Schwendener's treatise on "The Microscope," by Mr. Frank Crisp and Mr. J. Mayall, will be published shortly by Messrs. Swan Sonnenschein and Co. The book was first sent to press in the autumn of 1878, was printed by April 1883, and was then entirely burnt in a fire at the printers'. It has since been revised and again printed, and will at length be in the hands of the public.

THE Syndics of the Cambridge University Press will publish early in October two works on "Elementary Chemistry." One, intended as a companion to lecture-work, is by Mr. Pattison Muir and Dr. Charles Slater; the other, intended to be used along with the book already mentioned, is a course of laboratory work by Mr. Pattison Muir and Mr. Carnegie. Both books deal with the subject of elementary chemistry in a manner somewhat different from that usually adopted in text-books.

THE International Shorthand Congress will meet in the Geological Museum, Jermyn Street, on September 26 and five following days. The inaugural address will be delivered by the Earl of Rosebery, and various papers on subjects connected with shorthand will be read. Men of science, like every other class of the community, are under a debt of gratitude to those who exercise the art of shorthand writing, and will wish the organizers of the Congress every success. We are glad to observe that one of the papers to be read is on the subject of shorthand in education, for the art is unquestionably an invaluable adjunct to any system of education, and is so useful, especially to those engaged in scientific pursuits, that it should be one of the subjects which every youth destined for a scientific career should acquire.

WE have received from the Essex Institute of Salem one of its occasional papers, describing a collection of Japanese pottery made by Prof. Morse, Director of the Peabody Academy of Science, Salem. The author is Mr. Sylvester Baxter, and his description is the first authorized account of the collection. An exhaustive work on the subject by Prof. Morse himself is preparing for publication.

Science states that in order to expedite the publication of short articles upon astronomical and meteorological subjects which may be prepared at Harvard College Observatory, it has been decided to print them as successive numbers of a series, which will constitute the eighteenth volume of the "Annals of the Observatory" when a sufficient amount of material has thus been collected. Each number will be published and distributed soon after it has been prepared.

DURING this month will appear, under the editorship of Dr. G. H. Rohé, a quarterly journal, the *Climatologist*, devoted to the consideration of questions in the domain of medical and sanitary climatology. As there is at present, says *Science*, no other journal in the world exclusively occupying this special field, the editor and publishers believe that there is room for such a publication. Each number will contain forty-eight quarto pages of reading-matter, the subscription price will be fifty cents per year, and the place of publication, S. E. Cor. Baltimore and South Streets, Baltimore, Md.

A SERIES of new salts, remarkable alike for their crystalline beauty and explosive proclivities, has recently been prepared by M. Klobb, of Nancy (*Ann. de Chimie et de Physique*, September 1887, p. 5). These salts, which contain at the same time groups of such opposite properties as ammonia and permanganic acid, are generally obtained by the addition of cold solutions of potassium permanganate to ammoniacal solutions of certain metallic salts; for example, with silver nitrate the compound $AgMnO_4 . 2NH_3$ is obtained as a crystalline dark-violet powder, decomposing on warming, with detonation. The salts of copper, cadmium, nickel, and zinc give analogous compounds, but it is around the salts of cobalt that the interest mainly concentrates. The ordinary simple salts of cobalt only yield compounds which are immediately oxidized, but the ammonio-cobalt salts, and especially the more stable ones known as luteo-cobalt salts, form the most interesting of the series. Luteo-cobalt permanganate, $(Co_2 . 12NH_3)6MnO_4$, is prepared by mixing concentrated solutions of luteo-cobalt chloride, $Co_2 . 12NH_3 . Cl_6$, and potassium permanganate in the proportion of one to twelve molecules, at a temperature not exceeding 60°; on cooling, the salt separates out in little black octahedra or pyramid-capped prisms belonging to the quadratic system, and exhibiting a fine lustre. If the carefully powdered crystals be warmed in a tube, they suddenly decompose with incandescence, and if the same warming operation be performed upon the crystals themselves, the instantaneous incandescence is accompanied by a loud detonation, the tube being shattered into fragments. If a crystal be struck with a hammer, a violent detonation is again the result, even powdering of the crystals in a mortar being accompanied by dangerous decrepitations. Compounds in which hydrochloric and hydrobromic acids partially replace the manganic acid have also been prepared, together with a most lovely salt of the composition $(Co_2 . 12NH_3)4MNO_4 . Cl_2$—$2KCl$, which forms dark-violet hexagonal plates, sometimes bearing low six-sided pyramids, and frequently grouped together in the form of six-rayed stars resembling the forms of snow-flakes. All these salts are of a more or less explosive character, but the luteo-cobalt permanganate itself is by far the most violent.

DURING last summer the Hydrographical Survey Office of Norway effected a series of soundings along the north-west coast of that country. The results have just been published. The Islands of Værö and Röst, at the extremity of the Lofodden group, were surveyed; and here the end of the great fishing-bank projecting from these islands appears to have been discovered. About 5 miles west of these islands a depth of 50 fathoms was found, the bottom being sand. Inside this the bottom gradually becomes more shallow, with occasional "skaller" or mounds. Outside the 50-fathom line the bottom gradually slopes, until about 50 miles west of Röst the depth is 100 fathoms. Here this depth runs in a line nearly north to south for a distance of about 60 miles, viz. from the southernmost islet by Röst to the latitude of Moskenæsö, one of the Lofodden Islands. South of the former islands the bank trends eastwards for about 6 miles, and, north of the latter, westwards for about 35 to 40 miles, when it again trends east and north-east. Inside the line indicated the bottom is everywhere fine sand mixed with pebbles and remains of shells. Some 75 miles west of Röst the depth was 150 fathoms, but on approaching this depth the bottom becomes clayey. Here the edge of the bank was struck, the depth oceanwards rapidly falling from 150 to 300 fathoms. About 85 miles west of Skomvær a depth of 438 fathoms was struck, the bottom being clay, and a sounding from the series of the Norwegian North Atlantic Expedition taken 5 miles further out shows a depth of 593 fathoms with similar bottom. The lines for the 150, 200, 250, 300, and 350 fathom depths seem to run nearly parallel; but as they approach closer to the 100-fathom line of depth northwards, the bank apparently falls more abruptly into the ocean in this direction. This is borne out by former soundings along the coast of the Lofodden and Vesteraalen groups of islands. Thus outside the Islands of Langö, Andö, and Senjen, the edge of the bank will probably be found only 20 miles from the shore, whilst north of the latter island we know it sheers rapidly straight northwards from the shore. A provisional map, scale 1 : 200,000, of the districts sounded has been prepared. The discovery of the limits of this bank will, it is believed, be of great importance to the Norwegian fisheries, as it is the spawning-ground of the herring and cod which descend every year in immense shoals from the North Atlantic.

THE Report of the Trustees of the Australian Museum of Sydney for the past year shows progress in most directions. The number of visitors has increased, the collections are increasing rapidly, especially in the natural history departments, and the building is increasing in size, and is still too small. Catalogues of Australian zoology are in course of preparation, and amongst the new publications which will shortly be issued is a catalogue of shells, one of eggs, one of sponges and Medusæ, and one of Australian birds. The Trustees also append a Report from the Committee of Management of the Technological, Industrial, and Sanitary Museum, which, like so many other institutions of the same character, suffers sorely from want of adequate space. "The Curator reports that the crowded state of the Museum is inconvenient to visitors, and that, apart from locomotion having become difficult, it is now impossible for a teacher or a parent to gather young people around a show-case for purposes of instruction." We are accustomed in this crowded country to limited space and difficult locomotion, but what have they to do with such things in boundless Australia? The specimens are increasing with great rapidity owing to many valuable donations, which is all the more reason why the Museum should be properly housed.

THE last number (No. 28, vol. xii.) of the *Excursions et Reconnaissances* of Saigon contains the conclusion of Père Azemar's elaborate paper on the Stieng tribe, which was commenced in No. 27. It describes the forays, dress, ornaments, manners, religion, houses, intoxicating beverage, food, hunting, and industry of the Stiengs. The writer's knowledge of the tribe may be judged from the circumstance that he has resided amongst them as a missionary, as one of themselves, for five years. The greater part of the number is occupied with the second portion of his dictionary of the Stieng language. The letters H to V occupy nearly a hundred pages in double columns.

WE have received copies of two papers read by Mr. H. C. Russell before the Royal Society of New South Wales—one on floods in Lake George, the other on the history of floods in the Darling River—both being accompanied by excellent maps. Mr. Russell's object is to produce all the historical facts accessible to him relating to these floods, with the dates. He believes that there is a cycle of nineteen years in the occurrence of the floods.

THE Proceedings of the Liverpool Naturalists' Field Club for the year 1886–87 is largely occupied by a third "Appendix to the Flora of Liverpool," by Mr. Robert Brown. The second Appendix was published as far back as 1875, and during these twelve years much additional information has been gathered respecting the distribution of plants within the district of the Field Club. In Mr. Brown's present list special reference is made to about 168 species, while some species new to the neighbourhood and new localities are mentioned.

THE additions to the Zoological Society's Gardens during the past week include a White-crowned Mangabey (*Cercocebus æthiops*) from West Africa, presented by Mr. C. Washington Eves; two Vervet Monkeys (*Cercopithecus lalandii*) from South Africa, presented by Capt. Archibald Douglas, R.N.; a Bonnet Monkey (*Macacus sinicus*) from India, presented by Mrs. La Primandage; a Brown Capuchin (*Cebus fatuellus*) from Guiana, presented by Mr. W. R. Sheppard; a Sharp-nosed Crocodile (*Crocodilus acutus*) from Central America, presented by Mr. E. H. Blomefield; a Mississippi Alligator (*Alligator mississippiensis*) from Florida, presented by Mr. William J. Craig; four Common Chameleons (*Chamæleon vulgaris*) from North Africa, presented by Mr. H. Thornton; six Aurora Snakes (*Lamprophis aurora*) from South Africa, presented by Mr. Walter K. Sibley; a Raven (*Corvus corax*), British, deposited; a —— Ichneumon (*Urva cancrivora*) from Nepal, two Tesselated Snakes (*Tropidonotus tesselatus*), four Dark-green Snakes (*Zamenis atrovirens*), seven Common Snakes (*Tropidonotus natrix*, var.), South European, purchased.

OUR ASTRONOMICAL COLUMN.

NEW VARIABLE.—Prof. Lewis Boss, in *Gould's Astronomical Journal*, No. 160, draws attention to the star DM. + 3° No. 766. Its magnitude in the DM. is given as 9·2m., and Argelander, who observed it twice with the Bonn meridian circle, gave it the same magnitude in the "Bonner Beobachtungen." Prof. Boss, however, was unable to find it with the Albany meridian circle in 1880 and 1881, but has since picked it up with the 13-inch equatorial of the Observatory as an 11·5m. star. It would therefore appear to be either a "temporary" star or a variable of long period.

THE DEARBORN OBSERVATORY.—The Report of the Director of the Dearborn Observatory recently issued is for the two years ending May 10, 1887. Prof. Hough's principal work is that with the great 18½-inch equatorial, and includes observations of difficult double stars and of Jupiter. During the period to which the Report refers 130 new double stars have been discovered and measured. Of these, 45 have a distance less than 0″·5, 11 have a distance between 0″·5 and 1″·0, and the remainder belong to the class of stars having very minute companions. The companion to Sirius has been measured in 1886 and also in 1887. The planet Jupiter has been systematically observed with reference to the physical phenomena on his surface, special attention having been paid, as in former years, to the great red spot. With regard to this remarkable object, Prof. Hough reports that in outline, shape, and size it has remained without material change since the year 1879. During 1885 the middle of the spot was very much paler in colour than the margins, causing it to appear as an elliptical ring. This ring-form has continued up to the present time, although during the last three years the spot has at times been so faint as to be scarcely visible. Four sketches of the planet made in 1886 are given in the Report. The appendices to the Report contain: a catalogue of 209 new double stars, and a description of a printing chronograph, by Prof. Hough; nebulæ found at the Dearborn Observatory 1866–68, by Prof. Safford; orbit of the Clark companion of Sirius, and motion of the lunar apsides, by Mr. Colbert. The last-named paper is of a "paradoxical" character, and we much regret that the Directors of the Chicago Astronomical Society should have recommended its publication.

THE SPECTRA OF HYDROGEN, OXYGEN, AND WATER VAPOUR.—Prof. Grünwald, of Prague, has recently published (*Astr. Nachr.* 2797), a brief account of a theory respecting the relationship of the spectra of gases and their compounds, which, if it should prove well founded, will be of the highest importance in the light it promises to throw on the structure of many of those substances we now call "elements." The fundamental idea is as follows:—Let $[a]$ be the volume occupied by a primary chemical element, a, in the unit of volume of a gaseous substance, A. Let A be chemically combined with a second gaseous body, B, to form a third, C. The element a now takes the form a' and the volume [a']. Then the wave-lengths, λ, of the lines in the spectrum of A, which belong to a, are to the wave-lengths, λ', of the lines in the spectrum of C, which belong to a', as [a] is to [a']. If there be no condensation the lines are the same as to their position, since the volume remains constant, though their relative intensities may vary greatly; the compounds of hydrogen with chlorine, bromine, and iodine may be cited as examples. Assuming this principle, the spectra of hydrogen and water vapour offer some very interesting relationships. Thus, the wave-lengths of the second spectrum of hydrogen, which seems to belong to a molecule, H′, of a more complicated structure, when divided by 2 give the wave-lengths of the lines of water vapour, the volume of the free molecule H′ being double that which hydrogen occupies in water vapour. The wave-lengths of the elementary spectrum of hydrogen can be arranged into two groups, *a* and *b*, which give the lines of the water vapour spectrum when they are respectively multiplied by $\frac{19}{30}$ and by $\frac{4}{5}$. From this Prof. Grünwald concludes that hydrogen is composed of the combination of four volumes of the element *a* with one of the element *b*. The first element, *a*, should be the lightest of all the gases, and much lighter than hydrogen; and since it should therefore probably enter largely into the constitution of the corona, Prof. Grünwald gives it the name of "coronium." The D_3 or "helium" line is found in the spectrum of the second element, *b*; and the Professor therefore gives *b* the title "helium." The correspondences between the wave-lengths calculated by Prof. Grünwald for the elements *a* and *b* and those of lines actually observed in the spectrum of the sun are certainly striking. Following out the same method, the Professor finds the chemical formula of oxygen as follows—

$$O = H'O' = H'[b_4O''_5] = H'[b_4(b_4c_5)_5].$$

The line of the corona, 1474 K, should belong to the element "coronium," and would correspond—$5316 \times \frac{2}{3} = 3544$—to a line, as yet unknown, of the elementary spectrum of hydrogen, with wave-length 3544. Prof. Grünwald had hoped that the late eclipse would have afforded an opportunity of searching for this line. It is clear that the dissociation of hydrogen in the sun is a necessary consequence of this theory, since its two constituent elements will thus both be in the free state in the solar atmosphere.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *SEPTEMBER* 25—*OCTOBER* 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 September 25

Sun rises, 5h. 52m.; souths, 11h. 51m. 42·7s.; sets, 17h. 51m.; decl. on meridian, 0° 50′ S.: Sidereal Time at Sunset, 18h. 8m.

Moon (one day after First Quarter) rises, 14h. 54m.; souths, 19h. 14m.; sets, 23h. 36m.; decl. on meridian, 19° 26′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury ...	... 6 57	... 12 34	... 18 11	... 5 6 S.
Venus ...	... 5 43	... 11 16	... 16 49	... 6 1 S.
Mars ...	... 1 38	... 9 9	... 16 40	... 16 30 N.
Jupiter...	... 9 7	... 14 3	... 18 59	... 12 56 S.
Saturn...	... 0 20	... 8 10	... 16 0	... 19 30 N.

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. ° °
25	... *f* Sagittarii	... 5	... 23 27	... 0 30*	... 125 330
26	... B.A.C. 7053	... 5½	... 17 35	... 18 55	... 73 270
26	... *o* Capricorni	... 5½	... 17 36	... 18 55	... 73 269
27	... *v* Capricorni	... 5½	... 0 1	... 0 48	... 93 7
28	... 42 Aquarii	... 6	... 22 14	... 23 1	... 65 1

* Occurs on the following morning.

Meteor-Showers.

	R.A.	Decl.	
Near δ Aurigæ	78°	... 57° N.	... Swift.
From Lynx	105	... 51 N.	... Very swift.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	... 81 16 N.	... Sept. 28,	5 34	*m*
R Ceti	2 20·3	... 0 41 S.	... ,, 28,		*M*
Algol	3 0·8	... 40 31 N.	... Oct. 1,	4 1	*m*
λ Tauri...	3 54·4	... 12 10 N.	... Sept. 26,	22 36	*m*
			,, 30,	21 28	*m*
R Boötis	14 32·2	... 27 14 N.	... ,, 28,		*M*
δ Libræ	14 54·9	... 8 4 S.	... ,, 26,	3 13	*m*
U Coronæ	15 13·6	... 32 4 N.	... ,, 29,	21 59	*m*
R Scorpii	16 10·9	... 22 40 S.	... ,, 28,		*M*
U Ophiuchi... ...	17 10·8	... 1 20 N.	... ,, 26,	4 37	*m*
			and at intervals of	20 8	
X Sagittarii... ...	17 40·5	... 27 47 S.	... Sept. 28,	23 0	*m*
			Oct. 1,	20 0	*M*
W Sagittarii ...	17 57·8	... 29 35 S.	... ,, 1,	19 0	*m*
β Lyræ...	18 45·9	... 33 14 N.	... Sept. 25,	4 0	m_2
R Lyræ	18 51·9	... 43 48 N.	... Oct. 1,		*m*
S Vulpeculæ ...	19 43·8	... 27 0 N.	... Sept. 30,		*M*
η Aquilæ	19 46·7	... 0 43 N.	... ,, 26,	3 0	*m*
S Sagittæ	19 50·9	... 16 20 N.	... ,, 25,	3 0	*m*
			,, 28,	3 0	*M*
R Vulpeculæ ...	20 59·4	... 23 22 N.	... ,, 30,		*m*
δ Cephei	22 25·0	... 57 50 N.	... ,, 28,	5 0	*M*
			Oct. 1,	23 0	*m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

THE UNWRITTEN CHAPTER ON GOLF.[1]

THERE are two ways of dealing with a difficulty—the metaphysical and the scientific way. The first is very simple and expeditious—it consists merely in giving the Unknown a name whereby it may be classified and categorized. Thenceforward the Unknown is regarded as having become part of knowledge. The scientific man goes further, and endeavours to find what lies concealed under the name. If it were possible for a metaphysician to be a golfer, he might perhaps occasionally notice that his ball, instead of moving forward in a vertical plane (like the generality of projectiles, such as brickbats and cricket-balls), skewed away gradually to the right. If he did notice it, his methods would naturally lead him to content himself with his caddie's remark—"Ye heeled that yin," or, "Ye jist slicet it" (we here suppose the metaphysician to be right-handed, as the sequel will show). But a scientific man is not to be put off with such flimsy verbiage as this. He *must* know more. What is "heeling," what is "slicing," and why would either operation (if it could be thoroughly carried out) send a ball as if to cover-point, thence to long slip, and finally behind back-stop? These, as Falstaff said, are "questions to be asked."

As the most excellent set of teeth, if but one incisor be wanting, gives pain rather than pleasure to the beholder; so is it with the works of the magnificent Clark, the sardonic Hutchinson, and the abstruse Simpson. These profess to treat of golf in theory as well as in practice. But in each a chapter is wanting, that which ought to deal with "slicing," "heeling," "toeing," "topping," &c., not as metaphysical abstractions enshrined in homely though unpleasant words, but as orderly (or disorderly) events due to physical causes and capable of receiving a physical explanation. Mayhap, with the aid of scissors and paste, some keen votary of the glorious game will employ this humble newspaper column to stop, however imperfectly and temporarily, the glaring gap which yawns in the work of every one of its exponents! If so, this scrap will not have been written in vain. It may even, in the dim future, lead some athletic pundit to elaborate *The Unwritten Chapter*.

Every one has heard of the uncertain flight of the projectile from Brown Bess, or from the old smooth-bore 32-pounders, and of the introduction of rifling to insure steadiness. Now, all that rifling secures is that the ball shall rotate about an axis nearly in its line of flight, instead of rotating (as the old smooth-bore projectiles did) about an axis whose direction is determined by one or more of a number of trivial circumstances whose effects cannot be calculated, barely even foreseen. Thus it appears that every deviation of a spherical projectile from its line of flight (excluding, of course, that due to gravity) is produced by rotation about an axis perpendicular to the line of flight.

[1] From *The Scotsman*, August 31, 1887.

This question was very skilfully treated by Magnus in 1852. He showed by experiment that, when a rotating sphere is exposed to a current of air whose direction is perpendicular to the axis of rotation, the side of the sphere which is advancing to meet the current is subject to greater pressure than is that which is moving in the direction of the current. This difference of pressures tends to make the sphere move in a direction perpendicular at once to the current and to the axis of rotation—the direction, in fact, in which the part of the sphere facing the current is being displaced. But it is a matter of no consequence whether the current of air comes against the sphere, or the sphere moves in the opposite direction (and with the same speed) through still air. Hence Magnus's experimental result amounts to this:—*If a spherical ball be rotating, and at the same time advancing in still air, it will deviate from a straight path in the same direction as that in which its front side is being carried by the rotation.*

The physical explanation of the difference of pressures in question requires analysis which would be altogether out of place in an article like this. But, even without it, we feel ourselves to be on perfectly safe ground when we recollect that Magnus's result was obtained by direct experiment, and therefore expresses a physical truth.

Bearing in mind the statement italicized above, let us now consider the anomalous behaviour of a golf ball. The key of the position is "slicing." He who understands this will, without much further trouble, master the rest of the difficulties above referred to. Slicing is effected by the player's drawing the club towards his body while it is in the act of striking the ball. The ball is thus treated almost precisely as is a whipping-top—*i.e.* it is not merely driven forwards, but is made to spin about a nearly vertical axis. The side of the ball to which the club was applied was drawn in towards the player. Hence, as the ball advances, its front is moving towards the player's right, and the deviation takes place to that side accordingly.

A "topped" ball "dooks" (*i.e.* plunges, as it were, headlong downwards). We can see at once that it should be so, in accordance with the general statement. For, in topping, the upper part of the ball is made to move forward faster than does the centre, consequently the front of the ball descends, in virtue of the rotation, and the ball itself skews in that direction.

When a ball is "under-cut" it gets the opposite spin to the last, and, in consequence, it tends to deviate upwards instead of downwards. The upward tendency often makes the path of a ball (for a part of its course) concave upwards in spite of the effects of gravity. This is usually regarded as a very strange phenomenon, even by men to whom "dooking" seems natural enough. As will be seen later, a "jerked" ball must, from the way in which the face of the club is moving at impact, have this spin, and consequently must skew upwards.

Since a "heeled" ball deviates to the right as a "sliced" ball does, it must be rotating in a similar manner. But a "toed" ball deviates to the left, and must, therefore, have the opposite spin. The way in which the spin is produced in these cases is not so easy to explain as it was in the case of topping. We may begin, however, by saying that the terms "heeling" and "toeing" are entirely misleading, if they be taken to imply necessarily the hitting of the ball with the heel or the toe of the club as the case may be. For, as will soon appear, a ball may be heeled off the toe of a club, or toed off the heel, at pleasure! And when a man holds his club properly, so that in the act of striking the ball *the club-head is moving in a direction exactly perpendicular to the face*, there will be neither heeling nor toeing whatever part of the face strikes the ball, provided it be struck by the face proper, and not by an edge. It will not be driven so far by the heel, or by the toe, as by the proper centre of percussion; but there will be no spin, and therefore no skewing.

The true explanation, therefore, of heeling and toeing is to be found in the fact that the club-head, when it strikes the ball, is not moving perpendicularly to the face; or, what comes practically to the same thing, the face of the club is not perpendicular to the direction in which the club is moving (*i.e.* it is to be presumed the direction which it is desired that the ball should take). In this case we may regard the motion of the head as resolved into two parts—one perpendicular to the face, the other parallel to it. The former gives translation only to the ball. The latter gives it not only translation, but rotation also. When the toe of the club is too much thrown back—*i.e.* when the heel is too much forward—the motion parallel to the face is from toe to heel, exactly as in "slicing." "Heeling" and "slicing" are thus practically the same thing, so far at least as the ball is concerned. But, so far as the player is concerned, they are quite different; and (what is of far more importance) the modes of cure are entirely dissimilar. To cure slicing, cease to pull in your arms; to cure heeling, place your club beside the ball as in addressing, and note the lie of the head. If that be incorrect, put it right; if it be correct, the fault lies in "gripping" (instead of holding loosely) with your right hand. Many a man's play has been spoiled for the day by his having applied (too often by his caddie's advice) the cure for "heeling" when the disease was "slicing," or *vice versâ*.

When the toe of the club is turned inwards, the face is pushed tangentially outwards behind the ball, so that the spin and its consequences are exactly the reverse of those just described.

From what has been said above, it is obvious that the flight of a ball, if it be nearly spherical and have its centre of gravity at its centre, depends solely upon the impulse originally given to it. [If the centre of gravity be not in the centre of the ball, it is only by mere chance (in teeing) that the ball escapes having a rapid rotation given to it, even by the most accurate of drivers. Should it fortunately escape initial rotation, still its flight cannot be regular. A simple and exceedingly expeditious test of this defect consists in placing the ball on mercury in a small vessel. If, in that position, it oscillates rapidly about the vertical, it should be at once rejected as absolutely worthless.] This is a point on which opinions of the wildest extravagance are often expressed. Some balls, it is said, "will not fly," &c. How if they were fired from a blunderbuss? Nobody seems to have made the trial in the only reasonable way—viz. by using a cross-bow or a catapult to give the initial speed. With such an instrument two homogeneous spherical balls of equal size and weight, whatever their other peculiarities, would be despatched under exactly the same conditions, and their behaviour could be *compared*—it would not require to be *contrasted*.

But he is correct (in meaning, though not in his English) who says that some balls "won't drive." It is easy to recognize a good ball by trial, but difficult to define one, at least without periphrasis. A good ball is one which acquires, under given conditions of good driving, as great an initial speed as possible, coupled with the minimum of rotation.

So far as we are aware, all direct scientific experiments on elastic resilience have been made at low speeds, and consequently with but slight distortion of the impinging bodies. But the circumstances of a "drive" in golf are of a totally different character; so that the results of the drive must be themselves regarded as the only data of the requisite kind which we possess. In this matter very valuable data (not for golf alone) might easily be obtained by measuring the height to which a ball rebounds when fired from a powerful catapult against a wooden or stone floor; recording on each occasion the extent to which the springs of the weapon were extended, and the appended weight which would produce the same extension. Some keen golfer may thus find thoroughly useful as well as congenial occupation, when his happy hunting-grounds are inches deep in snow. P. G. T.

SCIENTIFIC SERIALS.

Bulletin de l'Académie Royale de Belgique, June.—On the problematic satellite of Venus, by Paul Stroobant. After a complete survey of the various appearances of this object between the years 1645 and 1768, the author discusses the different conjectures advanced by astronomers to explain the phenomenon. The theory of a true satellite is rejected on the ground that no orbit could be made to correspond with all the recorded observations, while the elements calculated by Lambert from some of them would make the planet ten times larger than its actual size. In the same way are disposed of the other suggestions that it might be the reflection of Venus on certain frozen particles in the atmosphere, or an inter-Mercurial planet, or a planet with a revolution slightly differing from that of Venus, or an asteroid, and the like. Several reasons are then advanced in support of the view that the pretended satellite is to be referred to certain small fixed stars near which Venus was passing when the various observations were taken. This explanation is specially obvious in one instance, where the movement attributed to the supposed satellite is precisely the proper motion, but in the opposite direction, of Venus at that moment in relation to the fixed stars.—On a specimen of crystalline iron-glance formed on some old iron weapons, by W. Prinz. An examination of these crystals and of their physical properties, now for the first time detected on some ancient Frankish arms, shows that they are formed of specular iron, and their presence is compared with that of anhydrous ferric oxide in sedimentary deposits of all ages, produced, as on the arms in question, by the moist process at a low temperature. —On the origin of the curative effects of hypnotism, by J. Delbœuf. The author, who is one of the founders of the new branch of the medical art, based on the application of hypnotism to the cure of numerous maladies, here treats the subject as throwing light on the reciprocal action of mind on the body. He believes that there is a great future for hypnotism in the field of therapeutics, and describes in detail some of his own remarkable experiences and successful treatment of hypnotized patients.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 12.—M. Hervé Mangon in the chair.—Experimental researches on the morphology of the muscles, by M. Marey. By comparing the form of the gastrocnemian muscles in the white race with those of the Negro, the author has discovered a fresh example of the harmony that exists between the form and functions of the muscles. His conclusions were confirmed by experiments made on rabbits at the Physiological Station, and a fresh proof is thus afforded of the evolutionist doctrine that the organs tend to adapt themselves to the varying conditions under which their functions are performed. To complete these researches nothing now remains except to bring about variations in the muscular form by changing the outer conditions of locomotion without modifying the anatomical relations of the organs by the direct intervention of surgery, and then ascertain to what extent the modifications thus obtained become fixed by heredity.—Invasions, varying aspects, and intensity of the pestilence in the Caucasus, Persia, Russia, and Turkey, since 1835, by M. J. D. Tholozan. From a careful study of all the circumstances attending the various visitations of the plague in this region since the great epidemic of 1830–35, the author concludes that in the great majority of cases the outbreaks have been of a purely local character, appearing in one or two houses, spreading thence by secondary contagion to others in the village, occasionally also to one or two neighbouring villages, but scarcely ever advancing beyond the district and never sweeping over extensive regions, like the cholera and pest in former times. A remarkable instance was that of Resht in Northern Persia, where it carried off 2000 of the 24,000 inhabitants, lasting altogether over a twelvemonth, during which period the people emigrated freely to the neighbouring towns, which nevertheless remained unaffected despite the absence of prophylactic measures and quarantine regulations. He therefore considers that, without denying the possibility of future widespread diffusions like those of the past, the contagion has now entered a new phase of purely local or isolated development, without any tendency to spread further. The special conditions of its appearance in such places should therefore be studied, just as those, for instance, of typhoid fever are sought and found in the districts where this disorder happens to make its appearance. In Turkey the plague has from time to time acquired a certain intensity, but without ever assuming the deadly character of certain previous outbreaks, except in Mesopotamia in 1873. But in Persia it has often been attended by an excessive mortality, and a very great local development relatively to the actual number of the inhabitants. Its range has been mainly confined to an area stretching for 1700 kilometres from Merv to Bagdad, and for 1760 from Bassora to Astrakan, but within these limits mainly confined to isolated points and never radiating from them to any great distance.—Observations of Olbers' comet (1815 I.) on its return in 1887, made at the Observatory of Bordeaux with the 0·38 m. equatorial by MM. G. Rayet and Courty. The observations cover the time from September 8–10 inclusive, and comprise the mean position of three stars taken as points of comparison.—Observations of Brooks's new comet (August 24, 1887) made at the Observatory of Nice with the 0·38 m. Gautier equatorial, by M. Charlois. The apparent positions are given for the period from August 25 to September 2 inclusive. On the former date the comet had a nucleus of the tenth magnitude surrounded by an elongated nebulosity at the angle of position of 304°.—On the variations of the telluric currents, by M. J. J. Landerer. During the last nine years, the number of days when the current flowed north-east and south-west being indicated by 1, those on which it flowed in the opposite direction will be represented by 6·7. Several changes of direction very seldom occurred on the same day, and they were nearly always connected with violent atmospheric disturbances. From 8 a.m. to 9 p.m. the intensity of the current going north-eastwards attained a maximum towards 10 o'clock and two minima about 4 and 9 o'clock, the mean intensity of the maximum being 0·000124 ampere, that of the minima 0·000073 and 0·000074. For the opposite current this maximum and these minima become respectively one minimum and two maxima at about the same hours, with mean intensities 0·00064, 0·000122 and 0·000138 ampere.—Formation and elimination of ferruginous pigment in poisoning by toluylendiamine, by MM. Engel and Kiener. Having in a previous communication studied the ferruginous residuums of hæmoglobin, which accumulate in certain organs of animals poisoned with the sulphuret of carbon, the authors here submit the results of similar researches in the case of another substance, toluylendiamine.—Experimental researches in connexion with the physiological action of *Cytisus laburnum*, by MM. J. L. Prevost and Paul Binet. The experiments here described were made on frogs and on warm-blooded animals, such as cats, dogs, rats, guinea-pigs, rabbits, and pigeons, with the general results that Cytisus must be regarded as a good emetic with central action, acting rapidly and better by hypodermic injection than by ingestion.—Note on *Greeneria fuliginea*, by MM. L. Scribner and Pierre Viala. This is a new species of microscopic fungus which has lately made its appearance in North Carolina, where in very hot and moist districts it attacks and destroys in a few days vines that had been spared by the black rot. Its true characters not being yet determined, the fungus must be provisionally included in the numerous class grouped by M. Saccardo under the general name of Sphæropsideæ.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Text-Book of Algebra: W. S. Aldis (Clarendon Press).—Longman's Shilling Geography (Longmans).—Die Bildung des Natronsalpeters: Dr. C. Ochsenius (Stuttgart).—Die Crustaceen der Böhmischen Kreideformation: Dr. Ant. Fritsch und Jas. Kafka (Prag).—Fauna der Gaskohle und der Kalksteine der Permformation Böhmens, Band ii. Heft 1: Dr. Ant. Fritsch (Prag).—Astronomical Revelations (E. Dexter).—Manual of Mineralogy and Petrography, 4th edition: J. D. Dana (Trübner).—Botanische Jahrbücher für Systematik, Pflanzengeschichte und Pflanzengeographie, Neunter Band, 1 Heft: A. Engler (Engelmann, Leipzig).

CONTENTS

THURSDAY, SEPTEMBER 29, 1887.

THE ORIGIN OF THE FITTEST.

The Origin of the Fittest: Essays on Evolution. By E. D. Cope, A.M., Ph.D., &c. (London: Macmillan and Co., 1887.)

CONSIDERING the good work which Prof. Cope has done in the cause of evolution, the present collection of essays appears to us disappointing. Originally published from time to time as independent lectures or articles in journals, they are now republished in the form of a book, apparently without any revision, and certainly without any such revision as would have been required to constitute them a connected treatise. The consequence is that instead of a systematic work on "The Origin of the Fittest," we have a number of disjointed papers bound up together, the larger number of which contain more or less close repetitions of parts of the others—sometimes in the form of long quotations, at other times without special reference. The effect of such frequent reduplications is somewhat tedious, and might easily have been avoided by slightly modifying the constituent essays.

But, apart from the method of compilation, our chief disappointment has reference to some of the leading ideas which characterize the whole series of essays. For example, great store is everywhere set upon the author's doctrine of "growth-force," which so far as we can see is merely an abstraction serving as a shorthand expression of all the phenomena of growth as already known. It is merely a *re-statement* of certain facts, in no way serving to *explain* them. Similarly with the so-called law of acceleration and retardation, which the writer everywhere upholds as a scientific generalization of the highest importance. The idea is that when in any series of generations "growth-force" is accelerated, the organs thus affected will undergo evolution; while, when "growth-force" is retarded, the organs in question will atrophy and disappear. But surely it is hard to see in what other way progressive modification could take place than by one or other of these so-called laws. The laws merely serve to re-state the facts—viz. that organs do evolve and do degenerate.

Another general view which Prof. Cope is fond of frequently insisting upon is that the theory of natural selection does not explain the "*origin* of the fittest" variations, but only the *preservation* of them. This, of course, is an objection to Darwinism which is abundantly familiar in general literature, but we are disappointed to find it so warmly sanctioned by Prof. Cope. In his case, however, Darwinism might reply, "Out of thine own mouth will I judge thee, thou wicked servant." Take, for example, the following passage:—

"Admitting evolution as proved (see Part I.), we perceive that an almost infinite chance exists against any usual amount of variation, as observed, producing a structure which shall be fit to survive in consequence of its superior adaptation to external circumstances. It would be incredible that a blind or undirected variation should not fail in a vast majority of instances to produce a single case of the beautiful adaptation to means and ends which we see so abundantly around us. The amount of attempt, failure, and consequent destruction, would be preposterously large, and in no wise consistent with the facts of teleology as we behold them."

Now the very essence of Darwinism is that, preposterously large as the amount of attempt, failure, and consequent destruction may be if regarded from a teleological point of view, as a matter of fact it does occur in so "vast a majority of instances," that there can be no real question as to its furnishing sufficient material for the mechanical interpretation. Prof. Cope forgets that it is only the lines of fortunate variation (as represented by the successful competitors) that have been allowed to show themselves. And when we calculate the opportunities of favourable variations arising under a geometrical rate of propagation, with "failure and consequent destruction" going on at the rate of thousands—if not of millions—to one, surely we must fail to appreciate the alleged difficulty of explaining the "origin of the fittest." Only if we could suppose that some malign intelligence were always on the look-out for a favourable variation when it does happen to arise, in order to destroy or unfavourably to handicap its chances of survival—only on this supposition of a *super*-natural selection intentionally working against the Darwinian principle could it be said that the facts of organic Nature are not sufficient to justify the Darwinian theory. True enough, "we perceive that an almost infinite chance exists against any usual amount of variation, as observed, producing a structure which shall be fit to survive;" but we likewise perceive that this almost infinite chance is satisfied by "the amount of attempt, failure, and consequent destruction," "which we see so abundantly around us." In short, the doctrine of variation in definite and beneficial lines is incompatible with this large amount of failure and consequent destruction, while the fact of such failure and destruction being everywhere so enormous renders it needless for the Darwinian theory to look further than the chapter of accidents for its "origin of the fittest."

As we have no wish to fall foul of so capable a man of science, we will not pursue further our criticism of his views theoretical and speculative, although there is much else—especially in his long essay on "Metaphysical Evolution"—which appeals to us as sheer nonsense. The writer's strength is in his facts, and in our judgment he would have been wise to have kept within his own province of palæontology. He is full of valuable information upon this important subject, and we may remark that as one result of his studies he gives a very decided opinion upon a matter which has recently been debated in these columns—namely, as to whether or not specific or other typical characters are invariably of adaptive meaning. Premising merely that his opinion has been formed independently, and as a result of his own extensive observations in the sphere of morphological fact, we will conclude by quoting one of the passages in which that opinion is conveyed.

"Another reason why natural selection fails to account for the structures of many organic beings is the fact that in expressing the survival of the fittest it requires that the structures preserved should be especially useful to their possessors. Now, perhaps half of all the peculiarities of the parts of animals (and probably of plants) are of no use to their possessors, or not more useful to them than many other existing structures would have been. . . . Less attention has been directed to the non-adaptive characters, yet they are as numerous as the adaptive. I do not include under this head useless organs or parts only, but also those which are useful, but whose peculiarities do not relate to that use as advantageous to it."

By the last qualification is meant that even useful organs often present peculiarities, which may run through whole orders and classes, and which nevertheless present no utilitarian significance. In the opinion of the present reviewer, the above estimate of the proportion of non-adaptive to adaptive structures is excessive, and some of the instances which are given of the former may be open to question. But even if a Darwinist is not prepared to allow that so many as "one-half of all the peculiarities of the parts of animals (and probably of plants) are of no use to their possessors," he may feel that the mere possibility of such a first-hand observer as Prof. Cope making such a statement is enough to discredit the non-Darwinian assumption of utility as *universal*.

GEORGE J. ROMANES.

THE TEACHING OF GEOGRAPHY.

The Teaching of Geography. By Archibald Geikie, LL.D., F.R.S. (London: Macmillan and Co., 1887.)

EIGHTEEN years ago the Royal Geographical Society instituted its public school medals. That was the beginning of its efforts in the cause of geographical education. We believe that it is to Mr. Francis Galton that the honour of having started this policy is due. For many years he, Mr. Clements Markham, and a few others persisted in the face of cold indifference and with little success. At last, in 1884, it was determined to make a fresh and energetic start. Mr. Keltie was appointed Inspector of Geographical Education, and from the date of the publication of his report progress has been very rapid. It was seen at once that one result of that report would be a large crop of geographical text-books. Of those which have already appeared that of Mr. Chisholm is certainly the best; and although it bears sadly too many of the marks of haste, it is a decided advance on all previous work of the kind in the English language. In Dr. Geikie's little book we have before us the first instalment of a still more ambitious scheme. Fourteen more volumes are contemplated to complete the series to which this is an introduction.

It is needless to say that when it was first rumoured in geographical circles that the author of "The Scenery of Scotland" was engaged on a book on the teaching of geography much was expected. After reading the work carefully through we cannot say that we are disappointed. Except in matters of detail, the only criticism we feel inclined to pass has reference to the title. The author has scarcely been just either to his book or to geography in using the present title. We have before us in fact an admirable essay on certain methods of teaching, but applicable to many subjects, and illustrated by many subjects which even the most grasping geographer would scarcely claim as his. It is surely an abuse of terms to say that we are teaching geography when we are giving lessons on the "house-fly, grasshopper, dragon-fly, wasp, beetle, and butterfly—showing the similarity and diversity of plan in the great class of insects" (pp. 103–4), or learning that "the breast fins in fishes, the wings in birds, the fore-limbs in quadrupeds, and the arms in man are all modifications of the same parts of the vertebrate skeleton." Again, on p. 106, the teacher is advised to insinuate the laws of health into his geographical teaching, and to explain the physiology on which they are based; and on p. 119 to tell of exchange by barter, and of the value of a medium of exchange. There are many similar instances, but we are reluctant to press this kind of criticism too far, for in this book Dr. Geikie renders great services to the causes both of education and of geography.

There are, in truth, at the present time, several great ideas in the air, destined probably to revolutionize education, but as yet hardly differentiated from one another. It is beginning to be generally recognized that geography, so far from being an elementary subject, is one which requires as a basis a great and most varied fund of information. But there is a tendency to throw geography into confusion by including all this within the subject itself. The essence of geography is topography, and its method is the comparative method of Carl Ritter. Its function is to compare localities, to ascertain their relations in space, and to assign causes for those relations and for the similarities and differences of the localities compared—and all this, as Dr. Geikie says, with especial reference to the earth as a dwelling-place for man. But before we can with advantage compare localities, we must know scientifically and by personal experience at least one. To acquire such knowledge is to learn to observe and to reason independently, to learn the use of our eyes and hands, to learn many of the great laws of science and the scientific explanations of a multitude of every-day experiences. In the greater portion of his book Dr. Geikie is occupied in asserting and demonstrating the possibility of laying this foundation in general science outside the laboratory, and by a skilful use merely of the experience of common life, and he does so with signal success. If teachers will study this method, and if examining authorities will cease to thwart their efforts, there may be some chance of removing the book-bias from our teaching, and of making the decisions of our examiners more nearly resemble those of the great world in after life. It is natural that at first these efforts should be made in the name of geography. But a cleavage is already discernible in the geographical confusion. On the one hand, is a science investigating a definite set of relations, but linked in all directions with other subjects, and in that similar to other composite sciences, like geology and anthropology; and, on the other hand, an educational method for the teaching of the rudiments of scientific thought and facts, an excellent foundation equally for scientific specialism and for practical life.

In matters of detail we have found many valuable hints in the last few chapters, which deal with subjects more strictly geographical. The list of books of reference at p. 46, however, hardly rises to our ideal. The list is, of course, intended for general teachers, and not for geographical experts. Bearing the practical requirements of this class in view, we should have preferred fewer books of early, and more of recent, date. The lack of modern authorities on European lands is especially noticeable. We are aware that this is intentional on the part of the author, but we hardly think that he has been well advised. It is among the multitude of modern books of travel, for the most part indifferent or bad, that guidance would be most valuable. H. J. MACKINDER.

OUR BOOK SHELF.

Chemistry and Heat. By R. G. Durrant, M.A., F.C.S. (London: Rivingtons, 1887.)

THIS little book is a collection of laws and definitions connected with chemistry and heat, intended more especially for the use of students preparing for examination in these subjects. It is to be regarded chiefly as a companion to more extensive treatises, and as a substitute for the extracts and notes which the average student would make for himself.

The various laws are stated very clearly, and the examples illustrating them have been happily chosen. The subject of heat is only considered in so far as it enters into chemical work. The laws relating to atomic weights are particularly well arranged, and zinc being taken as a typical element, a whole chapter is devoted to the method of estimating its atomic weight. A chapter is also devoted to the determination of vapour densities by the well-known methods of Dumas, Hofmann, and Victor Meyer, each being illustrated by a numerical example. In all cases the details of the calculations are gone through with great care.

At the end of the book is a table showing the characteristic tests for the more important metals and acids. This is not nearly up to the same standard as the earlier part of the book, but still it will be of service where expedition is of maximum importance. We are afraid, however, that the results obtained by analyses without separations would not always be perfectly trustworthy. We should have expected the author to be aware that separations are indispensable for most examinations in practical chemistry. Several specimens of analyses are given in detail.

We have no doubt that the book will prove a useful addition to the already large family of hand-books prepared for the use of the fortunate student of chemistry.

A. F.

On Overwork and Premature Mental Decay. By C. H. F. Routh, M.D., M.R.C.P. Fourth Edition. (London: Baillière, Tindall, and Cox.)

DR. ROUTH takes a very gloomy view of some of the characteristics of the present age. He holds that insanity and premature mental decay are decidedly on the increase, and this fact he attributes chiefly to overwork. Whether or not he is correct in his interpretation of the statistics relating to insanity, there can be no doubt that overwork is far too common in these days of excessive competition, and Dr. Routh has done good service by showing clearly in this little book the inevitable consequences of any severe and continuous strain upon the powers either of the mind or the body. As to remedies for the evil consequences of overwork, he offers many wise suggestions, and prudent readers will probably be all the more inclined to pay attention to his counsels when they find that he lays stress mainly upon the necessity for periods of rest, for the cultivation of a variety of intellectual interests, and for rigid self-control. Dr. Routh has added to the value of his work by discussing in the present edition the effects of overwork upon women and young persons.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Hall and Knight's "Higher Algebra."

IN your issue of September 1 (p. 409), there appeared a review of our "Higher Algebra," bearing the signature "R. B. H." The work under consideration is a sequel to an elementary text-book, as the title-page asserts; it does not profess to be anything more, and we should have thought that any reader would have recognized that "Higher" and "Elementary" are merely used as relative terms. This point has been practically ignored by your reviewer, and in consequence his remarks contain certain inaccuracies and misconceptions likely to leave a false impression on the mind of the reader.

In the first place, "R. B. H." complains that we postpone to our thirty fourth chapter the discussion of the fundamental laws of algebra, the "remainder theorem," symmetrical expressions, and identities. As a matter of fact, these things do not appear here "for the first time;" propositions scattered over the "Elementary Algebra" are here summarized, and in some cases rediscussed; and, with regard to the "remainder theorem," its proof and easy applications are to be found in the twenty-ninth chapter of the elementary work—a chapter which also contains a section on identities and transformations, especially those which can be treated cyclically. Hence, in criticising the miscellaneous chapter of the more advanced work, it should be remembered that for the most part it merely extends and develops what has elsewhere already been treated far more comprehensively than is usual in an Elementary Algebra.

In the chapter on "Miscellaneous Equations," we are told that "there is no hint or caution given that the root obtained may not satisfy the original equation unless the sign of one or more of the radicals involved in it is changed," and our attention is called to an example on p. 99. Surely "R. B. H." must have read this page somewhat carelessly, for at the head of it there stands a remark containing the required hint, while reference is made to a note in the "Elementary Algebra," in which the point is fully discussed.

The remarks on our chapter on permutations and combinations are somewhat vague and indefinite, and we do not gather which proofs are held to be "very difficult to understand." We have used the methods of this chapter very successfully with boys of ordinary intelligence for twelve years or more, and we have always found the proofs contained in Arts. 141, 152, 153, and 156 far more intelligible than those given in other text-books. The illustration given by "R. B. H." may be useful enough for blackboard work, and it is hardly conceivable that any teacher of experience would attempt to handle this part of algebra without a frequent and ready use of such exemplification; but we fail to see how a *proof* of the general formula for the number of permutations of n things v together could be framed on these lines in a form suitable for a text-book.

To the other points raised by your reviewer we do not reply because they are mostly matters of opinion, and whether we have been judicious in our arrangement and subdivision of the subject is a question which can only be decided by the experience of teachers using our books. But we may perhaps remark that the obvious necessities of a school text-book should not be overlooked. In its present form our "Elementary Algebra" contains upwards of 360 pages, and if we had included all the sections now suggested by "R. B. H." the book would have been increased by 150 pages or more, and would have been practically useless, for it would have been published at a price quite prohibitive for school use.

Our two books have been designed to form one complete treatise, and we venture to think that anyone who will fairly criticise the work as a whole will not find the glaring omissions and "defect of plan" suggested by your reviewer.

H. S. HALL.
S. R. KNIGHT.

PERMIT me to remark that far from "ignoring" Messrs. Hall and Knight's use of the terms "Elementary" and "Higher" for the two parts of their Algebra, I have called special attention to it. It seems to me that the term "Higher Algebra" (*algèbre supérieure*) having been employed by Serret as embracing "the algebraic resolution of equations" in general, and that of "Modern Higher Algebra" by Salmon for what "with greater precision might be called the Algebra of Linear Transformations," it is hardly open to the writers of what is really a text-book of elementary algebra in two parts to apply the same term to the second part of their work, and to object to a gentle protest from a reviewer. I am at a loss to see where in my subsequent remarks I have practically ignored their own use of the terms.

I observed that "the fundamental laws of algebra are for the first time *gathered together* and discussed in the thirty-fourth chapter." This implies that they have *appeared dispersedly* in the book before, but the references in this chapter hardly justify the contention that they have appeared as "*the fundamental laws*." In fact, the distinctive law of ordinary algebra that $ab = ba$, instead of being emphasized, is introduced as a fact "with which the student is familiar in arithmetic" ("Elementary Algebra," Art. 13), and nothing more said, as perhaps at this early stage might be expected. So again the "remainder theorem" does in fact appear in an article, *marked with an asterisk*, at the end of a chapter of "Miscellaneous Theorems" in the "Elementary Algebra," but only as an isolated proposition with a few exemplifications of its use. The point of my remark was that I should have expected such a fundamental theorem to be put in the forefront and largely made use of in the chapter on "Harder Factors." I would suggest this for the consideration of the authors in a future edition.

In like manner, as to my remark on the roots of equations involving radicals, the caution, which I regret that I overlooked in p. 99, appears only as a remark on one particular example, while nothing is said about it in the next, to which it is equally applicable, and in the answers at the end I find roots given which do not satisfy the equations as they stand. It is the practice of not requiring the pupil to select the signs suitable to each root, which I regretted to find that our authors in this way sanction.

Regard for your space prevents my adding more than the single remark that I regret that the practical commercial consideration of the cost of the first part of the book should have necessitated what I have regarded, and what, by urging this plea, the authors seem almost to admit, as, in itself, a "defect of plan." On reconsidering the matter for a future edition, the authors will, I cannot help thinking, find it possible, as it is desirable, to transfer so much from the second to the first part as will make the latter sufficient by itself, as at present it hardly is, for many pupils who need only a small portion of their higher part.

R. B. H.

On the Constant P in Observations of Terrestrial Magnetism.

HAVING been absent from town, I have to-day for the first time seen the letters of Prof. Harkness and Mr. Ellis on the calculation of P.

Though unaware that it was used at Greenwich, or elsewhere, Dr. Thorpe and I have, for a year at least, employed the formula given by Mr. Ellis in the reduction of observations made for the magnetic survey. We have, in fact, made it still more accurate by the addition of another term. Thus, if we write l and l_1 for log A and log A_1, and μ for the modulus, it may easily be shown that—

$$P = \frac{r_1^2 r^2}{r_1^2 - r^2} \times \frac{l - l_1}{\mu} - \frac{r_1^2 r^2 (r_1^2 + r^2)}{2(r_1^2 - r^2)^2}\left(\frac{l - l_1}{\mu}\right)^2 \text{ nearly.}$$

Using the metric system of units and taking as is usual $r = 0{\cdot}3$, $r_1 = 0{\cdot}4$, this becomes—

$$P = 0{\cdot}4737\,(l - l_1) - 1{\cdot}947\,(l - l_1)^2.$$

The value of P for our first year's work calculated by the ordinary method is ·000817. Deduced from the formula given by Mr. Ellis it is ·000824, which the second term given above reduces to ·000818.

In this case the effect of the correction on the value of H is considerably below the error of experiment, but as attention has been drawn to the matter, it may be as well to point out that by means of the second term the accuracy of the approximation can be readily tested without the trouble of calculating P directly.

ARTHUR W. RÜCKER.

September 21.

A Meteor's Flash and Explosion.

AT 8.52 p.m. (Dublin time) of yesterday, Tuesday, September 13, my wife and I while walking home were startled by a sudden bright flash like lightning, but slower and more regular in its movement. Simultaneously an intensely brilliant meteor shot majestically across the sky from north-north-west towards south-south-east, passing near, but to the eastward of, the zenith in its route. It seemed to take its origin from between the Pointers and the constellation Perseus, and died out at a height of 25° or 30° above the horizon.

Precisely three minutes and a half later a dull report was heard, which resembled that of a very distant field-gun, or of a peal of thunder far away, but it did not reverberate as thunder would have done.

It was impossible not to connect the phenomena of the flash and the report with each other. I accordingly made a rough calculation, which gave 43·4 miles as the distance—not necessarily vertical, but absolute—at which the meteor had become incandescent, and exploded, as a result of its collision with the earth's atmosphere.

JOHN WILLIAM MOORE.

40 Fitzwilliam Square West, Dublin, September 14.

A Monstrous Foxglove.

MR. TENNANT in NATURE of September 22 (p. 482), after describing a curiously abnormal specimen of *Digitalis purpurea*, writes to ask if "such monstrous forms are at all usual." Thinking your correspondent may be unacquainted with Mr. Herbert Spencer's "Principles of Biology," I write to draw his attention to p. 226, vol. i. of that work, where, in speaking of some foxgloves growing in Derbyshire, Mr. Spencer says of one :—

"The following are the notes I took of its structure :—First or lowest flower on the stem, very large ; calyx containing eight divisions, one partly transformed into a corolla, and another transformed into a small bud with bract (this bud consisted of a five-cleft calyx, four sessile anthers, a pistil, and a rudimentary corolla) ; the corolla of the main flower, which was complete, contained six stamens, three of them bearing anthers, two others being flattened and coloured, and one rudimentary ; there was no pistil, but, *in place of it*, a large bud, consisting of a three-cleft calyx, of which two divisions were tinted at the ends, an imperfect corolla, marked internally with the usual purple spots and hairs, three anthers sessile on this mal-formed corolla, a pistil, a seed-vessel with ovules, and, growing to it, another bud of which the structure was indistinct. Second flower, large ; calyx of seven divisions, one being transformed into a bud with bract, but much smaller than the other ; corolla large, but cleft along the top ; six stamens with anthers, pistil, and seed-vessel. Third flower, large ; six-cleft calyx, cleft corolla, with six stamens, pistil, and seed-vessel, with a second pistil half unfolded at its apex. Fourth flower, large ; divided along the top, six stamens. Fifth flower, large ; corolla divided into three parts, six stamens. Sixth flower, large ; corolla cleft, calyx six-cleft, the rest of the flower normal. Seventh and all succeeding flowers normal."

F. HOWARD COLLINS.

Churchfield, Edgbaston.

THE "UMBRIA'S" WAVE.

I HAVE been instructed by the Meteorological Council to send you the following report of the *Umbria's* wave from Capt. Watson, F.R.Met.Soc., who is General Superintendent of the Cunard Line of steamers.

HENRY TOYNBEE,
Marine Superintendent.

Meteorological Office, September 18.

MY DEAR CAPT. TOYNBEE,—I send you all the particulars I can get regarding the so-called "big wave" that struck the *Umbria*. No doubt there were some big waves knocking about the Atlantic on the morning of

July 26, but nothing more than could, under the conditions of weather, be expected.

I cannot find out anything about other steamers meeting an exceptionally big wave.

Abstract of Log, s.s. "Umbria."

Date.	Wind.	Bar.	Air.	Water.	Remarks.
July 25.					
Noon	S.W.	29·60	62°	63°	Strong wind and overcast.
4 p.m.	W.S.W.	29·50	60	61	Fresh wind and showery.
8 p.m.	W. by N.	29·45	60	61	Fresh wind and clear.
Midnight	W. by N.	29·31	60	62	Moderate gale, force 9.
26th.					
4 a.m.	N.W. by W.	29·42	59	61	Moderate gale and squally, force 9.
8 a.m.	N.W. by W.	29·50	60	62	
Noon	N.W. by W.	29·70	59	62	

"4.40 a.m. Sea came on board over the bows, breaking No. 2 companion hatch, twisting the forward bridge, breaking some iron stanchions on the bridge, breaking the short bridge between the forward end of the promenade deck and the break of the forecastle, and bending the brass rails on the port side of the main upper bridge, leaving the lower bridge intact. 8 a.m. Fresh gale, force 9, with a heavy, confused sea. Noon. Gale moderating and the sea going down, but still confused."

So much from the log-book; but the following particulars are from the chief officer's report, and the statement of the second officer, who was in charge at the time the sea came on board.

At midnight on the 25th the wind was freshening from west by north, and the weather becoming squally. A long, heavy sea was coming from west-south-west, but the ship was only taking an occasional spray over all. At 2 a.m., 26th, the wind was west-north-west, a gale, with heavy and frequent squalls, sea rising fast from north-west. At 4, the wind had veered to north-west, with heavy and frequent squalls. At this time the west-south-west sea was still very heavy, with a high north-west sea running across and over it, making a very high and confused sea; but the ship was making 16 knots, and though the spray was flying fore and aft, she had not up to this time taken a drop of solid water on board.

At 4.40 a.m., lat. 50° 50′ N., long. 27° 8′ W., the officer of the watch noticed a heavy-breaking sea coming from the north-west; he ordered the officer at the engine telegraphs to reduce to "half speed," but before this could be done the top of this sea came on board, but did no damage. The ship rose quickly to it, but as this wave passed under the stern she plunged heavily, and dipping her bows into the second wave—not breaking, or as the officer of the watch expresses it, "dead water,"—scooped up a mass of water which, running aft over the break of the forecastle, fell upon No. 2 companion hatch, breaking it to pieces, also breaking the short bridge between the fore-end of the promenade deck and the break of the forecastle. The look-out bridge between the lighthouses was twisted, and five iron stanchions and 20 feet of the iron rails on it broken, and four brass stanchions on the port side of the upper main bridge were bent; the middle part of the topgallant forecastle deck for 40 feet in a fore-and-aft line, was sent down 2 inches by the weight of water passing over it. Some water got down No. 2 hatchway and frightened a few passengers.

The second officer is certain that the first sea did no damage, as only the top of it broke over the ship, but he describes the plunge the ship took, as this wave passed astern, as very heavy, and that she went bows into the solid water of the second wave, which he is quite certain was not breaking, but "coming smoothly along." This made the ship "stagger, and the sensation was as if she had struck something hard." After the sea came on board the speed was reduced to 10 knots, and was not increased till noon.

The canvas screen on the port side of the upper main bridge was spread, and the spray striking this bent the brass stanchions. The lower bridge escaped, through there being no canvas screen spread.

Although the wind was three points on the starboard bow, with a heavy sea from the same direction, it seems to me, from the brass stanchions on the upper main bridge having been bent aft and to starboard, and from certain marks on the forecastle deck, that the second officer's statement, as to the damage being done by the second wave (in my opinion the west-south-west sea, which was still running high and fast), is correct; and in my own experience I have seen, on more than one occasion, serious damage done by a sea coming up on the lee bow and breaking on board hours after the wind had been blowing three or four points on the other bow.

If we take into consideration a long and heavy sea from west-south-west, a north-west gale, and heavy sea from the same quarter, we shall have an ugly, confused sea. If a very powerful ship with very fine lines is driven at the rate of 16 knots through this confused sea, I do not think there is the least occasion to call in the aid of tidal or earthquake waves to account for any damage the ship would receive.

In the engine-room there was no shock felt, and the sailors and firemen say they did not notice anything unusual save only some passengers making a noise.

The masthead light was extinguished through the chimney being unshipped and falling across the wick.

Yours very truly,
W. WATSON.

Huskisson Dock, Liverpool, August 18.

THE GARDEN ROSES OF INDIA.

THE principal garden roses cultivated in Europe and in India may be traced to Western Asia and China. The old-fashioned summer roses, which were the ornament of gardens in Europe forty to fifty years ago, are mostly referred to *Rosa gallica*, which has its home in South Europe and Western Asia, and to *Rosa centifolia* and *damascena*, which probably came from the mountains of Armenia and Northern Persia. All these are distinguished by the incomparable delicacy of their aroma, and of the two last-named kinds one or the other is cultivated on a large scale in Southern France, Italy, Macedonia, Asia Minor, Persia, and Northern India, for rose-water and essence of roses (attar). The flowering season of these kinds is short, lasting a few weeks only, and it was an important event for horticulture when, towards the close of last century, the China roses were introduced in Europe. The most important of these was *Rosa indica*, thus called by Linnæus because it was brought from India, where it has long been grown in gardens. Its home, however, is not India, but China, and its great value consists in this, that it flowers throughout summer and autumn, hence the name autumnal rose, also monthly rose (*Monatsrose*). For this reason a variety was called *Rosa semperflorens*. Another variety, described under the name of *Rosa fragrans*, distinguished by its strong though not always very delicate scent, became the parent of the tea-roses. By crossing these kinds and other species with the old garden roses, the numberless varieties of hybrid perpetuals and tea-roses have been obtained, which now ornament our gardens in Europe as well as in India.

In India nine or ten species of roses are indigenous, but with the exception of *Rosa moschata*, a magnificent climber of wide distribution, none have contributed to the production of garden roses. All have their local names in the language of the district where they grow, but—and this is a most remarkable fact—the rose has no name in Sanskrit. In some dictionaries *Java* is rendered as *Rose*, but this is an altogether different shrub, *Hibiscus Rosa-sinensis*, the well-known shoe-flower (used for blacking shoes) of Indian gardens, believed to be indigenous in China, and possibly also indigenous in tropical Africa.

As far as known at present, the roses of Western Asia have no Sanskrit name, and were not known in ancient India. Yet *Rosa damascena* is extensively grown on a large scale for the manufacture of rose-water and essence of roses, throughout Northern India, as far as Ghazipúr, in 25° N. lat. Hermann Schlagintweit was, I believe, the first to draw attention to this remarkable fact. It is not impossible that the western roses were introduced into India by the Mohammedans. As there is no Sanskrit word, so is there no original term for the rose in Hindi. In most Indian languages the cultivated rose is called *gúl*, which is the Persian name. It is also called *gúláb*, which really means rose-water, unless, indeed, as sometimes stated by Munshis in India, *áb* in this case is a suffix with no separate meaning. In addition to their local names, some of the wild roses of the Himalayas are often called *gúláb*, *bán gúláb* (the rose of the forest, or wild rose).

Besides *Rosa indica*, several other Chinese species are cultivated in India. The origin of one of the Indian garden roses, however, is doubtful: this is *Rosa glandulifera*, well described by Roxburgh in his "Flora Indica." It is a white subscandent cluster rose, which has erroneously been referred to *Rosa alba*. In Hindi and Bengali it is called Seoti, Sivati, Shevati. According to Piddington ("English Index to the Plants of India," 1832), this rose has a Sanskrit name, Sevati, pointing to *shveta* (white). This, however, requires verification. Roxburgh believed its origin to be China. D. BRANDIS.

THE IRON AND STEEL INSTITUTE.

THE autumn meeting of this Institute was held at Owens College, Manchester, from the 14th to the 17th inst. The address of the President was of a twofold character, having reference to the metallurgy of steel and to the question of trade. As regarded mild and hard steel, the success that had attended the application of the former was due, in the President's opinion, to the greater ease in its manufacture as compared with hard steels, in which the alloying compounds vary in specific gravity, melting-point or fusibility, and specific heat. Another feature in favour of the manufacture of mild steel is that not more than one-tenth per cent. of the combined elements—sulphur, phosphorus, and silicon—is admissible. As regards working, although the steam-hammer was requisite to force cinder out of puddled iron, with steel such violence is not required, and must be abandoned in favour of the quiet concentrated force of the forging press.

In his reference to trade, and especially the iron and steel trade, Mr. Adamson gave it as his opinion that, so long as we tax ourselves for the benefit of a foreign producer, and pay all the cost incident to the carrying on of our country, and enable the merchant to import manufactured goods from abroad (which bear no portion of the taxation of the country) at a greater profit than he could realize by purchasing at home, so long will our great trade remain depressed and our foreign competitor rejoice at our want of foresight. He also drew attention to the circumstance that the manufacture of goods at home employed not only the hands directly engaged in the industry, but gave work also to cognate industries.

The first paper read was on "Metallurgical and Mechanical Progress as illustrated at the Manchester Exhibition." The object of the paper was to point out to the visitor what was to be seen at the Exhibition; the paper was not discussed.

The paper of Sir Lowthian Bell, on "The Reduction of Ores of Iron in the Blast Furnace," was next read, and gave rise to a very lengthy discussion. The author first explained the general functions of the blast furnace, in which the ores are reduced in the uppermost zone, heated and chemically acted upon in the intermediate region, and melted in the lowest portion. In the reducing zone the ore and limestone are first heated only, but at about 400° F. the oxide of iron begins to lose oxygen; this is found in the gases, together with the carbon and oxygen contained in the carbonic acid of the limestone which is separated at a temperature of about 1500° F. A table was given, indicating generally the increased energy of carbonic oxide as a reducing agent, with increase of temperature, its influence being affected by variations in the ore. The paper is mainly of technical interest.

The next paper read was on "The Basic Open-hearth Process." The subject was treated under the three headings of plant, process, and produce. The furnace described is a modified form of the Siemens furnace, and it is necessary that, a basic-lined hearth having been obtained, a basic slag should be maintained in working.

The last paper read and discussed was on "Electric Lighting in Works and Factories," by Prof. J. A. Fleming. Several interesting points were brought forward. Thus, a table was exhibited comparing a 1200-light dynamo as manufactured in 1882 and 1887. The weight alone of the dynamo of the former machine was 44,820 pounds; that of the armature, 9800 pounds. It occupied 320 cubic feet. With a terminal electromotive force of 108 volts, and an output of 790 amperes, the total horse-power applied to rotate the armature was 154·8, the commercial efficiency was 67 per cent., whilst the price per 1000 watts output was £24. In the 1887 machine the weight is 11,760 pounds; that of the armature alone, 1568 pounds; the cubic space occupied is about 180 cubic feet. The terminal electromotive force is 105 volts, the current 720 amperes, the external electric activity 75,600 watts, and the power required on the pulley about 112 horse-power, the commercial efficiency being over 90 per cent., and the price 6*l*. per 1000 watts output. Statistics from the United States and France show that 117,201 lamps are used in the former country, and 55,321 in the latter; the great majority employed in each case are for workshops and factories. This is a very much larger extension of electric lighting than has taken place in this country. The author recommends, where possible, driving each dynamo from a separate steam-engine, controlled by a good governor, and having all the wiring and fittings carefully tested. In the matter of incandescence lamps attention is drawn to the importance of keeping the electromotive force within a volt of that marked upon the lamps; in this way the length of the life of the lamps is much increased. In the use of naked arc-lights the illumination rapidly falls off as we get away from the light. To obviate this, Mr. A. S. Trotter introduced his dioptric shades. The light is surrounded with a glass shade so cut or grooved into prismatic furrows that whilst causing only a small actual absorption, it will yet refract the rays of light in such a manner as to take away the light from directly under the lamp and increase the illumination at the remote districts. In conclusion, the author makes the statement that, as regards mills, works, and factories, the more closely its advantages and merits are inquired into, the more forcible they will seem, even in face of the fact that gas in the United Kingdom has a lower average price than in any other part of the world.

THE BRITISH ASSOCIATION.

SECTION H.

ANTHROPOLOGY.

OPENING ADDRESS BY PROF. A. H. SAYCE, M.A., PRESIDENT OF THE SECTION.

SURPRISE has sometimes been expressed that anthropology, the science of man, should have been the last of the sciences to come into being. But the fact is not so strange as it seems at first sight to be. Science originated in curiosity, and the curiosity of primitive man, like the curiosity of a child, was first exercised upon the objects around him. The fact that we are separate from the world about us, and that the world about us is our own creation, is a conviction which grows but slowly in the mind either of the individual or of the race in general. The child says, "Charley likes this," before he learns to say, "*I* like this," and in most languages the objective case of the personal pronoun exhibits earlier forms than the nominative.

Moreover, it is only through the relations that exist between mankind and external nature that we can arrive at anything like a scientific knowledge of man. Science, it must be remembered, implies the discovery of general laws, and general laws are only possible if we deal, not with the single individual, but with individuals when grouped together in races, tribes, or communities. We can never take a photograph of the mind of an individual, but we can come to know the principles that govern the actions of bodies of men, and can employ the inductive method of science to discover the physical and moral characteristics of tribes and races. It is through the form of the skull, the nature of the language, the manners and customs, or the religious ideas of a people that we can gain a true conception of their history and character. The thinker who wishes to carry out the precept of the Delphian oracle and to "know himself" must study himself as reflected in the community to which he belongs. The sum of the sciences which deal with the relations of the community to the external world will constitute the science of anthropology.

The field occupied by the science is a vast one, and the several workers in it must be content to cultivate portions of it only. The age of "admirable Crichtons" is past; it would be impossible for a single student to cover with equal success the whole domain of anthropology. All that he can hope to do is to share the labour with others, and to concentrate his energies on but one or two departments in the wide field of research. A day may come when the work we have to perform will be accomplished, and our successsors will reap the harvest that we have sown. But meanwhile we must each keep to our own special line of investigation, asking only that others whose studies have lain a different direction shall help us with the results they have obtained.

I shall therefore make no apology for confining myself on the present occasion to those branches of anthropological study about which I know most. It is more particularly to the study of language, and the evidence we may derive from it as to the history and development of mankind, that I wish to direct your attention. It is in language that the thoughts and feelings of man are mirrored and embodied; it is through language that we learn the little we know about what is passing in the minds of others. Language is not only a means of intercommunication, it is also a record of the ideas and beliefs, the emotions and the hopes of the past generations of the world. In spoken language, accordingly, we may discover the fossilized records of early humanity, as well as the reflection of the thoughts that move the society of to-day. What fossils are to the geologist words are to the comparative philologist.

But we must be careful not to press the testimony of language beyond its legitimate limits. Language is essentially a social product, the creation of a community of men living together and moved by the same wants and desires. It is one of the chief bonds that bind a community together, and its existence and development depend upon the community to which it belongs. If the community is changed by conquest or intermarriage or any other cause the language of the community changes too. The individual who quits one community for another has at the same time to shift his language. The Frenchman who naturalizes himself in England must acquire English; the negro who is born in the United States must adopt the language that is spoken there.

Language is thus a characteristic of a community, and not of an individual. The neglect of this fact has introduced untold mischief not only into philology, but into ethnology as well. Race and language have been confused together, and the fact that a man speaks a particular language has too often been assumed, in spite of daily experience, to prove that he belongs to a particular race. When scholars had discovered that the Sanskrit of India belonged to the same linguistic family as the European languages, they jumped to the conclusion that the dark-skinned Hindu and the light-haired Scandinavian must also belong to one and the same race. Time after time I have taken up books which sought to determine the racial affinities of savage or barbarous tribes by means of their language alone. Language and race, in short, have been used as synonymous terms.

The fallacy is still so common, still so frequently peeps out where we should least expect it, that I think it is hardly superfluous, even now, to draw attention to it. And yet we have only to look around us to see how contrary it is to all the facts of experience. We Englishmen are bound together by a common language, but the historian and the craniologist will alike tell us that the blood that runs in our veins is derived from a very various ancestry. Kelt and Teuton, Scandinavian and Roman, have struggled together for the mastery in our island since it first came within the horizon of history, and in the remoter days of which history and tradition are silent archæology assures us that there were yet other races who fought and mingled together. The Jews have wandered through the world adopting the languages of the peoples amongst whom they have settled, and in Transylvania they even look upon an old form of Spanish as their sacred tongue. The Cornishman now speaks English; is he on that account less of a Kelt than the Welshman or the Breton?

Language, however, is not wholly without value to the ethnologist. Though a common language is not a test of race, it is a test of social contact. And social contact may mean—indeed very generally does mean—a certain amount of intermarriage as well. The penal laws passed against the Welsh in the fifteenth century were not sufficient to prevent marriages now and then between the Welsh and the English, and in spite of the social ostracism of the negro in the Northern States of America intermarriages have taken place there between the black and the white population. But in the case of such intermarrying the racial traits of one member only of the union are as a general rule preserved. The physical and moral type of the stronger parent prevails in the end, though it is often not easy to tell beforehand on which side the strength will lie. Sometimes, indeed, the physical and moral characters are not inherited together, the child following one of his parents in physical type while he inherits his moral and intellectual qualities from the other. But even in such cases the types preserve a wonderful fixity, and testify to the difficulty of changing what we call the characteristics of race.

Herein lies one of the most obvious differences between race and language, a difference which is of itself sufficient to show how impossible it must be to argue from the one to the other. While the characteristics of race seem almost indelible, language is as fluctuating and variable as the waves of the sea. It is perpetually changing in the mouths of its speakers; nay, the individual can even forget the language of his childhood and acquire another which has not the remotest connexion with it. A man cannot rid himself of the characteristics of race, but his language is like his clothing which he can strip off and change almost at will.

It seems to me that this is a fact of which only one explanation is possible. The distinctions of race must be older than the distinctions of language. On the monuments of Egypt, more than four thousand years ago, the Libyans are represented with the same fair European complexion as that of the modern Kabyles, and the painted tomb of Rekh-mâ-ra, a Theban prince who lived in the sixteenth century before our era, portrays the black-skinned negro, the olive-coloured Syrian, and the red-skinned Egyptian with all the physical peculiarities that distinguish their descendants to-day. The Egyptian language has ceased to be spoken even in its latest Coptic form, but the wooden figure of the "Sheikh-el-beled" in the Bulaq Museum, carved six thousand years ago, reproduces the features of many a fellah in the modern villages of the Nile. Within the limits of history racial characteristics have undergone no change.

I see, therefore, no escape from the conclusion that the chief distinctions of race were established long before man acquired language. If the statement made by M. de Mortillet is true, that the absence of the mental tubercle, or bony excrescence in which the tongue is inserted, in a skull of the Neanderthal type found at La Naulette, indicates an absence of the faculty of speech, one race at least of Palæolithic man would have existed in Europe before it had as yet invented an articulate language. Indeed, it is difficult to believe that man has known how to speak for any very great length of time. On the one hand, it is true, languages may remain fixed and almost stationary for a long series of generations. Of this the Semitic languages afford a conspicuous example. Not only the very words, but even the very forms of grammar are still used by the Bedouin of Central Arabia that were employed by the Semitic Babylonians on their monuments five thousand years ago. At that early date the Semitic family of speech already existed with all its peculiarities, which have survived with but little alteration up to the present day. And when it is remembered that Old Egyptian, which comes before us as a literary and decaying language a thousand years earlier, was probably a sister of the parent Semitic speech, the period to which we must assign the formation and development of the latter cannot fall much short of ten thousand years before the Christian era. But on the other hand there is no language which does not bear upon its face the marks of its origin. We can still trace through the thin disguise of subsequent modifications and growth the elements, both lexical and grammatical, out of which language must have arisen. The Bushman dialects still preserve the inarticulate clicks which preceded articulate sounds in expressing ideas; behind the roots which the philologist discovers in allied groups of words lie, plainly visible, the imitations of natural sounds, or the instinctive utterances of human emotion; while the grammar of languages like Eskimaux or the Aztec of Mexico carries us back to the first mechanism for conveying the meaning of one speaker to another. The beginnings of articulate language are still too transparent to allow us to refer them to a very remote era. I once calculated that from thirty to forty thousand years is the utmost limit that we can allow to man as a speaking animal. In fact, the evidence that he is a drawing animal, derived from the pictured bones and horns of the Palæolithic Age, mounts back to a much earlier epoch than the evidence that he is a speaking animal.

Mr. Horatio Hale has lately started a very ingenious theory to account, not indeed for the origin of language in general, but for the origin of that vast number of apparently unallied families of speech which have existed in the world. He has come across examples of children who have invented and used languages of their own, refusing at the same time to speak the language they heard around them. As the children belonged to civilized communities the languages they invented did not spread beyond themselves, and after a time were forgotten by their own inventors. In an uncivilized community, however, it is quite conceivable that such a language might continue to be used by the children after they had begun to grow up, and be communicated by them to their descendants. In this case a wholly new language would be started, which would have no affinities with any other, and after splitting into dialects would become the parent of numerous derived tongues. I must confess that the evidence brought forward by Mr. Hale in support of his theory is not quite convincing to me. It is yet to be proved that the words used by the children to whom he refers were not echoes of the words used by their elders. If they were, a language that originated in them would show more signs of lexical affinity to the older language than is the case of one family of speech when compared with another. On the other hand, the theory would tend to throw light on the curious fact that the morphological divisions of language are also geographical.

By the morphology of a language I mean its structure; that is to say, the mode in which the relations of grammar are expressed in a sentence, and the order in which they occur. These vary considerably, the chief variations being represented by the polysynthetic languages of America, the isolating languages of Eastern Asia, the postfixal languages of Central Asia, the prefixal languages of Africa, and the inflectional languages of Europe and Western Asia. Now it will be observed that each of these classes of language is associated with a particular part of the globe, the isolating languages, for example, being practically confined to Eastern Asia, and the polysynthetic languages to America. Within each class there are numerous families of speech between which no relationship can be discovered beyond that of a common structure; they agree morphologically, but their grammar and lexicon show no signs of connexion. If we adopt Mr. Hale's theory we might suppose that the genealogically distinct families of speech grew up in the way he describes, while their morphological agreement would be accounted for by the inherited tendency of the children to run their thinking into a particular mould. The words and contrivances of grammar would be new, the mental framework in which they were set would be an inheritance from former generations.

I have spoken of the inflectional languages as belonging to Europe and Western Asia. This is true if we give a somewhat wide extension to the term inflectional, and make it include not only the Indo-European group, but the Georgian and Semitic groups as well. But, strictly speaking, the Indo-European, or Aryan, languages have a structure of their own, which differs very markedly from that of either the Georgian or the Semitic families. The Semitic mode of expressing the relations of grammar by changing the vowels within a framework of consonants differs as much from the Aryan mode of expressing them by means of suffixes as does the Semitic partiality for words of three consonants from the Indo-European carelessness about the number of syllables in a word. Though it is quite true that the Semitic languages at times approach the Indo-European by using suffixes to denote the forms of grammar, while at other times the Indo-European languages may substitute internal vowel change for external flection, nevertheless, in general, the kind of flection employed by the two families of speech is of a totally different character.

This difference of structure, coupled with a complete difference in phonology, grammar, and lexicon, has always seemed to me to negative the attempts that have been made to connect the Aryan and Semitic families of language together. The attempts have usually been based on the old confusion between language and race: both Aryans and Semites belong to the white race; therefore it was assumed their languages must be akin. As long as it was generally agreed that the primitive home of the Aryan languages was, like that of the Semitic languages, the western part of Asia, the confusion was excusable. If the earliest seats of the speakers of each were in geographical proximity, there was some reason for believing that languages which were alike spoken by members of the white race, and were alike classed as inflectional, would, when properly questioned, show signs of a common origin.

But that general agreement no longer exists. While the Asiatic origin of the Semitic languages is beyond dispute, scholars have of late years been coming more and more to the conclusion that Europe was the cradle of the Aryan tongues. Their European origin was first advocated by our countryman Dr. Latham, and was subsequently defended by the eminent comparative philologist Dr. Benfey; but it is only within the last half-dozen years that the theory has won its way to scientific recognition. Different lines of research have been converging towards the same result, and indicating North-Eastern Europe as the starting-point of the Indo-European languages, while the evidences invoked in favour of their Asiatic origin have one and all broken down.

These evidences chiefly rested on the supposed superiority of Sanskrit over the other Indo-European languages as a representative of the parent-speech from which they were all descended. The grammar and phonology of Sanskrit were imagined to be more archaic, more faithful to the primitive pattern than those of its sister-tongues. It was argued that this implied a less amount of migration and change on the part of the speakers, a nearer residence, in fact, to the region where the parent-speech had once been spoken. As a comparison of the words denoting certain objects in the Indo-European languages showed that this region must have had a cold climate, it was placed on the slopes of the Hindu-Kush or at the sources of the Oxus and Jaxartes.

But we now know that instead of being the most faithful representative of the parent-speech, Sanskrit is in many respects far less so than are its sister-languages of Europe. Its vocabulary, for instance, has been thrown into confusion by the coalescence of the three primitive vowel sounds *ă*, *ĕ*, *ŏ* into the single monotonous *ă*, a corruption which is paralleled by the coalescence of so many vowels in modern cultivated English in the so-called "neutral" *ə*. Greek, or even the Lithuanian, which may still be heard to-day from the lips of unlettered peasants, has preserved more faithfully than the Sanskrit of India the features of the parent Aryan. If the faithfulness of the record is any proof of the geographical proximity of one of the Indo-European languages to their common mother, it is in the neighbourhood of Lithuania, rather than in the neighbourhood of

India, that we ought to look for traces of the first home of the Aryan family.

But the theory of the Asiatic origin of the Indo-European family has not only been deprived of its main support by the dethronement of Sanskrit, and the transfer of its primacy to the languages of Europe; what Prof. Max Müller has termed "linguistic palæontology" has further assisted in overthrowing the crumbling edifice. When we find words of similar phonetic form and similar meaning in both the Asiatic and the European branches of the Aryan family—words, too, which it can be shown have not been borrowed by one Indo-European language from another—we are justified in concluding that the objects or phenomena denoted by them were already known to the speakers of the parent-language. When we find, for instance, that the birch is known by the same name in both Sanskrit and Teutonic, we may infer that it was a tree with which the speakers of the mother-tongue of Sanskrit and Teutonic were acquainted, and that consequently they must have lived in a cold climate. In Europe that would have been westward of a line drawn from Königsberg to the Crimea, to the east of which the birch-tree does not grow.

Four years ago a valuable contribution to the linguistic palæontology of the Aryan languages was made by Prof. Otto Schrader. For the first time the question was approached from the present level of comparative philology, and all words were excluded from comparison which did not satisfy the requirements of phonetic law. The results were sadly disquieting to the believers in that idyllic picture of primitive Aryan life to which we had so long been accustomed. Prof. Schrader proved that the speakers of the parent Aryan language must not only have lived in a cold climate—a fact which was known already—but that they must have lived in the Stone Age, with the skins of wild beasts only to protect them from the rigours of the winter, and nothing better than stone weapons with which to ward off the attacks of savage animals. Their general culture was on a level with their general surroundings. It was little better than that of the Fuegian before he came into contact with European missionaries. The minuteness with which the varying degrees of family relationship were named, instead of indicating an advanced social life, as was formerly imagined, really indicated the direct contrary. The primitive Aryan was indeed acquainted with fire; he could even sew his skins together by means of needles of bone; and possibly could spin a little with the help of rude spindle-whorls; but beyond this his knowledge of the arts does not seem to have extended. If he made use of gold or meteoric iron, it was only of the unwrought pieces which he picked up from the ground and employed as ornaments; of the working of metals he was entirely ignorant. But he already practised a kind of rude agriculture, though the art of grinding corn was as yet unknown, and crushed spelt was eaten instead of bread; while the community to which he belonged was essentially that of pastoral nomads, who changed from season to season the miserable beehive huts of wattled mud in which they lived. They could count at least as far as a hundred, and believed in a multitude of ghosts and goblins, making offerings to the dead, and seeing in the bright sky a potent deity.

In calling the speaker of the Aryan parent-speech the primitive Aryan I must not be supposed to be prejudging the question as to the particular race to which he belonged. This is a question which has recently been handled with great ability by an Austrian anthropologist—Dr. Karl Penka. In a remarkable book, published at the end of last year, he endeavours to substantiate the hypothesis advanced in an earlier work, and to show that the first speakers of the Aryan languages were the fair-haired, blue-eyed, light-complexioned dolichocephalic race which is still found in its greatest purity in Scandinavia; that it was this race which in the Neolithic period spread southwards, imposing its yoke upon subject populations, like the Norsemen and Normans of later days, and carrying with it the dialects which afterwards developed into the Aryan languages; and that, finally, it was the same race which in the remote days of the Palæolithic Age inhabited Western and Central Europe, where it has left its remains in the typical skulls of Cannstatt and Engis. Dr. Penka would ascribe to its long residence in the semi-arctic climate of palæolithic Europe the permanent blanching of its skin and hair—a form of albinoism which Dr. Poesche in 1878 endeavoured to explain by the climatic conditions of the Rokitno marshes in Russia, where he placed the cradle of the white Aryan race.

It cannot be denied that all the probabilities are at present on Dr. Penka's side, so far as his main contention is concerned. Without denying that the speakers of the Aryan parent-speech may have already included slaves or wives of alien race, it is probable that the majority of them were of one blood. They formed a single community, nomad it is true, and therefore less likely to mix with foreigners, but still sufficiently a single community to speak a language the several dialects of which were so alike as to be mutually intelligible. In the social condition in which the speakers were, and in an age when the waste lands of the world were still extensive, the greater part of such a community must necessarily, we should think, have belonged to the same race. The evidences of language, moreover, as we have seen, point to a cold and northerly climate as the original seat of the community; and since they further inform us that the birch was known to it, we may conclude that this climate lay westward of Königsberg and Russia. Penka has striven to show that the animals whose bones or shells are found in the Scandinavian kitchen-middens are just those whose names are common to the Indo-European languages, or at all events the European section of the latter. Now, the skulls disinterred from the prehistoric burial-places of Denmark and the southern districts of Sweden and Norway are, for the most part, identical with the skulls still characteristic of the Scandinavian population where they accompany a fair skin and light hair and eyes. By combining these two facts we arrive at the conclusion that the fair Scandinavian race is the modern descendant of the race which spoke the parent-language of the primitive Aryan community, and left traces of itself in the Scandinavian kitchen-middens. The conclusion is supported by the testimony of history. On the one hand we have the testimony of classical writers that the Aryan-speaking Kelts of the Christian era were not the dark small-limbed population which now occupies the larger part of France, but men of large stature, with the blue eyes and fair hair of their Teutonic brethren; while the ideal specimens of humanity conceived of by the aristocratic art of Italy and Greece were the golden-haired Apollo and the blue-eyed Athênê. On the other hand, it was from Scandinavia that in later times other bands of warriors poured forth, who made their way into the countries of the Mediterranean, and even Asia, and established themselves as conquering aristocracies in the midst of subject populations. The Kelts succeeded in reaching Asia Minor, the Scando-German hordes overthrew the Roman empire, the Northmen established themselves from Russia on the east to Iceland and Greenland on the west, and the Normans made Sicily their own long before the days of the German Frederick. The only point in which the later historical irruptions of the Scandinavian peoples differed from their prehistoric ones was, that while the later irruptions were made by sea, the older were made by land. The sail was unknown to the tribes of the north until the age of their intercourse with the Romans, from whom they borrowed both the conception and the name of the *sagulum*, or "sail." The course of their migrations must have followed the valleys of the great rivers.

If Southern Scandinavia is thus to be regarded as the original home of the Aryan languages, and if the race which first spoke those languages, and which we may therefore call Aryan, is to be identified with the Scandinavian type, it follows that the further south and east we advance from this primary starting-point the less pure will the type become. It will be in the neighbourhood of that starting-point and in Northern Europe that we shall expect to find the largest number of undiluted Aryan languages and the purest examples of the Aryan breed. In Greece and Armenia, in Persia and India, we must look for mixture and decay. And such indeed is the fact. Mr. Wharton has found, by a careful analysis of the Greek lexicon, that out of 2740 primary words only 1580 can be referred with any probability to an Indo-European origin, while the prevailing racial type in ancient as in modern Greece was distinctly non-Aryan. Indeed, I am inclined to believe that the culture revealed by the excavations at Mykênæ, Tiryns, and on other prehistoric Greek sites belonged not to a Hellenic but to a pre-Hellenic population, and that the Aryan Greeks first made their appearance in Hellas at the epoch of what later tradition called the Dorian immigration. It was to the north that Greek legends pointed as the primæval home of the Hellenic race and civilization, and Dôdôna ever continued to be revered as the oldest sanctuary of the Hellenic world. In India it is notorious that the Aryan-speaking Hindus entered the country from the north-west, and failed to spread far into the burning plains of the south. The date of their invasion is uncertain, but for myself I have grave doubts whether it was earlier than the eighth or even the seventh century B.C. At all events it was not until after the seventh century B.C., as we now know from the express testimony of the cuneiform inscriptions of Van, that the Aryan-speaking Armenians entered the land which now bears their name, and recent philological researches have confirmed the assertion of Greek writers that the Armenians were a colony of the Phrygians who had themselves emigrated from Thrace. Up to the closing days of the Assyrian empire the monuments make it clear that no Aryans had as yet settled between the Kurdish ranges on the east and the Halys on the west.

But while the extension into Asia of what I will now, following Penka's example, call the Aryan race, seems to be referred to a comparatively recent period, there is a curious fact which goes to show that the same, or a closely allied, race once spread along the northern coast of Africa. On Egyptian monuments, which date back to the sixteenth century before our era, the Libyan tribes of this district are described and depicted as white. Their descendants are still to be found in the mountainous parts of the coast, those of Algeria being commonly known under the name of Kabyles. I saw a good deal of them last winter, and must confess to being greatly struck by their appearance. I had known, of course, that they belonged to the white race and were characterized by blue eyes and light hair, but I was not prepared to find that their complexion was of that transparent whiteness which freckles readily and is supposed to mark the so-called "red Kelt." They are dolichocephalic, and as their skulls agree with those discovered in the prehistoric cromlechs of Roknia and other places it is plain that their distinctive features are not due, as was formerly supposed, to intermixture with the Vandals.

The cromlechs in which they once buried their dead are quite as remarkable as their physical characteristics. Cromlechs of a similar shape are found extending through Spain and western France to the northern portion of the British Isles. Since dolichocephalic skulls occur in connexion with them, while the physical characteristics of the modern Kabyle resemble so strikingly those of a particular portion of the modern Irish population, we seem driven to infer that the Kabyle and the "red Kelt" are alike fragments of a race that once spread from Scotland and Ireland to the northern coast of Africa and interred its dead in chambers formed of five large blocks of stone. Though the custom of burying in these cromlechs continued into the Bronze Age, the majority of them go back to the Neolithic period.

Are we to suppose, then, that one stream of Aryan immigrants, after making its way to the west, wandered along the western coast of Europe, and eventually crossed the Straits of Gibraltar and took possession of Africa? Or are we to believe that the Aryan race of southern Scandinavia was allied in blood, though not in language, with a population which inhabited the extreme west of Europe, and had, it may be at the close of the Glacial epoch, passed over to the neighbouring mountains of Africa? It must be remembered that the Kabyle complexion is not precisely the same as that of the Scandinavian. Both are white, but the skin of the one has a semi-transparent appearance, while the whiteness of the other may be described as mealy. It will be worth while to determine whether between the dolichocephalism of the Kabyle and the dolichocephalism of the Scandinavian any distinction can be drawn.

The question has a bearing on the origin of a part of our own population. I have already compared the Kabyle with the "red Kelt." But the expression "red Kelt," like most popular expressions, is by no means exact. It confuses in one two distinct types. The large limbed, red-haired Highlander, who calls to mind the description given of the Kelts by the Latin historians, stands in marked contrast to the small-limbed, light-complexioned Kelt of certain districts in Ireland, whose skin is freckled rather than burnt red by the sun. The determination of the several racial elements in these islands is particularly difficult on account of the intermixture of population, and nowhere is the difficulty greater than in the case of the Keltic portion of the community. Long before the Roman conquest the intrusive Aryan Kelt had been intermarrying with the older inhabitants of the country, who doubtless belonged to more than one race, the result being that the so-called Keltic race is an amalgamation of races differing physiologically but dominated by a common moral and intellectual character—the consequence of subjection for a long series of generations to the same conditions of life. It has become a commonplace of ethnology that the so-called Keltic race includes not only the fair-complexioned Aryan Kelt, but also the "black Kelt" or Iberian with dark skin, black hair and eyes, and small limbs. The subject, however, is much more complex than this simple division would imply. We have seen that under the "red Kelt" are included two distinct varieties; the "black Kelt" is equally irreducible to a single type, while the fact that the two types of "red" and "black" recur in the same family—my own, for example—not only indicates their long-continued intermixture, but suggests the existence of intermediate varieties. The limitations and relations of dolichocephalism and brachycephalism within the race also need further investigation. I hope that this meeting, held as it is on the borders of what is still a distinctively Keltic country, may help to settle these and similar problems.

Meanwhile I will conclude this address, which has already extended to an inordinate length, by directing your attention to two lines of evidence which have an important bearing on the question of the extent to which the Keltic element enters into the existing British population. A few years ago it was the fashion to assert that the English people were mainly Teutonic in origin, and that the older British population had been exterminated in the protracted struggle it carried on with the heathen hordes of Anglo-Saxon invaders. The statement in the "Saxon Chronicle" was quoted that the garrison of Anderida, or Pevensey, when captured by the Saxons in A.D. 491, was all put to the sword. But it is obvious that the fact would not have been singled out for special mention had it not been exceptional, while it is equally obvious that invaders who came by sea can hardly have brought their wives and children with them, and must have sought for both wives and slaves in the natives of the island. Mr. Coote, in his "Romans of Britain," and Mr. Seebohm, in his "English Village Community," have pointed out the continuity of laws and customs and territorial rights between the Roman and the Saxon eras, presupposing a continuity of population, and anthropologists have insisted that the survival of early racial types in all parts of the country cannot be accounted for by the settlement of the Bretons who followed William the Conqueror, or of the Welsh who came into England when the penal laws against them were repealed by Henry VIII. But the advocates of the theory of extermination had always one argument which seemed to them unanswerable, and which indeed was the origin of their theory. The language of the Anglo-Saxons contains scarcely any words borrowed from Keltic. Such a fact was held to be inexplicable except on the hypothesis that the speakers of the Keltic dialects were all exterminated before any intercourse was possible between them and the invading Teuton.

But I think I can show that the fact admits of quite another explanation. Roman Britain was in the condition of Roman Gaul; it was a Roman province, so thoroughly Romanized indeed that before the end of the first century, according to Tacitus ("Agric.," 18–21), even the inhabitants of North Wales had adopted the Roman dress and the Roman habits of luxury. After four centuries of Roman domination it is not likely under these circumstances that the dialects of the British tribes would have resisted the encroachments of the Latin language any more than did the dialects of Gaul. The language, not only of government and law, but also of trade and military service, was Latin, while the slaves and servants who cultivated the soil were bound to understand the language of their masters. Moreover, Britain was a military colony; the natives were drafted into the army, and there perforce had to speak Latin. If Latin had not been the language of the country at the time the Romans left it, the fact would have been little short of a miracle.

That it was so is certified by more than one piece of evidence. The inscriptions which have survived from the period of the Roman occupation are numerous; with the exception of three or four Greek ones, they are all in Latin. Of a Keltic language or dialect there is no trace. When the Romans had departed, and the inhabitants of Wales and Cornwall had been cut off from intercourse with the civilized world, Latin was still the ordinary language of the mortuary texts. It is only gradually that Keltic oghams take their place by the side of the Roman characters. When St. Patrick writes a letter to the Welsh prince of Cardiganshire, addressed not only to him but to his people as well, it is in the Latin language; when St. Germanus crosses into Britain to settle a theological controversy, and leads the people to victory against the Saxon invader, he has no difficulty in being understood; and the proper names of the British leaders continue to be Roman long after the departure of the Roman legions. What clinches the matter, however, is the positive statement of Gildas, the British writer, the solitary witness who has survived to us from the dark period of heathen invasion. He asserts that the ships called "keels" by the Saxons were called *longæ naves* "in *our* language" ("nostra lingua," "Hist.," 23). In the middle of the sixth century, therefore, Latin was still the language of the Kelt south of the Roman Wall. Such being the case, it is not Keltic but Latin words that we must expect to have been borrowed by Anglo-Saxon, if the British population, instead of being exterminated, lived under and by the side of their Teutonic invaders. Now these borrowed Latin words exist in plenty. They have come not only from the speech of the towns, but also from the speech of the country, proving that the country population must have used Latin like the inhabitants of the towns. In an interesting little book by Prof. Earle on the Anglo-Saxon names of plants, a list is given of the names of trees and vegetables that have been taken from a Latin source. Where the tree or the vegetable was one with which the invaders had not been acquainted in their original home, the name they gave to it was a Latin one, like the *cherry* or *cerasus*, the *box* or *buxus*, the *fennel* or *feniculum*, the *mallow* or *malva*, the *poppy* or *papaver*, the *radish* or *radix*. Such names they could have heard only from the serfs who tilled the ground for their new lords, not from the traders and soldiers of the cities. It is much the same when we turn to the names of agricultural implements which imply a higher order of culture than the simple plough or mattock, the name of which last, however is itself of Keltic origin. Thus the coulter is the Latin *culter*, the sickle is the Latin *secula*. That other agricultural implements bore Teutonic names proves merely that the Saxons and Angles were already acquainted with them before they had quitted their primitive seats.

The philological argument has thus been cut away from under the feet of the advocates of the theory of extermination, and shown to tell precisely the contrary tale. It has disappeared like the philological argument by which the theory of the origin of the Aryans in Asia was once supposed to be supported. But there still remains one difficulty in our path.

This is the fact that the languages spoken in Wales, and till recently in Cornwall, are Keltic and not Latin. If Latin had been the language of the Keltic population of Southern Britain when the Romans left the island, how is it that where the Keltic population still retains a language of its own that language is Keltic? The answer to this question is to be found in history and tradition. Up to the sixth century the Teutonic invaders gained slowly but steadily upon the resisting Britons. They forced their way to the frontiers of what is now Wales, and there their further course was checked. The period when this took place is the period when Welsh literature first begins. But it begins, not in Wales, but in Strathclyde, or South-Western Scotland, to the north of the Roman Wall. Its first records relate to battles that took place in the neighbourhood of Carlisle. From thence its bards and heroes moved southwards into North Wales. Tradition commemorated the event as the arrival in Wales of "Cunedda's men." The sons of Cunedda founded the lines of princes who subsequently ruled in Wales, and the old genealogies mark the event by suddenly substituting princes with Welsh names for princes with Latin names. The rude Keltic tribes of Strathclyde came to the assistance of their more cultured brethren in the south, checking the further progress of the foreigner and imposing their domination and language upon the older population of the country. It is probable that the disappearance of Latin was further aided not only by the destruction of the cities and the increasing barbarism of the people, but also by the settlement of Irish colonies, more especially in South Wales. At all events the ruin of cities like Caerleon and Caerwent must be ascribed to Irish marauders. We can now explain why it is not only that Wales speaks Welsh and not Latin, but also why a part of the country which, according to Prof. Rhŷs, was mostly peopled by Gaelic tribes before the Roman conquest, speaks Cymric and not Gaelic. As for Cornish, its affinities were with Breton, and since history knows of frequent intercourse between Cornwall and Brittany in the age that followed the departure of the Romans we may see in the Cornish dialect the traces of Breton influence.

The arrival of "Cunedda's men" and the re-Keltization of Wales leads me to the second line of evidence to which I have alluded above. The bearing of the costume of a people upon their ethnography is a matter which has been much neglected. But there are few things about which a population—more especially in an early stage of society—is so conservative as in the matter of dress. When we find the Egyptian sculptor representing the Hittites of the warm plains of Syria clad in the snow-shoes of the mountaineer, we are justified in concluding that they must have descended from the ranges of the Taurus, where the bulk of their brethren continued to live, just as the similar shoes with turned-up ends which the Turks have introduced among the upper classes of Syria, Egypt, and Northern Africa, point to the northern origin of the Turks themselves. Such shoes are utterly unsuited for walking in over a country covered with grass, brushwood, or even stones; they are, on the contrary, admirably adapted for walking on snow.

Now the dress of Keltic Gaul and of Southern Britain also when the Romans first became acquainted with it was the same as the dress which "linguistic palæontology" teaches us had been worn by the primitive Aryans in their first home. One of its chief constituents were the *braccæ*, or trousers, which accordingly became to the Roman the symbol of the barbarian. We learn, however, from sculptures and other works of art, that before the retirement of the Romans from the northern part of Europe they had adopted this article of clothing, at all events during the winter months. That the natives of Southern Britain continued to wear it after their separation from Rome is clear from a statement of Gildas ("Hist." 19) in which he refers in no flattering terms to the kilt of the Pict and the Scot. Yet from within a century after the time of Gildas there are indications that the northern kilt, which he regards as so strange and curious, had become the common garb of Wales. When we come down to the twelfth century we find that it is the national costume. Giraldus Cambrensis gives us a description of the Welsh dress in his own time, from which we learn that it consisted simply of a tunic and plaid. It was not until the age of the Tudors, according to Lluyd, the Welsh historian of the reign of Elizabeth, that the Welsh exchanged their own for the English dress.[1] The Welsh who served in the army of Edward II. at Bannockburn were remarked even by the Lowland Scotch for the scantiness of their attire (Barbour's "Bruce," ix. 600-603), and we have evidence that it was the same a century later.[2] If we turn to Ireland we find that in the days of Spencer, and later, the national costume of the Irish was the same as that of the Welsh and the Highland Scotch. The knee-breeches and sword-coat which characterize the typical Irishman in the comic papers are survivals of the dress worn by the English at the time when it was adopted in Ireland.

The Highland dress, therefore, was once worn not only in the Scotch Highlands and in Ireland, but also in Wales. It characterized the Keltic parts of Britain with the exception of Cornwall and Devonshire. Yet we have seen that up to the middle of the sixth century, at the period when Latin was still the language of the fellow-countrymen of Gildas, and when "Cunedda's men" had not as yet imposed their domination upon Wales, the old Keltic dress with trousers must have been the one in common use. Now we can easily understand how a dress of the kind could have been replaced by the kilt in warm countries like Italy and Greece; what is not easily conceivable is that such a dress could have been replaced by the kilt in the cold regions of the north. In warm climates a lighter form of clothing is readily adopted; in cold climates the converse is the case.

I see, consequently, but one solution of the problem before us. On the one hand, there was the distinctive Keltic dress of the Roman age, which was the same as the dress of the primitive Aryan, and was worn alike by the Kelts of Gaul and Britain and the Teutons of Germany; on the other hand, there was the scantier and colder dress which originally characterized the coldest part of Britain, and subsequently mediæval Wales also. Must we not infer, in the first place, that the aboriginal population of Caledonia and Ireland was not Keltic—or at least not Aryan Keltic? and, secondly, that the dominant class in Wales after the sixth century came from that northern portion of the island where the kilt was worn? Both inferences at all events agree with the conclusions which ethnologists and historians have arrived at upon other grounds.

Perhaps what I have been saying will show that even a subject like the history of dress will yield more results to ethnological study than is usually supposed. It will be another illustration of the fact that the student of humanity cannot

[1] "The Breviary of Brytaine," Twyne's translation, p. 35 (ed. 1573)

[2] See Jones, "History of the County of Brecknock," vol. i. p. 283; comp. "Archæologia Cambrensis," 5th ser. No. 7 (1885), p. 227.

afford to neglect any department of research which has to do with the life of man, however widely removed it may seem to be from science and scientific methods of inquiry. "Homo sum; humani nihil a me alienum puto"

REPORTS.

Report of the Committee, consisting of Profs. Tilden, W. Chandler Roberts-Austen, and Mr. T. Turner, on the Influence of Silicon on the Properties of Steel.—One series of experiments has been completed, and the results obtained are:—On adding silicon to the purest Bessemer iron, the metal is originally red short, especially at a dull red heat, though it works well at a welding temperature; the red shortness is increased by silicon. Silicon increases the elastic limit and tensile strength, but diminishes the elongation and the contraction of area, a few hundredths per cent. having a remarkable influence in this respect. The hardness increases with the increase of silicon. With 0·4 per cent. of silicon and 0·2 per cent. of carbon, a steel was obtained difficult to work at high temperatures, but tough when cold, capable of being hardened in water, and giving a cutting-edge which successfully resisted considerable hard usage.

Report of the Committee, consisting of Profs. Tilden, Ramsay, and Dr. Nicol, on the Nature of Solution.—The constants of supersaturated and non-saturated solutions have been examined. Starting from non-saturated solutions, the temperature was lowered until the point of saturation was reached, and the physical properties of solutions near the point of saturation were examined at a constant temperature (20°). There appears to be no difference of physical properties within these limits from those of ordinary solutions. Experiments are also described on the specific viscosity of solutions, and there is added also a report on the bibliography of the subject.

Report of the Committee on Isomeric Naphthalene Derivatives.—Prof. Armstrong pointed out how naphthalene obeys the α-law, and described the formation of the dichlorides by different methods. He showed that the products, though at first apparently the same, have been now proved distinct. He also went into the sulphonating of β-naphthols.

Second Report on the Cae Gwyn Cave, North Wales, by Dr. H. Hicks.—The main object that the Committee had in view this year was to extend the excavation which had been made in front of the new entrance to the cavern, discovered last year, so that a clear section of the deposits which covered that entrance might be exposed. Work was commenced on June 6 and continued to the 18th, when it was decided that a sufficient excavation had been made, and work was for the time suspended. The excavation was visited daily by some members of the Committee. It was found necessary to remove much of the timber placed last year to support the face in front of the entrance, so that the section might be clearly exposed, and the cutting was widened here sufficiently to show a vertical face of undisturbed deposits. The timber supporting the north-east face of the cutting was allowed to remain, as that portion had been well exposed last year, and it was thought that the excavation in front and to the south-west would yield all necessary evidence without incurring that additional trouble and expense. The cutting was carried in a south-south-west direction from the mouth of the cavern, and beyond the dip in the field supposed to indicate the line of an old fence; the length from the timber on the north-east face to the commencement of the dip in the field being about 30 feet, and the width varying from 5 to 10 feet; the narrowest part being at the furthest point from the cavern. In the face exposed in front of the entrance, and for a distance in the cutting from there of about 25 feet, the soil varied in depth from 18 inches to 2 feet, but at the slope supposed to indicate the line of the old fence it thickened considerably. Underlying this throughout the whole length of the cutting and in the field beyond this point, a boulder-clay of a reddish-brown colour was exposed. This boulder-clay contained thin seams of sand, which were traceable generally at the same horizon along the whole section. At a depth of about 7 feet from the surface, in a continuous band of reddish sandy clay, numerous fragments of marine shells and some perfect ones were met with, and these have been recognized by Mrs. McKenny Hughes to belong to the following species, viz. *Ostrea* sp., *Mytilus* sp., *Nucula nucleus*, *Cardium echinatum*, *C. edule*, *Cyprina islandica*, *Astarte borealis*, *Artemis exoleta*, *Venus gallina ?*, *Tellina balthica*, *Psammobia ferröensis*, *Donax ?*, *Mya truncata*, *Littorina* sp., *Turritella terebra*, *Buccinum undatum*. Below the boulder-clay, at a depth of about 9 feet from the surface, there was exposed some sandy gravel and fine banded sand with a total thickness of over 6 feet, and under the latter a well-defined band of finely laminated reddish clay. Below the laminated clay the brecciated bone-earth was found to extend as far as the cutting was made in front of the entrance, and also for a distance of 7 feet in a southerly direction from the entrance. This year only a few fragments of bone and bits of stalagmite were obtained from this earth, though it will be remembered that last year it yielded many teeth as well as the flint flake which was discovered near the entrance. The limestone floor under the bone-earth was found to rise gradually outwards from the mouth of the cavern for some distance, forming a shallow basin-shaped space in front of the entrance. In the bone-earth in this space there were several large angular blocks of limestone. It was not thought necessary to dig down to the floor along the whole length of the cutting, but it was traced for 7 feet in that direction by the side of the cliff against which the deposits abutted. Beyond that point the cutting was made deep enough to reach the sandy gravel under the boulder-clay, and at different parts test-holes were sunk still deeper into the gravel and sand. One hole was also sunk in the field in front of the cutting at a distance of over 35 feet from the entrance to the cavern. The deposits here were found to be similar to those in the cutting and in front of the cavern, but the depth of soil over the boulder-clay was only from 1 foot to 18 inches. A very large number of smoothed and ice-scratched boulders were found, many of considerable size; the majority being fragments of Wenlock shale from the neighbourhood and Lower Silurian rocks from the Snowdonian area. Amongst them also were fragments of granite, gneiss, quartzites, flint, diorites, basalts, Carboniferous rocks, &c.

Report of the Committee, consisting of Mr. John Cordeaux, (secretary), Prof. A. Newton, Mr. J. A. Harvie-Brown, Mr. William Eagle Clarke, Mr. R. M. Barrington, and M. A. G. More, reappointed at Birmingham for the purpose of obtaining (with the consent of the Master and Brethren of the Trinity House and the Commissioners of Northern and Irish Lights) observations on the Migration of Birds at Lighthouses and Lightvessels, and of reporting on the same.

The General Report[1] of the Committee has been printed in a pamphlet of 174 pages, and includes observations from 126 stations out of a total of 198 supplied with schedules, letters of instruction, and cloth-lined envelopes for wings; altogether 280 schedules have been sent in. In the last report attention was particularly directed to those main highways or lines of migration by which birds approach the east coast of Scotland both in the spring and autumn. Two chief lines seem to be clearly indicated, by the Pentland Firth and Pentland Skerries, also by the entrance of the Firth of Forth as far north as the Bell Rock Lighthouse. Continued observations also indicate that on the east coast of England the stream of migration is not continuous over the whole coast line, but seems to travel along well-established lines, which are persistently followed year by year.

On the east coast of England there seems to be a well-marked line, both of entry and return, off the Farn Islands, on the coast of Northumberland. Scarcely second to this in importance is the mouth of the Tees, both in the spring and autumn. The North Yorkshire coast and the elevated moorland district from the south of Redcar to Flamborough, including the north side of the headland, is comparatively barren, few birds appearing to come in. Bridlington Bay and Holderness to Spurn and Lincolnshire, as far as Gibraltar Point, on the coast of Lincolnshire, give, perhaps, the best returns on the east coast. The north of Norfolk is poor, but there are indications, in the heavy returns annually sent from the Llynwells, Dudgeon, Leman and Ower, and Happisburgh Lightvessels, that a dense stream pours along the coast from east to west, probably to pass inland by the estuary of the Wash and the river systems of the Nene and Welland into the centre of England, thence following the line of the Avon valley and the north bank of the Severn and Bristol Channel, and crossing the Irish Sea to enter Ireland at the Tuskar Rock, off the Wexford coast. This is apparently the great and main

[1] "Report on the Migration of Birds in the Spring and Autumn of 1886." McFarlane and Erskine, 19 St. James's Square, Edinburgh, price 2s.

thoroughfare for birds in transit across England to Ireland in the autumn. Large numbers of migrants also which pass inland from the coasts of Holderness and Lincolnshire may eventually join in with this great western highway by the line of the Trent, avoiding altogether the mountainous districts of Wales. The Norfolk seaboard between Cromer and Yarmouth and the corresponding lightvessels show a large annual immigration, but the returns are much less, and comparatively meagre between Yarmouth and Orfordness. The coast of Essex, with the northern side of the Thames, is fairly good; but the coast of Kent, between the North and South Forelands, including the four Goodwin and the Varne Lightships, is a barren and pre-eminently uninteresting district for arrivals, both as regards numbers and species, the chief migrants seen being such as are apparently following the coast to the south.

Such migrants, both local and otherwise, which in the autumn follow the east coast from north to south, seem, as a rule, to pass directly from the Spurn to the Lincolnshire coast without entering the Humber; and there are no indications that they follow the shores of the Wash in and out, but shape their course from about Gibraltar Point to the Norfolk coast. The well-filled schedules sent in annually from the Shipwash, Swin Middle, Kentish Knock, and Galloper Lightships, indicate that a stream passes from the south-east coast of Suffolk across the North Sea in the line of these stations, to and from the Continent, both in the spring and autumn.

Autumn migrants approaching the Humber from the sea do not appear to follow the course of the river into the interior, that is, from south-east to north-west. The line would seem to cross the river diagonally, and is from east-south-east to west-north-west. This course is so persistently followed that year by year, on such days when migration is visible, birds are observed to cross the same fields and at the same angle. Supposing this course to be continued, they would strike the Trent at or near Gainsborough.

Much information has been obtained from the legs and wings sent in the envelopes provided for that purpose; and by this means already several rare and unusual wanderers have been recorded, not the least interesting being the occurrence of a small Asiatic species, the yellow-browed warbler, at Sumburgh Head, Shetland, on September 25, and an immature example of the American red-winged starling, at 3 a.m. on October 27, at the Nash Lighthouse, Bristol Channel. This station, situated on the coast of Glamorgan and on the north side of the Bristol Channel, lies directly in the track of the great highway followed by migrants from England to Ireland. The black redstart was killed at the Nash Lighthouse on the night of October 29; and another interesting occurrence was that of the green woodpecker, seen on October 26, with many other birds at sunrise passing to the south-east.[1] The black redstart was also received from the Fastnet, co. Cork, found dead on October 30. It is also recorded at four other stations on the south coast of Ireland, and its regular occurrence in the winter on the south and east coasts of that island has now been fully established by this inquiry. The regular occurrence in migration of the black redstart both off and on the east coast of England, as well as the example from the Nash Lighthouse, are suggestive of the route followed annually by some small portion of this Continental species, which curiously select as their winter quarters the south-west coasts of the British Islands. From the Irish coasts the rarities received were numerous, including the second Irish specimen of the wryneck from Arran Island, co. Galway, killed striking at 2 a.m. on October 6. From the Tearaght, co. Kerry, a pied flycatcher was caught at the lantern, September 21, the species only having once before occurred in Ireland—in April 1875. The repeated occurrence of the corncrake, several miles from shore—killed striking against lanterns between 100 to 200 feet above sea-level—must satisfy the sceptical that this well-known species can fly at a high level with great power and velocity. The waterrail, which seems so unwilling to fly, was received from the Fastnet and Tuskar on October 26 and 28; also from Spurn Lightvessel, November 1, one; Llyn Wells Lightvessel, November 4, two; and Coquet Island Lighthouse, same date, one; showing a widely extended migratory movement of this species during the last week in October and early in November.

The great spotted woodpecker occurred in considerable numbers in the eastern counties of Scotland about the middle of October. Almost all the specimens examined were either old birds or with very slight traces of immaturity. This immigration extended southward to the coast districts of Lincolnshire, where very considerable numbers were obtained in the autumn and winter.

At Rathlin O'Birne (West Donegal) immense flocks of birds—starlings, thrushes, and fieldfares—passed *west* from December 18 to 23. The nearest land to the west of this rocky island is America. This is not an isolated occurrence. The westerly flight of land-birds at stations off the west coast of Ireland has been noticed on other occasions; the movement is apparently as reckless as that of the lemmings.

The autumnal passage of quails from England is shown by their occurrence at the Smalls Lighthouse, September 3, and the Eddystone on October 5; also a wing from the Shipwash Lightvessel, off the Essex coast, obtained on October 26.

An enormous rush of immigrants is recorded from the east coast of England on October 4, 5, and 6, with easterly and south-easterly winds, pressure system cyclonic, but the adverse meteorological conditions during this period slowly passing away. Much fog and thick weather at the time, which in a great measure may account for the immense numbers of birds seen at the lanterns of lighthouses. The movement was less apparent on the east coast of Scotland, the winds being east-north-east and north-east, having a tendency to crush down migration, giving it a more southerly direction. On the west coast of Scotland, during the same period, at the majority of stations the rush of birds was enormous; but the movement was much less accentuated on the west coast of England, and to a less degree still on the Irish coasts. The rush is by far the largest ever recorded since the opening of this inquiry.

As usual on the east coast of England, rooks, daws, hooded crows, starlings, and larks occupy a considerable portion of the returned schedules. Chaffinches have crossed the North Sea in extraordinary numbers. They are always numerous, but this autumn the immigration has been in considerable excess of previous years. With these exceptions, however, there has been a singular and very marked falling off in the migration of some species whose breeding range lies chiefly in the north of Europe. This has been especially noticeable in the small arrivals recorded of fieldfares, redwings, ring-ousels, bramblings, snow-buntings, short-eared owls, and woodcocks.

Eight reports have now been issued by your Committee, and the stations have again been supplied with the necessary papers for the returns of the observations in the present year. It seems highly desirable that an attempt should shortly be made to analyze, classify, and digest the large mass of facts brought together in these reports, so as to show, statistically and otherwise, the actual results which have been arrived at by the inquiry. It is intended that this shall be carried out at as early a date as possible. The Committee respectfully request their reappointment.

Report of the Committee, consisting of H. Seebohm, R. Trimen, W. Carruthers, and P. L. Sclater (secretary), appointed for the purpose of Investigating the Flora and Fauna of the Cameroons Mountain.—The Committee have the pleasure of reporting that a successful ascent of the Cameroons Mountain was made by Mr. H. H. Johnston on their behalf in the autumn of 1886. Mr. Johnston encamped at Mann's Spring, at an altitude of 7350 feet, about 300 feet above the forest region of the mountain, and remained there several weeks. A popular account of his expedition has been published, with illustrations, in the *Graphic* newspaper ("An Ascent of the Cameroons Mountain"). Mr. Johnston made considerable collections in zoology and botany. The zoological collections have been worked out by specialists in different branches, to whom the collections were referred by the Committee, and the results published in a series of papers in the Proceedings of the Zoological Society of London, of which the following are the titles:—

(1) "List of Mammals from the Cameroons Mountain, collected by Mr. H. H. Johnston," by Oldfield Thomas, Proc. Z.S. 1887, p. 121.

(2) "On a Collection of Birds made by Mr. H. H. Johnston on the Cameroons Mountain," by Capt. G. E. Shelley, Proc. Z.S., 1887, p. 122.

[1] Mr. H. Nicholas, of the Nash [East] Lighthouse, under date of September 3, has recorded an enormous arrival of small birds—the greatest number ever seen there at any one time. These include four nightjars at 2.10 a.m., one killed; fifteen to twenty common buntings from 2.15 to 3 a.m., eight killed; fifty to sixty greater whitethroats from 2.15 to 3 a.m., twenty-four killed; twenty to thirty willow wrens from 2.30 to 3.20 a.m., seventeen killed; six young cuckoos at 3 a.m., two killed; fourteen house sparrows and one robin killed at 3 a.m.; thirty to forty wheatears at 3.10 a.m., two killed; three blackbirds from 3 to 3.15 a.m., one killed.

(3) "List of the Reptiles collected by Mr. H. H. Johnston, on the Cameroons Mountain," by G. A. Boulenger, Proc. Z.S., 1887, p. 127.

(4) "On the Mollusca collected at the Cameroons Mountain by Mr. H. H. Johnston," by Edgar A. Smith, Proc. Z.S., 1887, p. 127.

(5) "On some Coleopterous Insects collected by Mr. H. H. Johnston on the Cameroons Mountain," by Charles O. Waterhouse, Proc. Z.S. 1887, p. 128.

It will be observed that, although the collections are small, they are by no means devoid of interest. Out of eighteen species of birds of which examples were obtained, four were new to science, and a new land shell, of the genus *Gibbus*, was also discovered. The zoological specimens have been placed in the collection of the British Museum. The botanical specimens collected by Mr. Johnston were sent by the Committee to the Kew Herbarium, where they were placed in Prof. Oliver's hands for determination. As was to be expected, although the specimens were in many case acceptable, they have added very little to our knowledge of the flora of the Cameroons Mountain. With few exceptions all Mr. Johnston's species, of which a complete list is given in the appendix to this Report, are enumerated in Sir Joseph Hooker's paper on Mann's plants of the Cameroons, published in the Journal of the Linnean Society in 1864 ("Bot.," vol. vii. p. 181). A complete set of the duplicates has been deposited in the Botanical Department of the British Museum, and a second set of duplicates has been sent to the Royal Museum of Berlin. The sum of £75, granted to the Committee at Birmingham, has been paid to Mr. Johnston as a contribution towards the expenses of his expedition. The Committee ask to be reappointed, and a further sum of £100 placed at their disposal, as Mr. Johnston will in all probability be able to undertake a second expedition up the Cameroons Mountain in the course of the present autumn.

Report of the Committee, consisting of Mr. Thiselton Dyer (secretary), Mr. Carruthers, Mr. Ball, Prof. Oliver, and Mr. Forbes, appointed for the purpose of continuing the preparation of a Report on our present knowledge of the Flora of China.—The grant made by the Association has enabled the Committee to proceed with this important work, the third part of which, carrying the enumeration down to the end of the Rosaceæ, is now in the hands of the printer, and the fourth part has been commenced. Since the work was begun, about two years ago, several collections of dried plants have been received at Kew from China, notably a very extensive one from Dr. A. Henry, made in the little-known district of Ichang, in the province of Hupeh, in the very centre of China. And the Trustees of the British Museum have acquired the herbarium of the late Dr. Hance, containing the types of the large number of species published by him from time to time during a long residence in the country. Dr. Henry's collection includes a large number of novelties, besides the addition of many Himalayan and Japanese forms not previously known, from China; and Dr. Hance's herbarium greatly facilitates the limitation of the species where comparisons with his types are necessary. The published parts of the report have been freely distributed among English residents in China, and have no doubt been the means of stimulating some of them to greater activity now that they perceive that there is a probability of the results of their exertions being promptly published. Dr. Henry is specially interested in the origin of the numerous drugs used in Chinese medicine, and, aided by our determinations of the plants, we may assume that he will be able to make a substantial addition to our knowledge of the Chinese pharmacopœia. Mr. Ford, too, the Superintendent of the Hong Kong Botanic Garden, takes a lively interest in the work, and has rendered valuable assistance, doubtless with advantage to the establishment under his charge. Several eminent foreign botanists have alluded to the work as of great interest and importance, and the Committee have much satisfaction in reporting that circumstances are now favourable to more rapid progress in the future than hitherto. Simultaneously with the appearance of our "Index Floræ Sinensis," a French botanist, M. Franchet, is publishing a very extensive collection of plants made by French missionaries in Yunnan, a province from which there is almost nothing in the London herbaria; hence his labours supplement ours, and cover a distinct floral region. The Committee recommend their reappointment, and that a further grant of £100 be placed at their disposal.

The Report of the Committee appointed to make suggestions with reference to the production of a bathy-hypsographical map of the British Isles and surrounding seas was presented by Mr. Ravenstein. Other members of the Committee were General Walker, Sir William Thomson, and Mr. A. Buchan.—The conclusions arrived at by the Committee were that the heights as well as the depths should be referred to the Ordnance datum level, and that contours of the land and ocean-bed should correspond. With regard to the various methods of tinting the maps so as to express height, it was proposed that the sea should be coloured blue, lowlands up to 500 feet green, the next region orange, the really mountainous parts brown, the depth of colour increasing with the height. Maps tinted in various ways were exhibited.

Report of the Committee, consisting of the Rev. Canon Carver, the Rev. H. B. George, Sir Douglas Galton, Prof. Bonney, Mr. A. G. Vernon Harcourt, Prof. T. McKenny Hughes, the Rev. H. W. Watson, the Rev. E. F. M. McCarthy, the Rev. A. R. Vardy, Prof. Alfred Newton, the Rev. Canon Tristam, Prof. Moseley, and Mr. E. G. Ravenstein (secretary), appointed for the purpose of co-operating with the Royal Geographical Society in endeavouring to bring before the authorities of the Universities of Oxford and Cambridge the advisability of promoting the study of Geography by establishing special Chairs for the purpose.—The Committee beg leave to report that, at a meeting held on January 12, 1887, at the office of the Association, the following resolutions were adopted:—(1) That the Committee fully recognize the educational value of the scientific study of geography, and are agreed in thinking that geography should occupy a place among the subjects of study at the Universities of Oxford and Cambridge. (2) That the Council of the British Association be requested to give their support to the representations and offers made to the Vice-Chancellors of the two Universities by the Council of the Society in letters dated July 9 and December 9, 1886, of which copies are inclosed.

Report of the Committee, consisting of Dr. J. H. Gladstone (secretary), Prof. Armstrong, Mr. Stephen Bourne, Miss Lydia Becker, Sir John Lubbock, Bart., Dr. H. W. Crosskey, Sir Richard Temple, Bart., Sir Henry E. Roscoe, Mr. James Heywood, and Prof. N. Story Maskelyne, appointed for the purpose of continuing the inquiries relating to the Teaching of Science in Elementary Schools.—Your Committee, in continuing their periodic reports upon this subject, have to state that nothing has been done this year in the shape of actual legislation, but that great advance has been made in regard to the public appreciation of the importance of scientific and technical instruction.

The only alteration in the Code of this year that at all bears upon the matter is that drawing is withdrawn from the list of class subjects, which gives an advantage to the claims of geography and elementary science by removing a powerful competitor in those schools that can only take two class subjects.

The return of the Education Department for this year shows that the diminution previously noted in the teaching of science subjects still continues.

The statistics of the class subjects for the four years are given in the subjoined table, which shows an actual decrease in geography and elementary science, notwithstanding the increase in the number of departments examined. It will be seen that drawing begins to figure in this year's return, but the effect of it will be much more apparent in that for next year.

Class Subjects.		1882–83.	1883–84.	1884–85.	1885–86.
English ...	(Departments)	18,363	19,080	19,431	19,608
Geography ...	,,	12,823	12,775	12,336	12,055
Elementary Science	,,	48	51	45	43
History ...	,,	367	382	386	375
Drawing ...	,,	—	—	—	240
Needlework ...	,,	5,286	5,929	6,499	6,809
		18,524	19,137	19,266	19,522

The return of passes in the scientific specific subjects on the individual examination of children shows again an actual falling off in the total, and either an actual or relative falling off in every subject except Mechanics, A. The large increase in the teaching of mechanics is due to the carrying out of the peripatetic method of teaching it by the School Boards of Liverpool Birmingham, Nottingham, and London. The figures are given in the following table:—

Specific Subjects.	1882–83.	1883–84.	1884–85.	1885–86.
Algebra ... (Children)	26,547	24,787	25,347	25,393
Euclid and Mensuration ,,	1,942	2,010	1,269	1,247
Mechanics, A ... ,,	2,042	3,174	3,527	4,844
,, B ... ,,	—	206	239	128
Animal Physiology ... ,,	22,759	22,857	20,869	18,523
Botany ... ... ,,	3,280	2,604	2,415	1,992
Principles of Agriculture ,,	1,357	1,859	1,481	1,351
Chemistry ... ... ,,	1,183	1,047	1,095	1,158
Sound, Light, and Heat ,,	630	1,253	1,231	1,334
Magnetism and Electricity ,,	3,643	3,244	2,864	2,951
Domestic Economy ... ,,	19,582	21,458	19,437	10,556
	82,965	84,499	79,774	78,477
Number of Scholars in Standards V., VI., VII. ...	286,355	325,205	352,860	393,289

The rapid and serious decrease of attention paid to these science subjects is shown by the percentage of children who have passed as compared with the number of scholars that might have taken these subjects, viz.:

In 1882–83	29·0 per cent.
,, 1883–84	26·0 ,,
,, 1884–85	22·6 ,,
,, 1885–86	19·9 ,,

and it must be remembered that when children have passed in two of these subjects they count twice over.

Of course a good deal of scientific instruction is given in many elementary schools under the name of object-lessons, not only in the infants', but also in the boys' and girls' departments; but this is neither examined by Her Majesty's inspector, nor encouraged by a grant except in the few cases where it comes in as a class subject under the name of elementary science. These object lessons are therefore very apt to be neglected. The same remark applies in the case of pupil teachers. It may be worthy of record that in the pupil-teachers' schools of the London Board natural history and the principles of physics are taught systematically in the junior division, and this year an examination has been held by the Board inspectors, and certificates of proficiency are to be awarded.

The Royal Commission appointed to inquire into the working of the Education Acts of England and Wales issued their first Report in August last, from which it appears that two of the points of inquiry bore directly upon the scope of this Committee. The one was, "Elementary Science: to what extent can it be taught in elementary schools?" The other, "Technical Instruction: as grants are made in girls' schools for needlework, why not for mechanical drawing and handicraft in boys' schools?" Another instalment of the evidence was issued in June last.

With reference to the first-named subject of inquiry, Her Majesty's inspectors and others who were examined appear not only of opinion that elementary science is of importance, but some maintain, with Matthew Arnold, that "*Naturkunde* should be a necessary part of the programme." Most of them agree with the view expressed by this Committee, that the absolute preference given to English as a class subject should be abolished, and the choice thrown perfectly open.

With reference to the second subject of inquiry, the evidence of Sir Philip Magnus, Dr. Crosskey, Mr. Hance (Clerk to the Liverpool School Board), and others is distinctly in favour of it, showing that it is both desirable and practicable.

It appeared to your Committee that the British Association should contribute its views on these subjects to the Royal Commission, and they accordingly passed a resolution to that effect. This met with the approval of the Council. Two of the members of the Committee have since given evidence. The Rev. Dr. Crosskey enforced strongly the importance of elementary science and technical instruction, and more recently Sir Henry Roscoe, as the mouth-piece of the Committee, presented a series of the reports of this Committee and a memorial emphasizing the two points of special importance, viz. as to the absolute preference given to English, and as to the want of provision for insuring the instruction of pupil-teachers in any kind of elementary science. The memorial also repeated their approval of the recommendation of the Royal Commission on Technical Instruction, "That proficiency in the use of tools for working in wood and iron be paid for as a specific subject, arrangements being made for the work being done, as far as practicable, out of school hours. That special grants be made to schools in aid of collections of natural objects, casts, drawings, &c., suitable for school museums."

An important meeting of gentlemen interested in popular education was held at the house of Mr. George Dixon, at Birmingham, last November, at which some of your Committee were present. This has led to several courses of action. The resolutions come to at this meeting were adopted in the following form by the School Board for Birmingham:—

I. That it is desirable that an enabling Bill should be introduced into Parliament to give School Boards power to provide and maintain schools in connexion with the Science and Art Department, in which a course of instruction extending over a period not exceeding three years may be given in accordance with its regulations, such schools to be open only to scholars who have passed the sixth standard in public elementary schools.

II. That in Article 113 of the Code of Regulations of the Education Department, affecting evening schools, Paragraphs IV., V., and VII. of sub-section (b) should be omitted. These paragraphs read thus:—"IV. No scholar may be presented for examination in the additional subjects alone. V. No scholar may be presented for examination in more than two of the additional subjects. VII. Scholars presented for examination in the third or fourth standard, if they take one additional subject, must take English; if they take two, the second subject must be drawing, geography, or elementary science."

III. That the words in Article 13 of the Code of Regulations of the Education Department, which exclude scholars who have passed the seventh standard from the number of grant-earning scholars, and also the words in the Instructions to Her Majesty's Inspectors which bear on this part of the said article of the Code, should be expunged.

These were afterwards brought before the Education Department on December 14 by a deputation of the Birmingham, Leicester, and Nottingham Boards, which was unofficially joined by members of the London Board. Two Bills have been brought into Parliament, and have passed their first reading. The one introduced by Sir Henry Roscoe relates to technical education (day schools), and embodies the substance of the above resolution, No. I. The other is introduced by Prof. Stuart, and relates exclusively to evening continuation schools, embodying the substance of Resolution No. II. Sir Richard Temple, the Vice-Chairman of the London School Board, also propounded a scheme by which technical and commercial instruction might be given in Board schools. Quite recently the Government have brought in a Bill dealing with the same subject, which has been read the first time.[1]

In consequence of the Government having given notice of their intention to introduce such a Bill this session, Mr. George Howell withdrew the resolution of which he had previously given notice—"That in the opinion of this House it is essential to the maintenance and development of our manufacturing and agricultural industries, in view of the rapidly increasing competition of other nations, both at home and abroad, and in consequence of the almost universal abandonment of the system of apprenticeship, that our national scheme of education should be so widened as to bring technical instruction, the teaching of the natural sciences, and manual training within the reach of the working classes throughout the country."

The Brighton School Board has opened an "Organized Science School," under the sanction of the Science and Art Department;

[1] This Bill of Sir Wm. Hart Dyke was read a second time with little opposition, though with some suggestions of amendment; but it had to be abandoned on August 18, on account of press of business.

but the official auditor has decided that all expenses incurred in respect of it are illegal, and has surcharged the Board with the balance not covered by the receipts. Appeal will be made to the Local Government Board against the decision of the auditor.

The experiment in manual instruction at Beethoven Street School was considered by the London School Board so successful that it was resolved to open five more classes of the same kind, but they were suspended in consequence of the official auditor having in the meantime surcharged the Board with the costs incurred for the workshop and tools. Appeal was made in November last against the surcharge of the auditor, but no answer has yet been received from the Local Government Board. The instruction is now being continued at Beethoven Street School, as a specific subject, with the concurrence of the inspector. That this subject finds favour with the elementary teachers is manifest from the fact that eighty of them have availed themselves of the opportunity offered by the City and Guilds of London Institute of qualifying themselves to give instruction in the use of tools, and many more applied who could not be accommodated.

The British and Foreign School Society have started a joinery class at their Training College in the Borough Road, which is attended by all the senior students, in which instruction is given both in the theory and practice of carpentry.

The London School Board on May 19 adopted, by a very large majority, the motion of the Rev. C. D. Lawrence—"That, in the opinion of this Board, it is necessary to introduce into elementary schools some regular system of manual training,"—and the matter was referred to a special committee on the subjects and modes of instruction in the Board's schools, which is now sitting.

The first examination by the Science and Art Department in the alternative first stage of chemistry has taken place, and may be considered to mark a great advance in the teaching of that subject. That the teachers were eager for such instruction is evident from the fact that as many applied for permission to attend Prof. Armstrong's course of lectures established by the City and Guilds of London Institute as that institution could be made to accommodate.

There has recently been formed a "National Association for the Promotion of Technical Education," which includes the leading politicians who have given special attention to the subject of education.

From this review of the present situation it would appear that the action of the Education Department tends positively to frustrate the efforts of those who desire to increase the teaching of natural science in elementary schools; but your Committee do not believe that that is the intention of those in authority, and feel sure that the great advance in public opinion will ultimately lead to a knowledge of the elements of science being made an essential part of all State-aided education.

Report of the Egyptian Photographs Committee.—A full account was given of the valuable work done by Mr. Flinders Petrie, on behalf of the Committee, in the early part of the year. In addition to the photographs, Mr. Petrie obtained 180 casts from paper squeezes of the sculptures, and from these casts photographs have been taken on a uniform scale; a full list of the casts and photographs is appended to the report.

Report of the North American Indian Committee.—This report contains a Circular of Inquiry drawn up for distribution amongst those most likely to be able to supply the Committee with information; and a Report on the Blackfoot Tribes, drawn up by the Rev. Edward F. Wilson, and supplemented by some notes by Mr. Horatio Hale.

Report of the Electrolysis Committee, by Dr. Oliver Lodge.—In laying before the Section this report of the Committee appointed by Sections A and B to consider the subject of electrolysis in its physical and chemical bearings, I should first say that, whereas the main lines of the report have been approved and ordered by the Committee, the details and wording have not yet received final sanction, so that if the first person singular should accidentally occur in places where it ought not, it is to be understood that the real official report is that which will appear in the annual volume of the Association, and that the present is to be regarded as merely a general outline of that report.

Work has been carried on during the past year by several members of the Committee; and nearly all the questions issued after the Aberdeen meeting by the secretary have been in some shape or other attacked.

The first, on the accuracy of Ohm's law of electrolysis, by Prof. FitzGerald and Mr. Trouton, who reported last year and will make a further report to-day.

The second, on conduction in semi-insulators, by Prof. J. J. Thomson and Mr. Newall. See the Proceedings of the Royal Society, No. 256, 1887.

On the third question, the mode of conduction of alloys, Prof. Roberts-Austen will inform us of his experiments to-day.

Mr. Shelford Bidwell has experimented on the subject of the fourth question, concerning the transparency of electrolytes.

The sixth, seventh, and eighth, on the velocity of ions, are being worked at by the secretary.

Concerning the ninth we have heard from Mr. J. Brown, of Belfast; and on the tenth we have had a letter from Prof. Willard Gibbs.

In order to enable the members of so large a Committee to work with some knowledge of what each other is doing, and also to keep up a general intercommunication and interest in the subject, it has been thought desirable and proper to spend a certain portion of the sum granted to the Committee in printing and postage. Periodical circulars have been sent among the members and to a few outsiders likely to be interested, and these have been the means of drawing out one or two communications of very distinct interest and value.

It is felt that such informal reports of discussion and free circulation of provisional communications are sufficiently useful to justify the Committee in continuing the practice, which was begun as an experiment, and they accordingly are asking for reappointment, with another grant of £50, of which not more than £20 is to be spent in printing and postage. They should explain that of the grant made last year to the Committee £20 has been purposely allowed to lapse, for it had been intended to try some chemical experiments on very pure substances, and these experiments have not yet been begun. The £30 applied for has been spent, about £15 in printing, £4 in postage, and £11 in experimental expenses contracted by the secretary.

Your Committee feel that the expenditure of a small sum such as this has acted, and may be expected to act, as a trigger capable of liberating in useful directions a considerable amount of energy, which otherwise might have remained potential.

There are several moot points at present more or less under discussion within the Committee, and I am instructed to lay them before this meeting with the object of eliciting some opinions, suggestions, or information.

First I may instance the obvious question whether electrolytic conduction and metallic conduction are sharply separated off from one another by a line of demarcation, so that no substance distinctly possessing one also possesses a trace of the other.

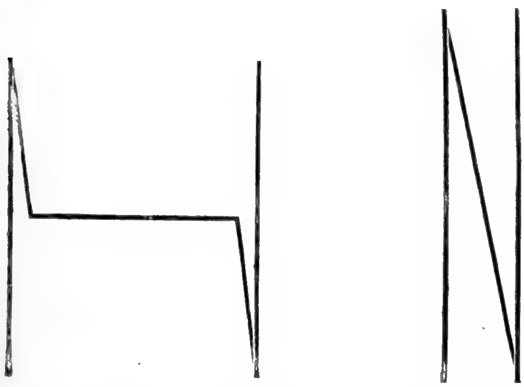
Certain contributions by von Helmholtz, among which we must reckon one on our list for to-day, lead one to believe that the conduction of ordinary electrolytes is *purely* electrolytic, and that no trace of current slips through them without carrying the atoms with it, *i.e.* without incipient decomposition.

A contribution expected from Prof. Roberts-Austen may perhaps answer the opposite question, viz. whether any ordinary metallic alloy can conduct in the least electr lytically—*i.e.* whether a well-marked metallic alloy or *quasi*-compound can be in the slightest degree electrolyzed by an exceedingly intense electric current.

Supposing both these questions answered in the simplest manner, viz. in the negative, there must surely remain a group of bodies on the borderland between alloys proper and electrolytes proper, among which some shading off of properties, some gradual change from wholly metallic to wholly electrolytic conduction is to be looked for. Until all such bodies as are tractable to experiment have been cautiously and strenuously examined, we are unable to say whether there is a hard and fast line between the two modes of conduction, or in what manner the gradation from one to the other occurs.

That is the first question. A second concerns the very vital point whether an electric current actually decomposes or tears asunder the molecules of a liquid through which it passes; or whether it finds a certain number of them already torn asunder or dissociated into their atoms by chemical, or at any rate non-electrical, means, these loose and wandering atoms thus falling an easy prey to the guiding tendency of the electric slope, and joining unresistingly one or other of two processions towards either electrode, only at the last moment attempting a brief and unavailing struggle, when the electrode suddenly looms foreign and forbidding across a molecular distance of 10^{-8} centimetres.

One mode of regarding the facts is to say that across this molecular range of 10^{-8} the electrical forces *are* competent to tear atoms asunder. The E.M.F. of a volt or so can be shown by calculation to be able to do this, so that the difference between an electrolyte and a dielectric may be typified diagrammatically thus :—



Electrolyte. Dielectric.

The two vertical lines are electrodes, the slant or broken line represents the slope of potential in the two cases respectively.

Prof. Schuster has now discovered one way in which dielectrics shade off into electrolytes; for he finds that in the neighbourhood of an electric discharge rarefied gases are able to conduct as electrolytically as liquids themselves. This discovery that the atoms of gas possess atomic charge as well as those of liquids, if confirmed by further research, is one of considerable interest.

But why do we assert the horizontality of the line of slope in the fluid? Why do physicists feel constrained to assert that no internal static electric stress is possible in the interior of a mass of fluid? The question is but the paraphrase of another—Why do we believe liquids to obey quite accurately Ohm's law for very minute forces? On this head we have direct experimental evidence by Prof. FitzGerald and Mr. Trouton, and less direct but equally conclusive evidence from von Helmholtz. Whether the evidence is perfect and thorough is doubtless a debatable point, but this much is not debatable: it is out of the question to assert that liquids obey Ohm's law and at the same time to assert the existence of a finite electrostatic stress in the interior of a fluid. In other words, however chemists are able to explain the fact of unresisting atomic processions through the liquid—whether by actual procession of individuals or by continual directed interchange—they will be rigorously driven to some form of such doctrine as soon as they accept the evidence for the accuracy of Ohm's law in electrolytic conduction.[1]

We all know that this doctrine of non-resistance is in some shape or another the old Williamson-Clausius hypothesis, which was based on then newly-known facts concerning dissociation.

It would appear, however, that some chemists demur to the existence of a constant average of dissociation among the molecules of a liquid; and it behoves us of Section A to receive their scruples with great respect, being, we may suppo.e, based upon intimate familiarity with all manner of circumstances and reactions of which we physicists are only superficially cognizant.

But there are ways of picturing all that is necessary to free atomic interchange without postulating actual and constant dissociation. A *potential* dissociation will be granted, sufficient for all purposes, provided chemists admit the probability of a frequent interchange of atoms among the molecules of an electrolyte going on always before any E.M.F. has been applied.

Concerning this or other mode in which electrolytic conduction takes place, we may congratulate ourselves on the presence here of Prof. Quincke and Prof. Wiedemann, and we hope to hear something from them. The experiments of Dr. Gladstone, and also some unpublished ones of Prof. J. J. Thomson, communicated to the Committee in a letter, will probably be found to have a bearing on this point.

[1] This sentence must be modified in the final report, because of some interesting observations of a controversial character made by Prof. FitzGerald at the meeting concerning it.

The question whether there is any radical distinction to be drawn between ordinary compounds and so-called molecular compounds appears to be an open one. Various physical facts lead one to suppose that whereas the ordinary forces of chemical affinity are strictly electrical there may be other non-electrical forces as well, and that such compounds as are held together by these latter forces are intractable to electrical influence. It is difficult for physicists to understand certain facts without the hypothesis of some non-electrical forces between atoms; but on such a subject as this chemists are likely to have in their hands evidence which, if at all decided and distinct, would be entitled to very great weight.

The subject of the partition of the current among different electrolytes when mixed together, and the question of the part the solvent plays in the conduction seem scarcely suitable for discussion at the present stage, because they only require a few rigorous experiments on lines already laid down to settle them. But I may just say that whereas at a former meeting I thought I had obtained experimental evidence that the water conducted some fourth part of the current in certain solutions, I have since found that, using purer substances, and taking extreme care to avoid loss of weight by spray, which source of loss is very subtle, this evidence puts on another complexion; and at the present time I am disposed to coincide more cordially with the orthodox view, that water conducts almost as little when forming part of a solution as when existing alone. Further experimental evidence is still being obtained, however, and perhaps Mr. Shaw has something to communicate on this head.

Among several communications received by the Committee from non-British philosophers is an exceedingly suggestive one by Prof. Willard Gibbs, which raises a very interesting point.

It is perfectly well known that in 1851 our present chairman, Sir William Thomson, reasoning from some experiments of Joule, taught us how to calculate the E.M.F. of a cell from thermo-chemical data,

$$E = \Sigma(J\epsilon\theta) ;$$

or

$$\frac{\Sigma\theta''}{46000} \text{ volts.}$$

Strictly speaking he hedged with regard to reversible heat effects in a way equivalent to the complete equation—

$$E = \Sigma(J\epsilon\theta) - \Sigma(J\Pi) \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Where Π_1 is the heat developed at junction 1 per unit quantity of electricity conveyed across it, Π_2 the same at the second junction, and so on.

But the value of Π, in any given case, is extremely difficult to measure, especially at metal-liquid and liquid-liquid junctions. Bouty has attempted it with but small success.

Fortunately Helmholtz has thought of applying the second law of thermodynamics to the subject, and shown that it was only necessary to know the rate at which the E.M.F. of a cell varied with temperature in order to know the sum of the Π. For, quite analogous to Prof. James Thomson's freezing-point relation—

$$dp\delta v = J\frac{dT}{T} L,$$

is the following E.M.F. relation—

$$dE\delta Q = J\frac{dT}{T}\delta H,$$

or

$$\Sigma\Pi = \frac{\delta H}{\delta Q} = \frac{T\,dE}{J\,dT} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Putting the two equations together we get—

$$E = JT\epsilon\int\frac{\theta\, dT}{T^2} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which we may say is certainly true.

But now Prof. Willard Gibbs suggests a novel mode of applying the second law or doctrine of entropy.

He takes into account the temperature of dissociation, or temperature at which the reaction could reversibly take place; and, calling this T_0, he writes the E.M.F. at any actual temperature T thus—

$$E = J\theta\epsilon\frac{T_0 - T}{T_0} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This he gives as the complete expression; wherein, therefore, $J\theta\epsilon$ is the chemical portion of the total E.M.F., and $J\theta\epsilon\frac{T}{T_0}$ the thermal portion of the whole E.M.F., equal to $J\Sigma\Pi$.

If this were a correct mode of regarding the matter, it would be of the highest interest to be able to calculate dissociation temperatures in this way. Unfortunately, several of the best judges in this country have expressed to the Committee their serious doubts as to the validity of thus stepping, unguided, outside the region of safe knowledge, across the great gap separating ordinary from dissociation temperatures. We wish Prof. Willard Gibbs were here to support and strengthen his position.

These are the main problems at present under discussion among the members of the Committee, and with this summary of them and reference to such of to-day's papers as seem likely to contribute towards their solution the report proper may be understood to close.

I think, however, I am only expressing the feeling of the Committee if I say that they view this joint sitting of Sections A and B with great interest, and with the anticipation and hope that it may be the precursor of many other such gatherings during the era of development in the borderland of chemistry and physics which in many directions they feel to be now imminent.

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE.

New Electric Balances, by Sir William Thomson, F.R.S.—These balances are founded on the mutual forces, discovered by Ampère, between the fixed and movable portions of an electric circuit. The mutually-influencing portions are usually circular rings. Circular coils or rings are fixed, with their planes horizontal, to the ends of the beam of a balance, and are each acted on by two horizontal fixed rings placed one above and the other below the movable ring. Six grades of instrument are made, named centi-ampere, deci-ampere, ampere, deca-ampere, hecto-ampere, and kilo-ampere balance. The range of each balance is about 25. Thus, the centi-ampere balance will measure currents of from 2 to 50 centi-amperes, while the kilo-ampere balance will measure currents of from 100 to 2500 amperes. Since the indications of the instrument depend on the mutual forces between two parts of an electric circuit of permanent form and relative position, they are not subject to the changes with time which are so troublesome in instruments the constants of which depend on the strength of permanent magnets.

The most important novelty in these balances is the connexion between the movable and the fixed parts of the circuit. The beam of the balance is suspended by two flat ligaments made up of fine copper wires placed side by side. These ligaments serve instead of knife-edges for the balance, and at the same time allow the current to pass into and out from the movable coils. The number of wires in each ligament varies from 20 in the centi-ampere to 900 in the kilo-ampere balance. The diameter of the wire is about $\frac{1}{10}$ of a millimetre, and each centimetre breadth of the ligament contains about 100 wires.

The electric forces produced by the current are balanced by means of weights which can be moved along a graduated scale by means of a self-relieving pendant. Two scales are provided—one a scale of equal divisions, the other a scale the numbers on which are double the square roots of the numbers on the scale of equal divisions. The square-root scale allows the current to be read off directly to a sufficient degree of accuracy for most purposes. When high accuracy is required, the fine scale of equal divisions may be used, and the exact value of the current obtained from a table of doubled square roots supplied with the instrument.

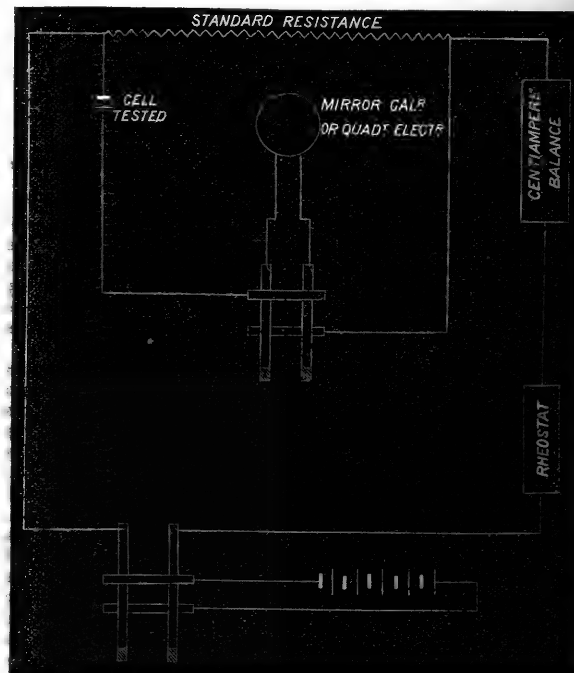
An engine-room voltmeter on a similar plan was described. It consists of a coil fixed to the end of a balance arm (suspended as above described) and acted on by one fixed coil placed below it. The distance apart of the two coils is indicated by means of a magnifying lever, and serves to indicate the difference of potential between the leads to which the instrument is connected. The coils of the instrument are of copper wire, and an external platinoid resistance of considerably greater amount is joined in circuit with it. The electrical forces are balanced by means of a weight placed in a trough fixed to the front of the movable coil and weights suited to the temperatures 15°, 20°, 25°, 30° C., as indicated by a thermometer with its bulb in the centre of the coil, are provided.

Two other instruments were described—namely, a marine voltmeter suitable for measuring the potential of an electric circuit on board ship at sea, and a magneto-static current-meter suitable for a lamp-counter.

In the marine voltmeter an oblate spheroid of soft iron is suspended in the centre of, and with its equatorial plane inclined at about 40° to, the axis of a small coil of fine wire, by means of a stretched platinoid wire. When a current is passed through the coil, the oblate of soft iron tends to set its equatorial plane parallel to the axis of the coil, and this tendency is resisted by the rigidity of the suspension wire.

The lamp-counter is a tangent galvanometer with special provision for preventing damage to its silk fibre suspension, and for allowing the constant to be readily varied by the user to suit the lamps on his circuit.

On the Application of the Centi-ampere or the Deci-ampere Balance for the Measurement of the E.M.F. of a Single Cell, by Sir William Thomson, F.R.S.—For the purpose of measuring the E.M.F. of a single cell, the centi-ampere or the deci-ampere balance is put in circuit with a battery of a sufficient number of cells, a rheostat, and a standard resistance in the manner shown in the diagram. The current measured by the balance is then



varied by means of the rheostat until the difference of potential between the ends of the standard resistance is exactly equal to the potential of the cell. The equality is tested by placing the cell in series with a mirror galvanometer or a quadrant electrometer in a derived circuit, the ends of which are connected with the ends of the standard resistance, and observing whether any deflection is obtained by closing this circuit. Suppose, for example, the standard resistance to be 10 ohms, and the current, as indicated by the balance, 0·108 amperes, when no deflection is obtained on the mirror galvanometer by closing its circuit, the potential of the cell is 10 × 0·108, or 1·08 volts. Proper precautions must, of course, be taken to eliminate thermo-electric or other disturbances in the circuit. The quadrant electrometer may be used with advantage in the derived circuit when it is important that no current should flow through the cell, but the mirror galvanometer has the advantage of much greater sensibility.

Conduction of Electricity through Gases, by Prof. A. Schuster, F.R.S.—A short time ago he communicated to the Royal Society the results of certain experiments which showed that, although a current can usually be sent through a gas only by employing a high electromotive force, yet a steady current can be obtained in air from electrodes which are at a difference of potential of only a fraction of a volt, provided that an independent current is maintained in the same closed vessel. He was now prepared to show the experiments in his laboratory.

On the Nature of the Photographic Star-Disks and the Removal of a Difficulty in Measurements for Parallax, by Prof. Pritchard, F.R.S.—If the telescope could be made to follow the star with perfect accuracy during the whole time of exposure of the photographic plate, the photographic image should be circular.

It is necessary, in order that photography may be of use, say in measuring parallax, that this condition should be approximately fulfilled. For it is from the centre of the star-image that the measurement must be made.

Now the image of a star exposed to a photographic plate driven by a clock having a small rate, and subject to small periodic oscillations, as is generally the case with the majority of driving-clocks, is not a simple linear trace, but a series of black dots joined together by intervals less dense. By hand-driving, those black dots, &c., coalesce or are superimposed. If for the purpose of measurement for parallax or otherwise a bright star be covered over by a stop during the greater part of the duration of the exposure of the plate, and the stop be then removed for a brief interval, it is shown by experimental measurement that the bright star is accurately represented on the plate.

Instruments for Stellar Photography, by Sir Howard Grubb, F.R.S.—He said that in considering the best form of object-glass for photographic purposes, one important point was to have as large a field as possible. An arrangement suggested by Prof. Stokes by which the first and second surfaces of refraction could be readily interchanged afforded the means of providing amateurs with an instrument which could be used at will either for ordinary or for photographic work. But by far the most important point in stellar photography was the clockwork. It was true that, no matter how perfect our clockwork might be, we could never dispense with the guidance of the hand. But if the clockwork be as good as it can be made, one is not tied down to the telescope as one is with bad clock-work. He thought it Utopian to expect that it might be possible to construct a clock accurate to 1/75 of a second, which some supposed to be necessary. He was at present able to correct any error greater than $\frac{1}{10}$ or at any rate than $\frac{1}{8}$ of a second, and he expected to be able to reduce the error to 1/20 of a second, but not farther. He did not believe that greater accuracy than this would be of any use.

In the course of a discussion which followed, Dr. Pritchard said he had actually traced the origin of the dots in the trail of his star-photographs to the periodicity of the screw.

On the Turbulent Motion of Water between Two Planes, by Prof. Sir W. Thomson, F.R.S.—He said that one of the most important questions in practical hydraulics was the flow or slipping of a liquid on a solid. He supposed for simplicity that there was nothing of finite slip of the fluid on the solid; but this was not essential to the reasoning. He considered the case of water flowing between two parallel planes; as an example of which a river with a plane bottom and covered with a sheet of ice might be taken. If the motion be laminar, *i.e.* free from turbulence, then the line of flow is represented by a parabola, supposing that there is no finite slip. The fluid moving in this way under the influence of gravity and possessing viscosity is in a state of stable equilibrium. If we suppose gravity and viscosity both suddenly reduced to zero, the motion becomes one of unstable equilibrium. The smallest amount of viscosity gives stability to the laminar motion, but the limits of stability are narrowed by either diminishing the viscosity, or increasing the effective component of gravity Osborne Reynolds made experiments some years ago on the flow of water through tubes. With great pressure there is always eddying, because the limits between which stable equilibrium is possible are narrow, and narrower the greater the pressure. He found that the laminar flow continued in a central film of the water for a certain distance, and then broke up suddenly into turbulence. Froude had made experiments on the resistance which a very smooth thin board met with in being moved at a uniform rate through water. His results show that, if one of two infinite planes (both at rest to begin with, and bounding a piece of water) be suddenly set in uniform motion, the water will at first move turbulently, and the turbulence will gradually pass into shearing motion. In conclusion Sir W. Thomson expressed the hope that candidates for the Adams prize would take up this subject.

Lord Rayleigh mentioned experiments which he had made on jets of coloured liquids and of smoky air. In the case of a jet projected into air the motion is unstable from the first, but the instability only shows itself at a certain distance. This distance diminishes with the velocity. He thought it possible that in Reynolds's experiments the instability was in like manner present from the first in the central film, and that the film remained distinct for a certain distance only in virtue of the purchase it had obtained.

On the Theory of Electrical Endosmose and Allied Phenomena, and on the Existence of a Sliding Coefficient for a Fluid in Contact with a Solid, by Prof. Lamb, F.R.S.—This paper deals with the laws governing the electric transport of conducting liquids through the walls of porous vessels or along capillary tubes, and other related phenomena, which have been investigated experimentally by Wiedemann and Quincke, and explained by the latter writer on the assumption of a contact difference of potential between the fluid and its solid boundaries. This explanation has been developed mathematically by Von Helmholtz. Applying the known laws of viscous fluids, he finds that the calculated results, so far as they depend on quantities which admit of measurement, are in satisfactory agreement with the experiments, and that the values which it is necessary to assign to the contact difference above spoken of are in all cases comparable with the E.M.F. of a Daniell's cell. Incidentally he arrives at the conclusion that in the cases considered there is no slipping of the fluid over the surface of the solid with which it is in contact. In the present paper a slightly different view is taken, and it is assumed that a certain finite (though possibly very minute) amount of slipping takes place, and that it forms an essential feature in the phenomena. The various cases considered by Von Helmholtz are treated on this assumption, and in some respects extended. In all cases the results differ from those obtained by Von Helmholtz by a factor $\frac{l}{d}$, where l is a linear magnitude measuring the "slip," viz. $\beta = \mu/l$, and d denotes the distance between the plates of an air-condenser whose capacity per unit area is the same as that of the apposed surfaces of solid and fluid. For example, by comparison with Wiedemann's experiments, Von Helmholtz infers that, for a certain solution of $CuSO_4$ in the pores of a clay vessel, E/D = 1·77, where D is the E.M.F. of a Daniell's cell. On the modified hypothesis adopted in the present paper, the inference would be that $\frac{E}{D} \cdot \frac{l}{d} = 1 \cdot 77$.

As this involves two unknown ratios, no such definite conclusion can be drawn, but it is evident that the phenomena are consistent even with very small values of E/D, provided l be a sufficient multiple of d. Since d is a quantity of molecular order of magnitude (comparable probably with 10^{-8} centimetres) the value of l may still be so minute as to render the effects of slipping quite insensible in such experiments as those of Poiseille. They come to be of importance in the cases at present under consideration only in consequence of the relatively great forces, due to the fall of potential along the course of the current, which act on the outer electrified layer of fluid and drag it over the surface of the solid.

On the Ratio of the Two Elasticities of Air, by Prof. S. P. Thompson, D.Sc.—Prof. Thompson said his paper would be chiefly interesting to those who had to teach thermo-dynamics to beginners. It was important to have some simple form of experiment for determining the ratio of the two elasticities of air, such as might readily be shown to a student. He described a simple form of apparatus which he considered more suitable than the usual one.

A Null Method in Electro-Calorimetry, by Prof. Stroud, D.Sc., and Mr. W. W. Haldane Gee, B.Sc.—The method is a modification of Joule's method for determining the specific heats of liquids and solids, possessing, however, the unique advantages of eliminating the correction for radiation as well as that for the thermal capacity of calorimeter and stirrer. The liquids for comparison in the two calorimeters are heated by wires carrying electric currents in such a way that the rises in temperature are the same in each case as tested thermo-electrically. The adjustment is effected by shunting the current through one of the calorimeters.

Prof. H. A. Rowland gave a description of a map of the solar spectrum. He said that for several years he has worked with concave gratings. He first tried a grating of 12 feet focal length, and the results were not as good as he thought they should be, so he then constructed a grating of 21 feet focal length, and the results he would be able to submit to the Section in his photographs. Having made the negatives, the next thing was to place the scale upon them. He first tried Ångström's numbers, but they would not match. He had therefore to determine the relative wave-lengths, and this he did by using overlapping spectra and micrometer measurements. As the spectrum was normal, all that was necessary was to get the scale to agree at two points of the photograph, and then it would agree at all. He had found it necessary to adopt a new scale. He was now engaged in making measurements in the red end of the spectrum in order to complete his work. This he is doing by the eye, and not by photography, as in this part of the spectrum photographs do not show so much as the eye. In laying the maps before the Section Prof. Rowland said that it would be seen how crowded the lines were in the ultra-violet region. He believed that on this account it would be almost impossible to determine the metals to which they belonged.

Capt. Abney thought it a serious thing to change the standard of wave-length, and suggested that a Committee of the Association should be appointed to confer with an American Committee on the subject.

Prof. Rowland said that Ångström's numbers do not agree among themselves, and therefore he could not fit a scale to his map in any way until he had made a new determination.

Prof. Young, of America, agreed with Prof. Rowland in thinking that Ångström's numbers were not consistent.

Mr. R. T. Glazebrook, F.R.S., exhibited negatives of photographs of the solar spectroscope taken by Mr. G. Higgs. The spectroscope was one constructed by Mr. Higgs himself. Twenty-one lines can be counted on the negatives between H_1 and H_2. There is a length of about 4 inches between G and H, and from 900 to 1000 lines can be counted between them.

Recent Determinations of Absolute Wave-length, by Mr. L. Bell, of Baltimore.—Some two or three years before Ångström's death he became aware that there was an error in the standard metre used in his researches. Nothing in the way of correction was done, however, until some three years ago, when Thalèn obtained a more accurate value for the metre and applied the appropriate correction to Ångström's wave-lengths. This amounted to 1 part in 8500.

Some few years ago a very careful determination was made by Mr. C. S. Pierce, and it was with the view of confirming or correcting his result that the writer began work. Pierce had found an absolute value corresponding to the wave-length 5896·26 for the less refrangible of the D lines, as the mean result from four gratings. The writer using two of Prof. Rowland's glass gratings, obtained for the same quantity respectively 5893·95 and 5896·11. The outstanding error must be ascribed to faults in the gratings. Nearly all gratings are afflicted with variations in the distance between the lines in various portions of the ruling. If the irregularity is extensive, the grating is likely to show false lines and give bad definition.

If, however, the abnormal spacing—however great—is confined to a few hundred lines, this portion, having little defining power, takes no part in the formation of the spectra actually seen, but simply scatters light, and of course introduces an error in the average of grating-space obtained by measuring the whole ruled surface.

The gratings used by the writer were therefore calibrated, and corrections calculated and applied to the above wave-lengths, reducing them to 5896·04 and 5896·09. The mean value taken was 5896·08.

On subjecting Pierce's gratings to a like examination, values nearly coincident with the above were obtained. During the present summer an admirable thesis by Dr. Kurlbaum has appeared giving results quite close to the writer's—about 5895·93 for the mean of two gratings, uncorrected, however, for errors of ruling.

We can, from the close agreement of the results obtained by Kurlbaum, Pierce (corrected), and the writer, feel sure that the wave-length of D is very near to 5896·00, and consequently all wave-lengths based on Angstrom's value are incorrect by at least one part in 8000. But this would not be a very serious matter if Ångström's relative wave-lengths were exact, which they are not.

On the Existence of Reflection when the Relative Refractive Index is Unity, by Lord Rayleigh, Sec.R.S.—He wished to find whether there was any reflection from a plate of glass immersed in a liquid of the same refractive index as the glass. The liquid used was a mixture of carbon bisulphide and benzole, and it was contained in a hollow prism. The glass plate was roughened behind to get rid of the second surface of reflection, and was mounted in the prism.

It was found that when the index was the same there was nothing like abolition of the reflection. The flame of a candle could be seen distinctly reflected in the glass. The phenomenon may be better followed by mounting the glass in such a way that it is possible to pass from a grazing to a perpendicular incidence. The ray for which the refractive index is made the same being chosen about the middle of the spectrum, as you alter the obliquity of the light, total reflection occurs for either end of the spectrum, and a dark band occupies the middle region. No doubt this band appears dark by contrast. In this way one could be certain that the index had really been equalized.

He next tried freshly-polished glass, and the reflection from it was not more than one-fourth of that from old glass, although the latter had been carefully cleaned. Still even from the polished glass the reflection was very copious. It did not need any care of adjustment in order to get the reflection of the candle-flame. The light so reflected was not coloured. There was a moderate reflection of all kinds. He confirmed this by using sunlight.

Where dispersion exists there is no reason to suppose that reflection should cease merely because the refractive index is equalized. If recently-fractured glass should give the same result, one might safely conclude that there was no residual film in play, and there would then be no doubt of the inaccuracy of Fresnel's law.

On the Action of an Electric Current in hastening the Formation of Lagging Compounds, by Dr. Gladstone, F.R.S.—The influence of the current was tried on various solutions from which, under normal conditions, precipitation takes place only slowly. A mixture of tartaric acid and potassium nitrate, a mixture of potassium oxalate and magnesium sulphate, a mixture of calcium sulphate and strontium nitrate, and some other mixtures were used. It was demonstrated that the current does hasten the action.

NOTES.

THE International Hygienic Congress at Vienna (attended by no fewer than 2250 members) was opened on Monday by the Austrian Crown Prince, who in a brief address referred to the vast importance of hygiene. After the Crown Prince's speech, which was much applauded, Prof. Grüber, the Hon. Secretary, read a report on the organization of the Congress. Two addresses were delivered by M. Brouardel and Herr Pettenkofer, the former of whom spoke on *typhus abdominalis*, the latter on hygienic instruction in Universities and technical schools. M. Brouardel said that the disease of which he spoke is far more dangerous to man than cholera, and that it is still an open question whether it owes its origin to the decomposition of organic matter or whether there is a specific virus. He maintained that in 80 cases out of 100 typhoid fever is caused by polluted water, and that the question of water supply must always take a foremost rank in hygienic administration. Herr Pettenkofer lectured on hygienic instruction in Universities and technical schools, and dwelt on the necessity of spreading hygienic principles among all classes of society. He largely quoted English authorities, and, in alluding to the English proverb "Cleanliness is next to godliness," remarked that the statistics of the mortality of London show how hygienic piety has been rewarded by the heavens. In the course of his address he dealt with the question of quarantine arrangements. He denied that the English are responsible for cholera coming to Europe through the Suez Canal. "This opinion," he said, "is clearly refuted by the fact that we were frequently visited by the disease before the Suez Canal was opened, and that since that time the epidemic has appeared in many European countries, while Great Britain, which now stands accused, and has suffered much through cholera in former times, remains free from it. Why do the English, in spite of their enormous traffic with India, where the cholera is never extinct, not transfer the disease to their own country? On looking more closely into the matter it must be admitted that England's immunity from cholera since 1866 is not caused by quarantines and other expensive obstructions to international traffic, and it is to be hoped that Italy, France, and Spain, as well as Russia, Germany, and Austria-Hungary, will follow England's example." The business of the Sections began on Tuesday. In the First Section, Mr. E. Frankland, who spoke in German, reported on the present state in England of the purification of sewage, with special reference to the prevention of river pollution, and on the legislation connected therewith. In the Second Section the necessity of placing schools under medical supervision was discussed, especially with reference to the prevention of the spread of infectious diseases and shortsightedness. The influence of drinking-water in the production and spread of epidemic disease was fully discussed in the Third Section. In the Fourth Section, Dr. Strulens, of Belgium, read a paper on phosphor necrosis of the jaws, and M. Violi, of Constantinople, a paper on vaccination and anti-vaccination. In the Demographical Section, M. Bertillon, of Paris, discussed the papers sent in by Dr. Grimshaw, of Dublin, and Prof. Koeroesi, of Buda Pesth, on the methods of drawing up census returns. There was an animated discussion, which was brought to a close by a resolution accepting the regulations of the International Statistical Institute. On Tuesday it was decided that the next meeting of the Congress should be held in London in 1889.

AT the meeting, in Toulouse, of the French Association for the Advancement of Science an address of welcome was delivered by the Mayor of the town, President of the Local Committee. M. Rochard, President of the Association, delivered an address on the future of hygiene. The annual report and the annual financial statement were read—the former by M. Schlumberger, Secretary, the latter by M. Galante, Treasurer. The most important subject dealt with in the annual report was the recent fusion of the French Association for the Advancement of Science with the Scientific Association of France.

WE learn from the *Montreal Gazette* that letters to date of July 29 have been received from Dr. G. M. Dawson, who is in charge of the geological party exploring the Yukon district. The party constructed two boats on Dease Lake, and left on June 3 to descend the Dease River to its junction with the Liard. From that place Mr. McConnell left with two men to descend the Liard. The remainder of the party, with five Indians, ascended the north fork of the Liard to Lake Francis, and leaving their boats crossed a long portage of sixty miles to Pelly River near the abandoned Hudson's Bay post of Pelly banks, where they arrived on July 29, all well. From this place the Indians were sent back, and Dr. Dawson, with Mr. McEvoy and two white men, remained to construct a boat and descend the Pelly to its junction with the Yukon. The country north of Dease Lake proved somewhat varied in structure, having a granitic nucleus with Palæozoic rocks on its flanks ranging from Cambrian to Carboniferous, and overlying Tertiary beds. The old portage was found to be entirely disused, and the party had to struggle through tangled woods, often knee-deep in moss. They got over, however, with a month's supply of provisions for the advancing party, and leaving stores for the returning Indians. Being north of the latitude of 60°, they enjoyed almost perpetual daylight, and the weather was good. The country is described as possessing well-grown trees, and a great number of the ordinary eastern plants were seen in flower, with some northern and western strangers. Only the great growth of sphagnous mosses and the abundance of reindeer moss give the country a different aspect from that of British Columbia. No Indians had been seen, except those the party brought with them from the coast. Though somewhat later in the season than he had expected to be, Dr. Dawson had hopes of reaching the coast before the freezing of the rivers, and the lines of section made by Mr. McConnell and himself will give a good general idea of the structure and resources of the country.

A PAPER on "Chemical Teaching" was read before the Chemical Section of the British Association at the recent meeting at Manchester. The paper was followed by a discussion wherein it was made apparent that teachers of chemistry are very dissatisfied with the methods now in use, and are anxious for great and wide-reaching changes. A Committee was appointed by the Association to inquire into and report on the methods adopted for teaching chemistry. The Committee consists of representatives of the universities and colleges, and also of the schools and technical institutions where chemistry is taught. The Committee is to begin its work by gathering facts regarding the courses of chemical teaching given in the various institutions where chemistry forms a part of the curriculum.

DR. CLEMENS WINKLER publishes in No. 15 of the *Journal für praktische Chemie* an account of his latest work upon the new element germanium, recently discovered by him in the Freiberg mineral argyrodite. In his first announcement last year, Dr. Winkler stated that the metal was obtained by reduction of the oxide in a stream of hydrogen gas, but since that time large quantities of the mineral have been found and dealt with on a much larger scale. The powdered oxide, after undergoing an elaborate process of purification, is intimately mixed with 15 to 20 per cent. of starch-meal, made into a paste with boiling water, and rolled into balls. These balls are then placed in a crucible in contact with powdered wood charcoal and heated to redness; on cooling, each ball is found to be converted into a regulus of metallic germanium. After removal of the adhering charcoal they are placed in a second crucible, covered with a layer of powdered borax-glass and melted in a gas furnace, when they fuse together to a single brittle regulus, fine octahedral crystals being formed at the outer surface. Among the numerous compounds of germanium prepared by Dr. Winkler, two are of great importance, as conclusively indicating the position of this new element in the periodic system. The first is germanium chloroform, $GeHCl_3$, analogous to the similar well-known compounds of carbon and silicon, which is obtained by gently heating germanium in a stream of dry hydrochloric acid gas; the metal glows and continues to do so after removal of the lamp, the chloroform passing along with the excess of hydrochloric acid, and being condensed to a liquid by means of a freezing mixture of ice and salt. The second is germanium ethide, $Ge(C_2H_5)_4$, analogous to the ethides of silicon and tin, which is obtained by the action of two volumes of zinc ethide upon one volume of germanium tetrachloride. The operation is performed in an apparatus filled with carbonic acid gas, and the reaction is very violent; if, however, the temperature be kept down by immersion in cold water, the action is more regular, and after 2 or 3 hours the whole solidifies. On the addition of water, gas is evolved and a layer of the oily ethide separates out; when pure, it is colourless and of weak garlic odour, slightly lighter than water, and boils at 160°. It burns with an orange-coloured light, giving off white clouds of the oxide. There can no longer be the slightest doubt that the gap in the periodic table between silicon and tin must be occupied by germanium, for Dr. Mendelejeff predicted that the metal thus filling up this particular gap would be found to form, if discovered, a tetrethide of specific gravity about 0·96 and boiling at 160°.

WE notice, from the prospectus of the University College (London) Engineering Department, that the work of this College begins for the session on October 5. The instruction in surveying and the lectures on the various branches of civil engineering are given by Prof. L. F. Vernon Harcourt. The general lectures on engineering and machine design, as well as the work in the engineering laboratory, are in the hands of Prof. Alex. W. Kennedy. In this laboratory, the arrangements of which formed a principal subject of the paper on the use and equipment of engineering laboratories read by Prof. Kennedy before the Institute of Civil Engineers last winter, students go through for themselves, during the session, a systematically arranged course of experimental work in connexion with elasticity and the strength of materials, the efficiency and economy of steam-boilers and engines, the appliances for which have been considerably extended during the last few months. Electrical technology is under the care of Prof. Fleming, by whom (with Prof. Carey Foster) a dynamo installation has lately been fitted up for the purpose of practical experimentation in applied electricity. Building construction forms the subject of lectures by Prof. T. Roger Smith, as a part of his course on architecture. Economic geology is treated as a special subject in a short course of lectures by Prof. T. G. Bonney, and chemistry as applied to engineering and architecture in a course by Prof. Chas. Graham. In addition to these matters directly connected with engineering, the College provides ample instruction in all the sciences on which engineering is based—mathematics, mechanics, physics, chemistry, geology, &c., and very special attention is given to graphic methods of calculation as applied to scientific and technical problems in the lectures and drawing class of Prof. Karl Pearson.

We have received the Calendar of the Glasgow and West of Scotland Technical College for the ensuing session. It is proposed, we observe, to make extensive additions to "Allan Glen's School," especially by providing laboratories, rooms for freehand and mechanical drawing, and a large workshop. These will not be fully ready for use until the session of 1888–89. No change will be made in the course of study pursued since its reorganization in 1878.

The session of the Science and Technical Classes at the Royal Victoria Hall, Waterloo Bridge Road, begins on Tuesday, October 4, when a lecture on "Museums for the People" will be delivered by Prof. H. G. Seeley, F.R.S. After the lecture, prizes will be distributed, and Dr. J. A. Fleming will give an address on the importance of scientific teaching. The lecture arrangements for the remainder of the month are as follows:—October 11, Prof. Kennedy, F.R.S., "Camping out in Wyoming"; October 18, Rev. Blomfield Jackson, "Rome and its Ruins"; October 25, Prof. A. W. Rücker, "A Ship's Compass." From lectures such as these has sprung the desire for systematic teaching which has resulted in the formation of the classes. These are held in rooms at the back of the stage, a new room having been built during the summer, to prevent inconvenient crowding in some of the classes. The subjects comprise arithmetic, mathematics, animal physiology, applied mechanics, machine construction and drawing, electricity and chemistry, with the possible addition of physics and astronomy. Many of the classes are in connexion with the Science and Art Department, and the fees (1*s.* 6*d.* per class per session, with an entrance-fee of 1*s.* for new students) are suited to the working-class neighbourhood where the Hall is situated.

A course of about eighteen lectures on "Agriculture" will be delivered during the ensuing winter session at King's College, London, by Mr. Frederick James Lloyd.

In the *Bollettino* of the Italian Geographical Society for July Dr. G. A. Collini describes some important additions recently made to the Prehistoric and Ethnological Museum of Rome. These include a part of the collections made by Count Giacomo di Brazzà Savorgnan and the Cavaliere Attilio Pecile during their late explorations in the Ogoway and Lower Congo basins. Although most of the objects remain in Paris, enough was secured for the Roman Museum to illustrate the arts and industries of numerous African peoples about whom next to nothing was known till quite recently. The objects are divided into two distinct categories: the first comprising the industrial and artistic products of the Fans, Adumas, Obambas, and Ondumbos of the Ogoway; the second those of the Bakongos, Bayanzi, and Batekes of the Congo, the Apfurus and Mboshi of the Alima, the Mbokos of the Likwala, and even some tribes of the lately discovered Ubanghi. Both classes contain personal ornaments, skins, fabrics woven of the raphia fibre, shields, hunting and fishing nets, musical instruments, earthenware remarkable for its correct forms, varied colours and artistic designs, besides a great diversity of iron implements and weapons such as axes, hatchets, spears, darts, hoes, knives, razors. The great skill possessed by these natives in wood-carving is shown by the spoons, domestic vessels, idols, stools, canoes, and other wooden objects included in this valuable collection.

Messrs. Marion and Co., of Soho Square, inform us that they have just introduced a set of universal 10 per cent. developing solutions for the use of photographers. The complicated constructions which accompany the majority of dry plates are made perfectly simple by them; and what is of more importance, the inconvenience of keeping different solutions for each kind of plate used is also done away with. Nothing can be simpler than their plan of first ascertaining the proportions of each constituent in the developer required, and then mixing solutions of known strengths in like proportions. The solutions are contained in three bottles, on each of which is given a list of the plates in common use and the quantity of solution required for each.

Messrs. Cassell and Co. announce a new edition of "Colour," by Prof. A. H. Church; a cheap edition of Prof. H. G. Seeley's "History of the Fresh-water Fishes of Europe," and of "Short Studies from Nature"; the completion of "Familiar Garden Flowers," by Shirley Hibberd; the third series of "Familiar Wild Birds," by W. Swaysland.

We understand that "A Quekett Club Man" is engaged upon another microscopical manual, "The Student's Hand-book to the Microscope," which will treat practically of the working of the instrument. Another well-known microscopist, Mr. T. Charters White, F.R.M.S., &c., is preparing a treatise on the mounting of objects. Both works will be published shortly by Messrs. Roper and Drowley.

We have received an interesting little pamphlet on the natural history of the coast of Lancashire by Dr. Thomas Alcock (Heywood, Manchester). The portion of the coast treated is that extending from the mouth of the Wyre nearly to the estuary of the Mersey, including Fleetwood, Blackpool, St. Anne's, Lytham, and Southport, although most attention is devoted to the latter place. The work is written in a popular style, and we can imagine no more interesting guide for visitors to Lancashire watering-places, even if their taste for natural history studies is imperfectly developed.

The September Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, presents some facts about annatto (to which attention was called in the Bulletin for July), and a number of valuable notes on articles contributed to the Museums at Kew from the Colonial and Indian Exhibition, 1886.

The Signal Service of the United States lately ordered the abandonment of the following stations on the Pacific coast:—Monterey, San Luis Obispo, Bakersfield, Modesto, Indio, San Bernardino, Carson, Yreka, Santa Rosa, and Mendocino City. *Science* says that as soon as this order was made known the publisher of the San Francisco *Chronicle* came forward and offered to provide observers, pay for telegrams, warnings, and so forth, if the Government would allow the instruments to remain. The offer has been accepted.

WE regret to announce the death of the Rev. W. S. Symonds, well known as a geologist and archæologist. He died at Cheltenham on September 15, and was buried at Pendock. He was sixty-nine years of age.

THE additions to the Zoological Society's Gardens during the past week include a Pig-tailed Monkey (*Macacus nemestrinus* ♂) from Java, presented by Mr. S. P. Grieve; a Black-eared Marmoset (*Hapale penicillata*) from South-east Brazil, presented by Mr. J. J. Foster; a White-collared Mangabey (*Cercocebus collaris*) from West Africa, presented by Mr. W. Tudor; a Vulpine Phalanger (*Phalangista vulpina*) from Australia, presented by Mr. Oscar F. Armytage; a Red Fox (*Canis fulvus*) from Canada, presented by Miss Cameron; a Common Jackal (*Canis aureus*) from Ceylon, presented by Capt. W. J. Geake; a Chinese Jay Thrush (*Garrulax chinensis*) from China, a Crested Lark (*Alauda cristata*) from India, presented by Colonel Verner; a Pale-headed Tree Boa (*Epicrates angulifer*) from Bahama, presented by Mr. H. A. Blake; an Alligator Terrapin (*Chelydra serpentina*) from North America, presented by Mr. G. S. Blythe; an Aldrovandi's Skink (*Plestiodon auratus*) from North Africa, presented by Mr. Arthur Colls; a Raven (*Corvus corax*), British, deposited; two Crested Pigeons (*Ocyphaps lophotes*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE CORDOBA OBSERVATORY.—The sixth volume of the observations of the Cordoba Observatory, which has recently been published, is mainly occupied by the observations made in the year 1875 for the great Zone Catalogue which Dr. Gould brought to so successful a conclusion. In all, 38,121 stellar positions were obtained in the year 1875, of which 22,315 were made in zones, the zones bearing the numbers 620 to 754. The remaining observations comprised 12,661 observations of 4373 stars for the General Catalogue, 1463 of circumpolar stars, and 1682 of stars for clock error. The individual members of certain star clusters were also observed. The volume illustrates forcibly, as do all the volumes issued by Dr. Gould, the energy, thoroughness, and system with which he carried out the great enterprise he had undertaken. Every care was taken that the observations should be as accurate, as well as numerous, as possible. For the General Catalogue stars, all four microscopes were read and the transits taken over three tallies of transit threads. For each zone, two time-stars and a circumpolar at upper and at lower transit were observed before the beginning of the zone, and the same after, together with observations of level, collimation, and nadir point. The separate determinations of the places of the stars for the General Catalogue are given, as well as their mean places in catalogue form. The tables used in the reduction of the various zones are also printed, together with corrigenda tables for the present and previous volumes, and an index to the 135 zones of this volume.

NEW MINOR PLANET.—A new minor planet, No. 269, was discovered by Herr Palisa on September 21 at Vienna. This is the sixtieth discovered by this observer.

OLBERS' COMET, 1887.—The comet discovered by Mr. W. H. Brooks on August 24 is now evidently the expected comet of Olbers, 1815 I. The following ephemeris for Paris midnight is by M. Lebeuf (*Astr. Nach.*, No. 2805):—

1887.		R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Oct.	1	11 30 59	27 41·3 N.	0·0757	0·2731	1·71
	3	41 2	27 12·4			
	5	51 0	26 41·1	0·0735	0·2714	1·74
	7	12 0 54	26 7·5			
	9	10 42	25 31·9	0·0724	0·2709	1·75
	11	20 23	24 54·2			
	13	29 57	24 14·8 N.	0·0726	0·2715	1·75

The brightness on August 27 is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 OCTOBER 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 October 2

Sun rises, 6h. 4m.; souths, 11h. 49m. 23·8s.; sets, 17h. 35m.; decl. on meridian, 3° 33′ S.: Sidereal Time at Sunset, 18h. 19m.

Moon (Full October 2, 4h.) rises, 18h. 11m.; souths, 0h. 31m.*; sets, 7h. 2m.*; decl. on meridian, 3° 16′ N.

Planet	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian ° ′
Mercury	7 35	12 47	17 59	10 4 S.
Venus	4 52	10 37	16 22	3 40 S.
Mars	1 35	8 59	16 23	15 11 N.
Jupiter	8 47	13 41	18 35	13 24 S.
Saturn	23 56*	7 45	15 34	19 22 N.

* Indicates that the rising is that of the preceding evening and the southing and setting those 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. ° °
2	15 Ceti	6½	1 9	1 50	192 260
2	35 Ceti	6½	18 15	near approach	161 —
4	μ Ceti	4	19 15	near approach	161 —
6	75 Tauri	6	22 45	23 39	102 215
6	θ¹ Tauri	4½	22 54	23 8	350 325
6	θ² Tauri	4½	23 4	near approach	338 —
6	B.A.C. 1391	5	23 45	0 40†	29 295
7	Aldebaran	1	3 20	4 2	42 354
8	71 Orionis	5½	23 56	1 1†	59 237

† Occurs on the following morning.

Oct.	h.	
8	1	Mercury at greatest distance from the Sun.

Saturn, October 2.—Outer major axis of outer ring = 39″·7; outer minor axis of outer ring = 13″·0; southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Oct. 8,	4 53	*m*
Algol	3 0·8	40 31 N.	,, 4,	0 50	*m*
			,, 6,	21 39	*m*
λ Tauri	3 54·4	12 10 N.	,, 4,	20 20	*m*
V Cancri	8 15·3	17 39 N.	,, 8,		*M*
S Cancri	8 37·5	19 26 N.	,, 7,	2 55	*m*
δ Libræ	14 54·9	8 4 S.	,, 3,	2 47	*m*
U Coronæ	15 13·6	32 4 N.	,, 6,	19 42	*m*
W Ophiuchi	16 15·3	7 26 S.	,, 7,		*M*
U Herculis	16 20·8	19 9 N.	,, 6,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 2,	21 46	*m*
		and at intervals of		20 8	
X Sagittarii	17 40·5	27 47 S.	Oct. 6,	0 0	*m*
			,, 8,	21 0	*M*
W Sagittarii	17 57·8	29 35 S.	,, 4,	23 0	*M*
R Scuti	18 41·3	5 50 S.	,, 2,		*M*
β Lyræ	18 45·9	33 14 N.	,, 4,	20 0	*M*
U Capricorni	20 41·9	15 12 S.	,, 6,		*M*
T Cephei	21 8·1	68 2 N.	,, 7,		*m*

M signifies maximum; *m* minimum.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, September 7.—Dr. Sharp, President, in the chair.—Mr. Arthur Sidgwick, M.A., was elected a Fellow.—Mr. Jenner-Weir exhibited a living larva of *Myrmeleon europæus*, which he had taken at Fontainebleau on August 6 last.—Mr. Elisha exhibited a series of bred specimens of *Zelleria hepariella*, Stn., and also, on behalf of Mr. C. S. Gregson, a long series of varieties of *Abraxas grossulariata*. Mr. Stainton remarked that the female of *Zelleria hepariella* had until lately been considered a distinct species, and was known as *Z. in-*

signipennella, but directly Mr. Elisha began breeding the insect its identity with *Z. hepariella* was established.—Mr. Tutt exhibited specimens of *Crambus alpinellus*, *C. contaminellus*, *Lita semidecandrella*, *L. marmorea*, and *L. blandulella* (a new species), *Doryphora palustrella*, and *Depressaria yeatiana*, all collected at Deal during last July and August. Mr. Stainton observed that *Crambus alpinellus* was so named from the earliest captures of the species having been made on the lower parts of the Alps, but that it had since been found on the low sandy ground of North Germany, and its capture at Deal quite agreed with what was now known of the distribution of the species in Germany. It was first recorded as a British species by Dr. Knaggs in 1871. Mr. Stainton further observed that he had named Mr. Tutt's new species *blandulella*, from its similarity to a small *maculea*, of which one of the best known synonyms was *blandella*. He also remarked that Deal was a new locality for *Doryphora palustrella*, which had hitherto only been recorded from Wicken Fen and the Norfolk Fens in England.—Mr. Waterhouse exhibited a variety of *Lycæna phlœas*; also a number of *Stenobothrus rufipes*, and three specimens of *Coccinella labilis*.—Mr. M. Jacoby exhibited several species of *Galerucidæ*, belonging to a genus which he proposed to call *Neobrotica*, closely resembling in shape and coloration certain species of *Diabrotica*, but differing therefrom in structural characters. He remarked that the late Baron Von Harold had described a *Galeruca* from Africa, which, except in generic characters, exactly resembled the South American genus *Dircema*.—Dr. Sharp communicated a paper, by Mr. T. L. Casey, "On a new genus of African *Pselaphidæ*."—Mr. Bridgman communicated a paper entitled "Further Additions to the Rev. T. A. Marshall's Catalogue of British *Ichneumonidæ*."—Mr. Distant read a paper entitled "Contributions to a Knowledge of Oriental *Rhynchota*."—Mr. Enock read notes "On the Parasites of the Hessian Fly," and exhibited specimens of injured barley. A discussion ensued, in which Dr. Sharp, Mr. Jacoby, Mr. Billups, Mr. Waterhouse, and others took part.

Paris.

Academy of Sciences, September 19.—M. Hervé Mangon in the chair.—Remarks accompanying the presentation of a copy of his treatise on "Thermo-dynamics," by M. J. Bertrand. Reference is made exclusively to the function long known to physicists under the name of Carnot's function, and the principle of which was accepted by Carnot's pupil Clapeyron. The author has sought, for the general case, the form that, according to their principles, Carnot and Clapeyron should have given to this unknown function, which they themselves did not determine. This form, as here rigorously deduced from those principles, is found to be very different from that which the progress of science has caused to be generally accepted.—Observations on the rotation of crops, by M. P. P. Dehérain. The system generally adopted in the North of France lasts five years, beginning with beetroot or potatoes, and followed by wheat with clover sown in the spring and yielding two crops the third year. The ground being then broken in the autumn, is again prepared for wheat, followed in the fifth and last year by oats. In this system two crops are here shown to be badly placed, the first wheat succeeding badly after beetroot, and oats badly after the second wheat. The author's experiments prove that the four years' rotation, as practised in England, and known as the Norfolk system, is in every way the best and most profitable.—Provisional elements of Brooks's new comet (August 24), by MM. Rambaud and Sy. These elements, based on the observations made at the Observatory of Algiers during the period from August 29 to September 2, are as under:—

$$T = 1887 \text{ October } 13{\cdot}9499$$

$$\pi = 157^\circ\ 54'{\cdot}5$$
$$\Omega = 85\ 39{\cdot}8$$
$$\iota = 45\ 58{\cdot}1$$
$$\log q = 0{\cdot}05717$$

Representation of the mean observation O − C

$$\Delta\alpha \cos\beta = +0''{\cdot}2,\ \Delta\beta = 0''{\cdot}0.$$

—Observations of the same comet made with the Brunner 6-inch equatorial at the Observatory of Lyons, by M. Le Cadet.—On the organization of the astronomical service in the United States, by M. A. Laussedat. The author's remarks, made in connexion with a recent visit to the Naval Observatory of Washington, deal more especially with the chronometer department, and with the arrangements made for transmitting the astronomical time to all the chief ports on the Atlantic sea-board. The same current corrects at noon the three or four hundred clocks in the public offices, schools, and other establishments in Washington. Certain important services, such as those of the Signal Service, the Coast Survey, and fire-stations, are directly connected by telegraph wires with the Naval Observatory, while private houses and firms can also obtain the time by paying a yearly subscription to the Telegraph Company.—On the reduction of alumina, by M. G. A. Faurie. Two parts of pure and finely-powdered alumina with one of petroleum or other hydrocarburet are worked into a paste, which is well kneaded, and one part of sulphuric acid added. When the mass becomes homogeneous, with a uniform yellow colour, and begins to liberate sulphuric acid, it is put in a paper bag, and placed in a crucible heated to a good red over 800° C., so as to decompose the petroleum. After cooling, the product thus obtained is carefully pulverized, mixed with its weight of a powdered metal, placed in a well-closed crucible in plumbago, and brought to a white heat with the blow-pipe. After again cooling, more or less rich grains of aluminium alloy will be found in the middle of a black metallic powder. The process is equally applicable to silica, lime, magnesia, &c.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Photographer's Indispensable Hand-book.—Annals of Botany, vol. i. No. 1 (Clarendon Press).—The Earth in Space: E. P. Jackson (Heath, Boston).—British Dogs, No. 11: H. Dalziel (Gill).—Bees and Bee-keeping, vol. ii. No. 12: F. R. Cheshire (Gill).—The State—The Rudiments of New Zealand Sociology: J. H. Pope (Wellington).—The Realistic Teaching of Geography: W. Jolly (Blackie).—Results of Meteorological Observations made in New South Wales during 1885: H. C. Russell (Sydney).—Morse Collection of Japanese Pottery (Salem).—The Advance of Science, Three Sermons (John Heywood).—Proceedings of the Academy of Natural Science of Philadelphia, Part 1, 1887 (Philadelphia).—Proceedings of the American Philosophical Society, vol. xxiv. No. 125 (Philadelphia).—Bulletin of the U.S. Geological Survey, Parts 34–39 (Washington).—Beobachtungs Ergebnisse der Norwegischen Polarstation Bossekop in Alten, i. Theil (Christiania).—Transactions of Vassar Brothers' Institute and its Scientific Section vol. iv. Part 1 (Poughkeepsie, N.Y.).

CONTENTS.

THURSDAY, OCTOBER 6, 1887.

ALPHITA.

Alphita. A Medico-Botanical Glossary from the Bodleian Manuscript Selden B 35. Edited by J. L. G. Mowat, M.A., Fellow of Pembroke College. [*Anecdota Oxoniensia.* Mediæval and Modern Series. Vol. I. Part 2.] (Oxford: Clarendon Press, 1887.)

THIS interesting vocabulary, which Mr. Mowat has transcribed and edited from a manuscript in the Bodleian Library, is offered by him as a contribution to the study of English plant-names. To explain why it has this and also other claims to attention, we must say a word about the class of literature to which it belongs.

When ancient Greek science was first brought to the knowledge of mediæval Europe it was by means of Latin versions of the Greek writers, made not directly, but at second hand, from Arabic versions written or brought into Europe by the Moors. The earliest known Latin translations of certain works of Galen, Hippocrates, and other medical writers, with probably some of Aristotle, originated in this way. On the basis of these versions, which began in the eleventh century with the writings of Constantine the African, a medical literature grew up, containing many Greek words corrupted by passing through an Arabic channel, as well as Arabic and some Latin terms hardly less strange to the mediæval reader. It is clear that these hard words presented great difficulties, not as a matter of language only, but of practical use, since it was difficult for the reader to identify the diseases spoken of and the drugs recommended for their cure.

To remedy the uncertainties and dangers thus arising, a new class of literature sprang up—that of the writers whom we may perhaps call the synonymists or glossarists—who compiled lists of the hard words occurring in medical works of the Arabian school, with explanations in Latin. The most celebrated though not the earliest of these was Simon of Genoa, whose list of medical synonyms, the "Clavus Sanationis," was largely borrowed from by subsequent writers. Several others might be named, but there are also anonymous collections of the same kind, one of which is the vocabulary or glossary known as "Alphita."

The anonymous character of this production is not merely a matter of accident. In its present form it is clearly not the work of one writer. A vocabulary or glossary originally intended to explain some work of practical medicine (possibly the "Antidotarium Nicolai," as Mr. Mowat suggests) was expanded by matter introduced from many sources, and by the work of many hands, till at length it could only be regarded as a sort of joint-stock compilation to which no one man's name could be attached.

The title under which it goes is not explained (so far as we can see) by Mr. Mowat, and therefore we may say that it is merely the first word of one of the glosses or definitions, "Alphita, farina ordei idem," or "ἄλφιτον—the same thing as barley-meal."

This definition happening to come first in an older form of the glossary was taken as its title. It has been reprinted under this title in the "Collectio Salernitana" of De Renzi, and another Bodleian manuscript (Ashmole, 1398) giving what appears to be an abridged form of that now published is also thus headed.

But what has all this to do with English plant-names? Merely this, that when scholars or scribes in Northern Europe copied or edited these glossaries (most if not all of which were produced in Italy) they often added the French or English vernacular names of plants. Hence these glossaries form a supplement to the earlier lists of names published by Prof. Earle in his valuable "English Plant Names." There is no reason to suppose that these names were contemporary with the original composition; we may rather assign them to the date of the manuscript, which Mr. Mowat refers to the fifteenth century. They thus form a connecting-link between the Anglo-Saxon and Old English names of the earlier lists, and those which we find in Gerarde and the printed "Herbals" which preceded his. Some of these are very interesting, and have been elucidated with much skill in Mr. Mowat's most laborious and valuable notes; the corrupt and barbarous forms of the Greek and Latin words making them often difficult of recognition.

The interest of the work then lies in the preservation of a number of plant-names; and it is worth inquiring first of all in what way the English names have been identified with their classical equivalents. Sometimes the modern name is a mere corruption of the ancient, as in rose, bugloss, tansy, and numbers more. Sometimes the one is a translation of the other; hound's tongue, coltsfoot, cranesbill, are familiar instances. But when a name was thus altered or translated it did not follow that the plant was identified. A curious instance of the confusion which may arise is the following.

Eleutropia, or elitropia, evidently represents the Greek ἡλιοτρόπιον = heliotropium, and the Latin equivalents *Solseqium* and *Sponsa solis* have the same meaning, viz. a flower which turns to the sun; and an Anglo-Saxon glossarist (quoted by Earle) boldly translates the Latin as *Sigel hweorfa* (turning to the sun). It might still, however, remain doubtful what flower was meant; but when we find Calendula used as a synonym of Solseqium, and when we read in "Alphita" (p. 88), "*Kalendula, sponsa solis, incuba* idem, *Anglice* goldwurt vel rodes," we see that the marigold, a common garden flower in the Middle Ages, and known as golde, gold wort, rode-wort, ruddes, marigolds, mary gowles, &c., is meant (though by the bye it had only borrowed from the marsh marigold—*Caltha palustris*—a name which originally belonged to the latter). The Latin name points clearly to the Calendula folding its flowers when the sun goes down.

But the synonym *incuba* in the above line betrays a confusion with intybum, chicory, as shown again in the gloss (p. 53): "Eleutropia, incuba, sponsa solis vel mira solis solseqium cicoria idem, anglice et gallice cicoree;" or in the line from "Sinonoma Bartolomei" which seems meant for a hexameter: "Incuba, solseqium, cicoreaque sponsaque solis." Gerarde also gives sponsa solis as a name of chicory. So that both marigold and chicory are made synonyms of heliotropium. The curious thing is that the glossary gives definitions quoted from Dioscorides of a larger and lesser "Eliotropium," neither of which can be identified with either of the plants above mentioned.

The glossarist can hardly have supposed that marigold and chicory meant the same thing, but he was evidently hazy as to the meaning of incubus, which occurs again in the following gloss (p. 39): "Cicuta, celena, incubus, coniza vel conium, herba benedicta idem. Gallice chanele vel chanelire; angl. hemelok vel hornwistel."

Gerarde has preserved the name Herb Bennet; the other synonyms we must leave Mr. Mowat to explain. He suggests that the strange name hornwistel may be derived from the offensive smell of the plant. Very likely he is right, but, without any pretensions to philological learning, we may suggest that a hemlock stem is easily converted into a *whistle*.

At p. 156 we have the true etymology of the deceptive name meadow-sweet, "Reginela, Regina Prati, medewort," the English name meaning a plant used for flavouring mead, and altered into meadow-sweet possibly, as Dr. Prior suggests, through some confusion with Regina Prati, queen of the meadow, which name, again, is preserved in the French "Reine des Prés."

Several glosses give the old form of primrose, primerole, a diminutive of Italian *prima vera*, the first flower of spring; and show, moreover, that this name was originally assigned to the daisy, called also *Consolida minor*, of which the German "Ortus Sanitatis" gives an unmistakable figure. The reason evidently was that our primrose is a rare flower in Italy, where the daisy is the herald of spring, but the northern botanists found the name better suited to the flower which now bears it, or to the cowslip, *herba Sancti Petri*.

It is still more startling to find Ligustrum (or modern privet) glossed in some lists (though not in this) as primrose or cowslip. But whatever plant may have been originally meant by Ligustrum, the name privet, or primet, was, as shown by Dr. Prior, originally identical in meaning and almost in etymology with primrose, being derived from French Prime-printemps = Primprint, primet, or prim. Why the Latin name was at one time applied to the flower, at another to the shrub now thus called, is not quite clear.

A curious relic of ancient medicine is preserved in the gloss (p. 5): "Allium domesticum, tyriaca rusticorum, gall. angl. garleke." Here *tyriaca* = θηριακή = *theriaca* (treacle), a once celebrated antidote against snakes and venomous animals. A plant supposed to be the garlick was called by Galen a name rendered in Latin *Theriaca rusticorum*, and so became "poor man's treacle," a name which garlick still bears, though the modern transference of the word treacle to molasses makes it appear absurd.

The medical terms in "Alphita" are extremely interesting, but space forbids entering upon the subject. One curious instance may, however, be quoted, which shows that "there is nothing new under the sun." Only last year Prof. Liebreich, of Berlin, introduced to the medical world, under the name of "lanoline," a new fatty substance for ointments, derived from wool, which has proved a most successful novelty. Now, we find in our glossary the following: "Ysopus cerotis vel Ysopum cerotum est succus lane succide per decoctionem extractus. Qualiter efficitur quere in Dyascorides" (p. 198). *I.e.* "the cerate (or ointment) Ysopum is a 'juice' extracted by boiling from uncleaned wool. For the mode of preparation consult Dioscorides." This is, in fact, οἴσυπος, or *æsopus* mentioned by Dioscorides and Pliny as a fat extracted from the fleeces of sheep, and is practically identical with Liebreich's lanoline.

While thanking Mr. Mowat for this valuable contribution to the history of mediæval science, and the Clarendon Press for their spirited endeavour to make the treasures of the Bodleian common property, we may suggest that there are other scientific relics equally worthy of attention: such, for instance, as some remarkable illustrated manuscripts of anatomy and natural history, or the works of John Arderne, the English surgeon, a relic at least equal in historical value to those already published, and of far greater national significance.

J. F. PAYNE.

OUR BOOK SHELF.

Fresh Woods and Pastures New. By the Author of "An Amateur Angler's Days in Dove Dale." (London: Sampson Low, 1887.)

IN this delightful little volume the amateur angler, who discoursed so pleasantly on the beauties of the stream and fields of Dove Dale a few years ago, recounts his subsequent experiences of country life and amongst country scenes. Angling plays but an inconsiderable part in the present book, but the spirit of the angler is over every chapter—the spirit, namely, which finds placid enjoyment in all the sights and sounds of Nature, and something new and interesting everywhere. His motto is, that the old simplicity of the country "though hid in grey Doth look more gay Than foppery in plush and scarlet clad." Of this capacity for finding amusement everywhere the chapter on turkeys and peacocks is an example. A battle between two flocks of turkeys is described with much humour; the method in which these birds fight is perhaps new even to persons who think they know a good deal about turkeys; it certainly will be to others. Again, the description of a peacock going to roost is full of quiet fun; few persons, even of those who live in the country, have ever seen a peacock perform the feat of flying into a tree for the night. Yet it is a feat to which great importance is attached by the bird himself; it is only to be done with great circumspection, hesitation, and show of indifference. A score of other topics connected with the country are treated with a like charm. The little book, both in subjects and mode of treatment, is a gem.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The British Museum and American Museums.

I VERY much regret to learn that my friend Prof. Flower thinks I have done great injustice to the British Museum of Natural History in my article on "American Museums," which has appeared in the September number of the *Fortnightly Review*. The article was sent to England last February, and I had no opportunity of correcting the proofs, as some very bad misprints will sufficiently indicate. Nothing was farther from my mind than to make any reflections on the management or arrangement of the Museum

Prof. Flower and the able heads of departments, for all of whom I have the greatest respect; and I am further convinced that much credit is due to them for doing the very utmost that is possible under the circumstances of the case. My strictures on the Museum were intended to apply solely and exclusively to the fundamental *principle* underlying its arrangement, which principle is embodied in the new building as in the old one. I contrasted strongly the principle of moderate-sized rooms as compared with large galleries,—the principle of exhibiting, to the public, on the one hand, strictly limited typical collections; on the other, almost complete series of species,—the principle of making a geographical arrangement the main feature of a museum, as compared with that in which almost no provision at all is made for such an arrangement.

I had always understood that for this fundamental system of arrangement neither the present Director nor the heads of departments of the Museum were in any way responsible, and that in criticising it frankly I should not be considered to reflect on them. So clear was I in my own mind that I was discussing this general system only, that I used some expressions which I now see, with much regret, were capable of being misunderstood. After referring to some of the improvements in the New British Museum, I say, "but the great bulk of the collection still consists of the old specimens exhibited in the old way in an interminable series of overcrowded wall-cases, while all attempt at any effective presentation of the various aspects and problems of natural history as now understood is as far off as ever." To the latter part of this sentence, Prof. Flower objects, as not recognizing the many improvements recently made and still making; but I intended it to apply, as I think the whole context of my article shows, to the *system* and the *building*, which themselves, from the point of view I have taken throughout the article, render any attempt at an "effective" presentation of these aspects and problems impossible. Again, at the end of my article I speak of Prof. Agassiz having said that he intended his museum "to illustrate the history of creation as far as the present state of scientific knowledge reveals that history," and then go on: "It is surely an anomaly that the naturalist who was most opposed to the theory of evolution should be the first to arrange his museum in such a way as best to illustrate that theory, while in the land of Darwin no step has been taken to escape from the monotonous routine of one great systematic series of crowded specimens arranged in lofty halls and palatial galleries, which may excite wonder, but which are calculated to teach no definite lesson." Here I was referring to the fact that the new Museum at South Kensington was constructed and arranged substantially on the same lines as the old one at Bloomsbury, and regretting that the only effective step towards inaugurating a new system of arrangement was not then taken. Prof. Flower, I find, thinks that I imply that no steps are being taken now to render the Museum more instructive and generally interesting. This was very far from my meaning, and I am exceedingly sorry that such an interpretation of my words should have been possible. I visited the Museum several times last summer before leaving for America, and I noted many improvements that were being introduced in all departments; but I could not fail to see that the main principle of the arrangement, both of the building itself and of the collections in it, had not been changed, and it was to this that all my criticisms were directed.

Godalming, September 22. ALFRED R. WALLACE.

The Law of Error.

MR. F. Y. EDGEWORTH has, in NATURE of September 22 (p. 482), replied to Dr. Venn's letter from the mathematical standpoint; perhaps a few words from the meteorological side may not be out of place. The gist of Dr. Venn's remarks lies in his statement that the law of error applies to cases where there are "equal and opposite independent disturbing causes" (September 1, p. 412). Now, the excess and defect of barometrical pressure from the average, depend mainly on anticyclones and cyclones respectively, which though in many respects opposite in character are by no means equal, the latter being much more intense than the former; and there is no reason in the nature of the case why they should be equal, as many of their characteristics are so dissimilar.

As regards the second instance given by Dr. Venn, the chief factor in the variations of temperature at different times of the year is the varying declination of the sun, the rate of change of declination passing through two minima yearly—namely, at the solstices, so named for this very reason. One would naturally expect that about these times the temperature should remain more nearly the same than about the equinoxes; Dr. Venn's curve would consequently give two maxima. The deviations of the temperature of each *day* from the average would not be unlikely to conform to the law of error, but it is evident that a curve formed from the temperatures for the whole year would be of a totally different kind. T. W. BACKHOUSE.

Sunderland, September 26.

Lunar Rainbows.

ON Sunday night, August 28, a lunar rainbow was visible here. As the occurrence seems to be uncommon, some particulars may interest your readers.

We had a very heavy shower before 11 o'clock, with a south-west wind. The rain left off suddenly, as it began, a few minutes past 11; and as the heavy cloud moved away to the north-east it left a gloriously clear sky behind, with the moon, then a little past its first quarter, shining brightly a few degrees above a heavy bank of cloud which lay on the horizon. Looking out of a window on the opposite side of the house, I had the satisfaction of seeing a complete pale white bow in the black cloud to the north-east, which lasted very clear and distinct for about five minutes, when it quickly grew faint as the bank of clouds on the horizon began to rise and obscure the falling moon. The outer edge of the bow was well defined against the intense black of the cloud beyond; the inner edge was much less distinct, and the area within was covered with a slight suffused light, which, however, appeared to diminish as the distance from the bow increased.

The drops of rain were unusually large, and the downpour, while it lasted, was extraordinarily heavy.

A. F. GRIFFITH.

15 Buckingham Place, Brighton, September 22.

A LUNAR rainbow was visible here shortly after 11 o'clock last night. It extended without break through three-quarters of a semicircle, the top of the arch being about 60° high. In colour the bow resembled a moonbeam shining between two clouds, and its brightness was sufficient to cause it to be immediately detected by a casual glance, in spite of the presence of numerous white clouds occupying its centre. The sky just outside the bow appeared darkest, probably by contrast with these clouds. Ten minutes elapsed before the rainbow faded.

Rock Ferry, September 27. S. J. H.

The Perception of Colour.

IS Mr. Stromeyer sure that the observations he made (see NATURE, July 14, p. 246) prove any difference in the rapidity of perception of colour, and that they do not rather show a difference in perception of brightness? It is well known that faint objects are not so quickly perceived as bright ones (see Webb's "Celestial Objects," p. 368 of the 4th edition, under ϵ Pegasi); and as the violet end of the spectrum is much fainter than the rest, the effect described would be produced by the difference in brightness apart from the difference in colour. I have tried Mr. Stromeyer's experiment of rotating the spectrum, and it appears to me that the red as well as the violet end lags behind the middle; though as the red is so much shorter, this is more difficult to see. T. W. BACKHOUSE.

Sunderland, September 15.

Tertiary Outliers on the North Downs.

IN August of last year (NATURE, vol. xxxiv. p. 341), I ventured to draw a distinction between the *unfossiliferous sands* found at certain places on the North Downs and the fossiliferous deposits at Lenham. For reasons assigned, I suggested a certain degree of probability of their being of Bagshot age, and indicating a former extension by overlap of the higher beds of that important Eocene formation. This summer I have had opportunities of examining all the principal outliers referred to; and I must say that I am strongly impressed with the Bagshot character of these unfossiliferous sands, and of the well-rolled flint pebbles associated with them, in some cases (as at Headley) in great quantity. I speak only of those which can be identified with some degree of certainty as Tertiary beds *in situ*. The sands at Netley Heath and at Chipstead have a remarkable *Upper* Bagshot facies. Those at Headley do not present such a strong character in this respect, but I have no hesitation in referring them on lithological grounds to the Bagshot series.

Wellington College, Berks, September 27. A. IRVING.

MODERN VIEWS OF ELECTRICITY.[1]

PART I.

I.

IT is often said that we do not know what electricity is, and there is a considerable amount of truth in the statement. It is not so true, however, as it was some twenty years ago. Some things are beginning to be known about it; and though modern views are tentative, and may well require modification, nevertheless some progress has been made. I shall endeavour in this lecture to set forth as best I may the position of thinkers on electrical subjects at the present time.

It will at once strike you that the whole subject of electricity as at present known is too gigantic for anyone to make an attempt to compass it in a single lecture, even though he assume on the part of his audience a perfect acquaintance with all the ordinary phenomena; and you will admit that it is much better to limit one's self definitely at the beginning to some one branch than by attempting too broad and discursive a survey to risk slurring the whole and becoming totally unintelligible.

I begin by saying that the whole subject of electricity is divisible for purposes of classification into four great branches.

(1) Electricity at rest, or static electricity: wherein are studied all the phenomena belonging to stresses and strains in insulating or dielectric media brought about by the neighbourhood of electric charges or electrified bodies at rest immersed therein; together with the modes of exciting such electric charges and the laws of their interactions.

(2) Electricity in locomotion, or current electricity: wherein are discussed all the phenomena set up in metallic conductors, in chemical compounds, and in dielectric media, by the passage of electricity through them; together with the modes of setting electricity in continuous motion and the laws of its flow.

(3) Electricity in rotation, or magnetism: wherein are discussed the phenomena belonging to electricity in whirling or vortex motion, the modes of exciting such whirls, the stresses and strains produced by them, and the laws of their interaction.

(4) Electricity in vibration, or radiation: wherein are discussed the propagation of periodic or undulatory disturbances through various kinds of media, the laws regulating wave velocity, wave-length, reflection, interference, dispersion, polarization, and a multitude of phenomena studied for a long time under the heading "Light." Although this is the most abstruse and difficult portion of electrical science, a certain fraction of it has been known to us longer than any other branch, and has been studied under special advantages, because of our happening to possess a special sense-organ for its appreciation.

Now, with some qualms of regret I have decided to refrain from speaking to you about any one of these great and comprehensive groups except the first. It is hopeless to attempt more; and even the small portion of that on which I shall touch will tax the time at our disposal to the utmost, and I must assume acquaintance with the elementary facts in order to proceed to their elucidation.

The great names in connexion with our progress in knowledge as to the real nature of electricity, irrespectiv of a mere study and extension of its known facts, are

FRANKLIN, CAVENDISH, FARADAY, MAXWELL.

To these, indeed, you may feel impelled to add th tremendous name of THOMSON; but one has som delicacy in attempting to estimate the work of livin philosophers, and as Maxwell has been very explicit i acknowledging his indebtedness to his illustrious con temporary, whose work will in the course of nature hav to be criticised and appraised by far abler hands tha mine and by the philosophers of generations yet unborn we may well afford to abstain from minute consideration and accept for the present the name of Maxwell as repre sentative of the great English school of mathematica physicists, under whose influence, Cambridge, in th pride of having reared them, is awaking to new an energetic scientific life, and whose splendid achievement will shine out in the future as the glory of this century

The views concerning electrification which I shall tr to explain are in some sense a development of thos originally propounded by that most remarkable ma Benjamin Franklin. The accurate and acute exper menting of Cavendish laid the foundation for the moder theory of electricity; but, as he worked for himself rathe than for the race, and as moreover he was in this matter fa in advance of his time, Faraday had to go over the sam ground again, with extensions and additions peculiar t himself and corresponding to the greater field of informa tion at his disposal three-quarters of a century late Both these men, and especially Faraday, so lived amon phenomena that they yielded up their hidden secrets t them in a way unintelligible to ordinary workers; bu while they themselves arrived at truth by processes th savour of intuition, they were unable always to expre themselves intelligibly to their contemporaries and make the inner meaning of their facts and speculatio understood. Then comes Maxwell, with his keen pen tration and great grasp of thought combined with math matical subtlety and power of expression; he assimilat the facts, sympathizes with the philosophic but untutor modes of expression invented by Faraday, links t theorems of Green and Stokes and Thomson to the fac of Faraday, and from the union there arises the you modern science of electricity, whose infancy at t present time is so vigorous and so promising that we a all looking forward to the near future in eager hope a expectation of some greater and still more magnifice generalization.

You know well that there have been fluid or mater theories of electricity for the past century; you kno moreover, that there has been a reaction against the There was even a tendency a few years back to deny t material nature of electricity and assert its position a form of energy. This was doubtless due to an analogi and natural, though unjustifiable, feeling that just sound and heat and light had shown themselves to forms of energy so in due time would electricity also. such were the expectation, it has not been justified the event. Electricity may possibly be a form matter—it is not a form of energy. It is quite tr that electricity *under pressure* or *in motion* represe energy, but the same thing is true of water or a and we do not therefore deny them to be forms of matt Understand the sense in which I use the word electrici *Electrification* is a result of work done, and is m certainly a form of energy; it can be created and stroyed by an act of work. But electricity—none is e created or destroyed, it is simply moved and strained l matter. No one ever exhibited a trace of positive elec city without there being somewhere in its immedi neighbourhood an equal quantity of negative.

This is the first great law, expressible in a variety ways: as, for instance, by saying that total algebraic p

[1] Expansion of a lecture delivered by Dr. Oliver Lodge, partly at the London Institution on January 1, 1885, and partly at the Midland Institute, Birmingham, November 15, 1886, but not hitherto published.

duction of electricity is always zero; that you cannot produce positive electrification without an equal quantity of negative also; that what one body gains of electricity some other body must lose.

Now, whenever we perceive that a thing is produced in precisely equal and opposite amounts, so that what one body gains another loses, it is convenient and most simple to consider the thing not as generated in the one body and destroyed in the other, but as simply *transferred. Electricity in this respect behaves just like a substance.* This is what Franklin perceived.

The second great law is that electricity always, under all circumstance, flows in a closed circuit, the same quantity crossing every section of that circuit, so that it is not possible to exhaust it from one region of space and condense it in another.

Another way of expressing this fact is to say that no charge resides in the interior of a hollow conductor.

Another is to say that total induced charge is always equal and opposite to inducing charges.

[This is illustrated by the well-known experiment of insulating and charging a parrot-cage with a sensitive electroscope inside connected to its wires; also by the ice-pail experiment.]

When we thus find that it is impossible to charge a body absolutely with electricity, that though you can move it from place to place it always and instantly refills the body from which you take it, so that no portion of space can be more or less filled with it than it already is, it is natural to express the phenomenon by saying that electricity behaves itself like a perfectly *incompressible* substance or fluid, of which all space is completely full. That is to say, it behaves like a perfect and all-permeating *liquid.* Understand, I by no means assert that electricity *is* such a fluid or liquid; I only assert the undoubted fact that it behaves like one, *i.e.* it obeys the same laws.

It may be advisable carefully to guard one's self against becoming too strongly imbued with the notion that because electricity obeys the laws of a liquid therefore it is one. One must always be keenly on the look-out for any discrepancy between the behaviour of the two things, and a single certain discrepancy will be sufficient to overthrow the fancy that they may perhaps be really identical. Till such a discrepancy turns up, however, we are justified in pursuing the analogy—more than justified, we are impelled. And if we resist the help of an analogy like this there are only two courses open to us: either we must become first-rate mathematicians, able to live wholly among symbols and dispensing with pictorial images and such adventitious aid; or we must remain in hazy ignorance of the stages which have been reached, and of the present knowledge of electricity so far as it goes. I need hardly say that by "modern views" I do not mean *ultimate* views; nor do I mean that I can give an account of all the speculations and ideas floating in the minds of some two or three of our most advanced thinkers. All I attempt is to give an account of the stage which has certainly been attained, and to ask you to take for granted that the next quarter of a century will see as great advances made upon these views as they are superior to the doctrines inculcated by the ordinary run of text-books.

Imagine now that we live immersed in an infinite ocean of incompressible and inexpansible all-permeating perfect liquid, like fish live in the sea, and how can we become cognizant of its existence? Not by its weight, for we can remove it from no portion of space in order to try whether it has weight.

We can weigh air, truly, but that is simply because we can compress it and rarefy it. An exhausting or condensing pump of some kind was needed before even air could be weighed or its pressure estimated.

But if air had been incompressible and inexpansible, if it had been a vacuum-less perfect liquid, pumps would have been useless for the purpose, and we should necessarily be completely ignorant of the weight and pressure of the atmosphere.

How then should we become cognizant of its existence? In four ways:—

(1) By being able to pump it out of one elastic bag into another [not out of one bucket into another: if you lived at the bottom of the sea you would never think about filling or emptying buckets—the idea would be absurd; but you could fill or empty elastic bags], and by noticing the strain phenomena exhibited by the bags and their tendency to burst when over full. [Water (or air) was here pumped out of one elastic bag into another, and the analogy with an electrical machine charging two conductors oppositely was pointed out.]

(2) By winds or currents; by watching the effect of moving masses of the fluid as it flows along pipes or through spongy bodies, and by the effects of its inertia and momentum. [A hanging vane in a tube deflected by a stream of water was here likened roughly to a galvanometer; also the effect of suddenly stopping a stream of water, as in a water ram, was mentioned as analogous to self-induction.]

(3) By making vortices and whirls in the fluid, and by observing the mutual action of these vortices, their attractions and repulsions. [Whirlwinds, sand-storms, waterspouts, cyclones, whirlpools.]

(4) By setting up undulations in the medium: *i.e.* by the phenomena which in ordinary media excite in us through our ears the sensation called "*sound.*"

In all these ways we have become acquainted with electricity, and in no others that I am aware of. They correspond to the four great divisions of the subject which I made above. But there are differences, very important differences, between the behaviour of a material liquid ocean such as we have contemplated and the behaviour of electricity. First it is doubtful whether electricity by itself and disconnected from matter has any inertia. It is by no means certain that it has not: the experiments made by Maxwell with a negative result need only prove either that its speed of flow is very small, or that an electric current consists of equal opposite streams of equal momentum. The laws of electric flow in conductors are such as indicate no inertia, and this fact would be conclusive were it not that a recent brilliant paper by Prof. Poynting explains the reason of it completely otherwise, and leaves the question of inertia quite open; on the other hand, the facts of magnetism seem definitely to require inertia, or something corresponding to it. Leaving this therefore as an open question, there can be no doubt but that when in connexion with insulating or dielectric matter *the combination* most certainly possesses inertia.

A more serious and certain difference between the behaviour of electricity and that of an incompressible fluid comes out in the fourth category—that concerned with wave-motion. In an incompressible fluid the velocity and length of waves would both be infinite, and none of the phenomena connected with the gradual propagation of waves through it could exist. Such a medium therefore would be incapable of sound vibrations in any ordinary sense. On the other hand, it is quite certain that the disturbances concerned in light radiation take place at right angles to the direction of propagation—they are tranverse disturbances — and such disturbances as these no body with the entire properties of a fluid can possibly transmit. Remember, however, that the medium which transmits light is the ether and not simply electricity. We have nowhere asserted that electricity and the ether are identical. If they are, we are bound to admit that ether, though fluid in the sense of enabling masses to move freely through it, has a certain amount of rigidity for enormously rapid and minute oscillatory disturbances. If they are not identical, we can more vaguely say that ether contains electricity as a jelly contains water, but that the rigidity concerned in the transverse vibrations belongs not to the water in the jelly but to the mode in which it is entangled in its meshes. However all this is a great and difficult question into which we shall be able to enter with more satisfaction twenty years hence.

Provisionally we will accept as a working hypothesis the idea of the ether consisting of electricity in a state of entanglement similar to that of water in jelly; and we are driven to this view by the exigencies of mode 1, the electrostatic or strain method of examining the properties of electricity, because otherwise the properties of insulators are hard to conceive. If it turn out that space is a conductor, which seems to me highly improbable, then we must fall back upon the other view that it is rigid only for infinitesimal vibrations, and fluid for steady forces.

Return now to the consideration of electrostatics. We are to regard ourselves as living immersed in an infinite all-permeating ocean of perfect incompressible fluid (or liquid), as fish live in the sea; but this is not all, for if that were our actual state we should have no more notion of the existence of the liquid than deep-sea fish have of the medium they swim in. If matter were all perfectly conducting, it would be our state: in a perfectly free ocean there is no insulation—no obstruction to flow of liquid: it is the fact of insulation that renders electrostatics possible. We could obstruct the flow and store up definite quantities of a fluid in which we were totally submerged by the use of closed vessels of course. But how could we pump liquid from one into another so as to charge one positively and another negatively? Only by having the walls elastic: by the use of elastic bags, and elastic partitions across pipes. And so we can represent a continuous insulating medium (like the atmosphere or space) by the analogy of a jelly, through which liquid can only flow by reason of cracks and channels and cavities.

Modify the idea of an infinite ocean of liquid into that of an infinite jelly or elastic substance in which the liquid is entangled, and through which it cannot penetrate without violence and disruption; and you have here a model of the general insulating atmosphere. Our ocean of fluid is not free and mobile like water, it is stiff and entangled like jelly.

Nevertheless bodies can move through it freely. Yes *bodies* can, it is the *liquid* itself only which is entangled. How we are to picture freely and naturally the motion of ordinary matter through the insulating medium of space it is not easy to say. It is a difficulty not fatal but sensible, and due to an imperfection in our analogy.

Insulators being like elastic partitions or impervious but yielding masses, conductors are like cavities, porous or spongy bodies perfectly pervious though with more or less frictional resistance to the flow of liquids through them. Thus, whereas bodies easily penetrable by matter are impervious to electricity, bodies like metals which resist entirely the passage of matter, are quite permeable to electricity. It is this inversion of ordinary ideas of penetrability that constitutes a small difficulty at the beginning of the subject.

However, supposing it overcome, let us think of these insulated spheres and cylinders on the table connected by copper wire as so many cavities and tubes in an otherwise continuous elastic impervious medium which surrounds them and us, and extends throughout space wherever conductors are not. All, however, cavities as well as the rest of the medium, are completely full of the universal fluid. The fluid which is entangled in insulators is free to move in conductors; whence it follows that its pressure or potential is the same in every part of a conductor in which it is not flowing along. For if there were any excess of pressure at any point, a flow would immediately occur until it was equalized. In an insulator this is by no means the case. Differences of pressure are exceedingly common in insulators, and are naturally accompanied by a strain of the medium.

[Here certain electrostatic experiments were shown as evidence of the strain existing at the ends of a long insulated wire connected to a Voss machine.]

There have been, as you know, two ancient fluid theories of electricity—the one-fluid theory of Franklin, and the two-fluid theory of Symmer and others. A great deal is to be said for both of them within a certain range. There are certainly points, many points, on which they are hopelessly wrong and misleading, *but it is their foundation upon ideas of action at a distance that condemns them, it is not the fluidity.* They concentrate attention upon the conductors; whereas Faraday taught us to concentrate attention on the insulating medium surrounding the conductors—the "*dielectric*" as he termed it. This is the seat of all the phenomena: conductors are mere breaks in it—interrupters of its continuity.

To Faraday the space round conductors was full of what he called lines of force; and it is his main achievement in electrostatics to have diverted our attention from the obvious and apparent to the intrinsic and essential phenomena. Let us try and seize his point of view before going further. It is certainly true as far as it goes, and is devoid of hypothesis.

Take the old fundamental electric experiment of rubbing two bodies together, separating them, and exhibiting the attraction and repulsion of a pith ball, say, and how should we now describe it? Something this way.

Take two insulated disks of different material, one metal, say, and one silk, touch them together, the contact effects a transfer of electricity from the metal to the silk; rub slightly to assist the transfer, since silk is a non-conductor, then separate. As you separate the disks the medium between them is thrown into a state of strain, the direction of which is mapped out by drawing a set of lines, called lines of force, from one disk to the other, coincident with the direction of strain at every point. As Faraday remarked, the strain is as if these lines were stretched elastic threads endowed with the property of repelling each other as well as of shortening themselves; in other words, there is a tension along the lines of force and a pressure at right angles to them. When the disks are near, and the lines short, they are mainly straight, Fig. 1,

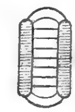


FIG. 1.—Rough diagram of the state of the medium near two oppositely charged disks when close together.

but as the distance increases they become curved, bulging away from the common axis of the two disks, and some even curling round to the back of the disk (Fig. 2), until when the disks are infinitely distant as many lines spring from the back of each as from its face; and we have a charged body to all intents existing in space by itself.

The state of tension existing in the medium between the disks results in a tendency to bring them together again, just as if they were connected by so many elastic threads of no length when unstretched. The ends of the lines are the so-called electrifications or charges, and the lines perpetually try and shorten and shut up, so that their ends may coincide and the strain be relieved. If one of the disks touch another conducting body, some of its lines instantly leave it and go to the body; in other words, the charge is capable of transference, and the new body is urged toward the other disk, just as the disk was from which it received the lines. If this new body *completely surrounds* the disk, it receives the whole of its lines, and the disk can be withdrawn perfectly free and inert. [Faraday's "ice-pail" experiment.]

Now take the two charged disks, facing one another, and let, say, a suspended gilt pith ball hang between them. Being a conductor there is no strain inside it, and so it acts partially as a bridge, and several of the lines pass through it—or, rather, they end at one side of it and begin at the other: thus it has opposite charges on its two faces—it is under induction (Fig. 3). Let it now be moved so as to touch one of the disks, the lines between it and the disk on that side have shut up, and it remains with those only which go to the other disk. In other words, it has received some lines from the touched disk. These will pull it over to the far disk and there shut

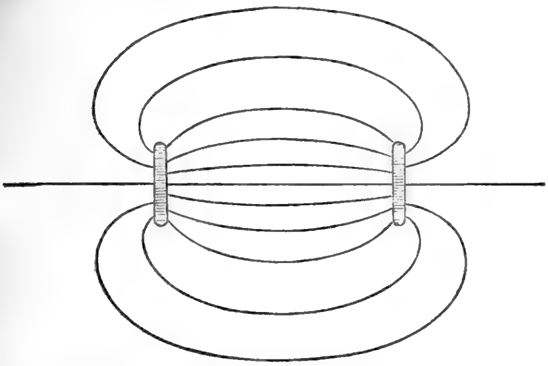


FIG. 2.—Rough diagram of the state of the medium near two oppositely charged disks when separated.

themselves up. From that disk it receives more, and travels with their ends back to the first disk, and so on (Fig. 4), perpetually receiving lines and shutting them up until they are all gone and the disks are discharged.

This mode of stating the facts involves no hypothesis whatever—it is the simple truth. But the "lines of force" have no more and no less existence than have "rays of light." Both are convenient modes of expression.

But so long as we adhere to this mode of expression we cannot form a complete mental picture of the actually

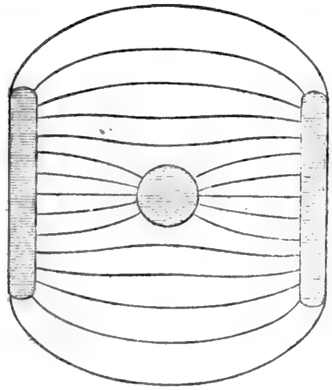


FIG. 3.—Rough diagram of the medium between two disks disturbed by the presence of an uncharged metal sphere. The two halves of the sphere are oppositely charged "by induction."

occurring operations. In optics it is usual to abandon rays at a certain stage and attend to the waves, which we know are of the essence of the phenomenon, though we do not know yet very much about their true nature.

Similarly in electricity, at a certain point we are led to abandon lines of force and potential theories, and to try to conceive the actual stuff undergoing its strains and motions. It is then we get urged towards ideas similar to those which are useful in treating of the behaviour of an incompressible fluid.

In an utterly modified sense, we have still a fluid theory of electricity, and a portion of the ideas of the old theories belong to it also.

Thus Franklin's view that positive charge was excess and negative charge was a deficit in a certain standard quantity of the fluid which all bodies naturally possessed in their neutral state, remains practically true. His view that the fluid was never manufactured, but was taken from one body to give to another, so that one gained what the other lost—no more and no less—remains practically true. Part also—a less part—of the two-fluid theory likewise remains true, in my present opinion; but this is not a branch of the subject on which I shall enter in the present discourse. It will suffice for the present to fix our attention on one fluid only.

You are to think of an electric machine as a pump which, being attached to two bodies respectively, drives some electricity from the one into the other, conferring upon one a positive and upon the other a precisely equal negative charge. One of the two bodies may be the earth, in which case the charge makes little or no difference to it.

But, as has been objected before, if electricity is like an incompressible and inextensible fluid, how is it possible to

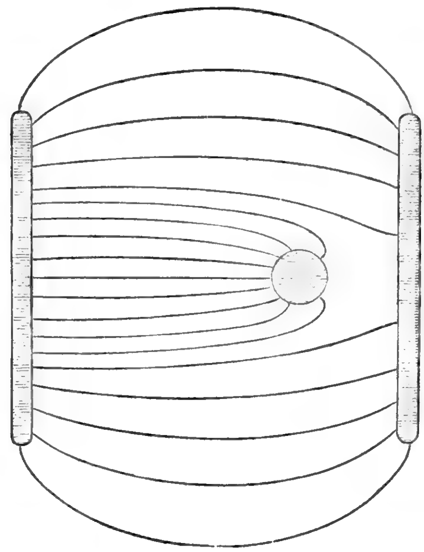


FIG. 4.—Rough diagram of the medium near two oppositely charged disks between which a metal carrier ball is oscillating, having just touched the right-hand disk. (Discharge by "alternate contact.")

withdraw any of it from one body and give it to another? With rigid bodies it is not possible, but with elastic bodies it is easy.

The act of charging this sphere is therefore analogous to pumping water into this elastic bag, or rather into a cavity in the midst of an elastic medium, whose thick walls, extending in all directions and needing a great pressure to strain them, better represent the true state of the case than does the thin boundary of a bag like this.

Draw a couple of such cavities and consider fluid pumped from one into the other, and you will see that the charge (*i.e.* the excess or defect of fluid) resides on the outside. You may also show that when both are similarly charged the medium is so strained that they tend to be forced apart; whereas when one is distended and the other contracted they tend to approach.

Further you may consider two cavities side by side, pump fluid into (or out of) one only, and watch the effect on the other. You will thus see the phenomena of induction, the near side of the second cavity becoming oppositely charged (*i.e.* the walls encroaching on the cavity), the far side similarly charged (the cavity encroaching on the walls), and the pressure on the fluid in the cavity being increased or diminished in correspondence with the change of pressure in the charged or inducing cavity. In other words, conductors rise in potential when brought near a positively charged body.

The actual changes in volume necessary to the strain of these cavities are a defect in the analogy. To avoid this objection, one will have to accept a dual view of electricity—a sort of two-fluid theory, which many phenomena urge one to accept, but about which I will say nothing to-night. It is sufficient at first to grasp the one-fluid ideas.

Return Circuit.—Sometimes a difficulty is felt about electricity flowing in a closed circuit—as, for instance, in signalling to America and using the earth as a return circuit: the question arises, How does the electricity find its way back?

The difficulty is no more real than if a tube were laid to America with its two ends connected to the sea and already quite full. If now a little more sea-water were pumped in at one end, an equal quantity would leave the other end, and the disturbed level of the ocean would readjust itself. Not the same identical water would return, but an equal quantity would return. That is all one says in electricity. One cannot label and identify electricity.

To imitate the inductive retardation of cables, the tube should have slightly elastic walls; to imitate the speed of signalling, the water must be supposed quite incompressible, not elastic as it really is, or each pulse would take three-quarters of an hour to go.

Condensers.—Returning to the subject of charging bodies electrically, how is one to consider the fact that bringing an earth-plate near a conductor increases its capacity so greatly, enabling the same pressure to force in a much larger quantity of fluid? how is one to think of a condenser, or Leyden jar?

In the easiest possible way, by observing that the bringing near an earth-connected conductor is really *thinning down the dielectric* on all sides of the body.

The thin-walled elastic medium of course takes less force to distend it a given amount than a thick mass of the same stuff took. A Leyden jar is like a cavity with quite thin walls—in other words, it is like an elastic bag.

But if you thin it too far, or strain it too much, the elastic membrane may burst: exactly, and this is the disruptive discharge of a jar, and is accompanied by a spark. Sometimes it is the solid dielectric which breaks down permanently. Ordinarily it is merely the air; and, since a fluid insulator constitutes a self-mending partition, it is instantaneously as good as new again.

There are many things of interest and importance to study about a Leyden jar. There is the fact that if insulated, it will not charge: the potential of both inner and outer coatings rises equally; that, in order to charge it, for every positive spark you give to the interior an equal positive spark must be taken from the exterior. There is the charging and the discharging of it by alternate contacts, as by an oscillating ball; and there are the phenomena of the spark-discharge itself.

But, as you know, *all* charging is really a case of a Leyden jar. The outer coat must always be somewhere—the walls of the room, or the earth, or something—you always have a layer of dielectric between two charges—the so-called induced and the inducing charge. You cannot charge one body alone.

To illustrate the phenomena of charge, I will now call your attention to these diagrams—which less completely but more simply than hydraulic illustrations, serve to make the nature of the phenomena manifest.

(*To be continued.*)

ON THE TEACHING OF CHEMISTRY.[1]

THE question is being often asked, Why does chemistry progress so slowly in this country? Different answers, all more or less true, may be given; one answer that has not, I think, been sufficiently insisted on is: Because chemistry is so little taught.

Classes, nominally in chemistry, are conducted in many schools, and in almost all the colleges, of the country; but I assert that very little of what is taught is really chemistry. For what is it that is taught? On the one hand, catalogues of so-called facts detached from reasoning and from generalizations; on the other hand, definitions and generalizations and speculations detached from the facts on which they rest. But neither detached facts, however accurately stated, nor definitions, in however sharply-cut words they are contained, nor speculations, however interesting they may be, are science; and chemistry really is a branch of natural science.

It is admitted by all that hydrogen is a colourless, odourless gas, 14·435 times lighter than air, produced by the interaction of dilute sulphuric acid and zinc, combustible, condensable to a liquid at very low temperatures. These statements are facts; but when the student of chemistry is required to read, and if possible to remember, such facts as these about each of the elements and its compounds, the statements cease to be facts to him, and become false, inasmuch as they cover up and hide the really important facts regarding the interactions of elements and compounds, and regarding the connexions between changes of composition and changes of properties, which form the subject-matter of chemistry.

I have known students have at their finger-ends the properties of all the elements—as these properties are detailed in the ordinary text-books—and yet be almost wholly ignorant of chemistry.

And I have also known students ready at a moment's notice to repeat the orthodox definitions of atomic and molecular weights, or to draw structural formulæ of complex minerals, or to speak fluently about double bonds and unsaturated units of affinity, and yet be quite innocent of any knowledge of chemistry.

A fatal distinction is too often drawn by chemical teachers between the facts on which chemical science rests, and reasoning and generalizing on these facts: the former, that is statements of detached facts, is called chemistry; the latter, that is reasoning on facts and generalizing to principles, is called chemical philosophy. I believe strongly that there are not two chemistries, but one chemistry. If chemical teachers were quite decided as to what they ought to teach, we might hope for marked advances in our science.

My own experience in teaching chemistry convinces me that it is a very difficult subject both to teach and to learn. Of course there is no great difficulty in restating to a class what is printed in the text-book, and occasionally enlivening the routine by a few experiments; nor does it require high mental capacities and training to tell the laboratory student that bottle A contains a double salt, to be analyzed by the help of the tables on page so-and-so of the book. But do not let us call this kind of thing teaching chemistry. To teach chemistry well requires experience and an educated mind. It is not easy to hit the golden mean. If the teacher despises facts his reasoning becomes absurd, because it is based on nothing; his principles become only speculations; and his laws merely phrases. If he disregards principles, generalizations, and theories, his facts become false, and when facts are false (as they often are) they are deadly, and kill those that trust in them.

Chemistry is a branch of natural science; it deals with one class of natural occurrences, it observes and experi-

[1] A Paper read before Section B of the British Association at Manchester, by M. M. Pattison Muir, M.A., Fellow of Gonville and Caius College, Cambridge.

ments on these occurrences, it classifies and generalizes, and tries to ascend from empirical generalizations to natural laws. The business of the teacher of chemistry I take to be to make his pupil understand the methods of chemistry; to select typical facts and put these before the learner so that he may to some extent see their relative importance in the general scheme of well-built knowledge; to show the student how complex are the phenomena chemistry investigates, and how she simplifies that she may explain; to imbue him with some of that fine enthusiasm without which no great work is possible, by presenting to him glimpses of the greatness of the subject he is studying, and the importance of the prosecution of the study; and thus to build up in the student a scientific spirit, until at last the teacher and the learner are merged in their common investigation of nature.

In teaching chemistry the all-important things appear to me to be chiefly four: (1) to teach so that the student shall acquire real knowledge; (2) to carefully select both the facts and the reasoning to be set before the student; (3) to impress the learner with the importance and value of what he is learning as a part of that orderly and methodized study of nature which we call science; (4) to teach without fear of the examiner.

Real knowledge of chemistry can only be acquired by connecting the experimental work in the laboratory with chemical reasoning and with the principles of the science. This is rarely done in our chemical schools. The student generally hears lectures on chemistry, or at least on the materials from which chemical science is constructed; he sees experiments performed that have some connexion with what the lecturer teaches; then he goes into the laboratory and day after day performs qualitative analyses, for the most part by rule of thumb. The learner, especially the beginner, cannot connect what he is taught in lectures, and told to read in books, with what he does in the laboratory. The introduction of a well-arranged and properly graduated system of practical chemistry, is, in my opinion, one of the things which will do much to hasten the advance of chemistry among us. The work done in the laboratory must be directly connected with the lecture-work and the reading of the student; it must be progressive, beginning with easy experiments and leading the student onwards until he is able to investigate chemical occurrences for himself; and it must be arranged so that as the experimental work becomes more difficult the reasoning on the results becomes more close and accurate.

Such a course of practical chemistry can be arranged without any complicated laboratory appliances. (I may say parenthetically that in my opinion the building of luxuriously fitted laboratories has not been an unmixed gain to chemical science.)

In the course of laboratory work which seems to me to be called for, the student would begin with easy experiments on chemical and physical change, on the distinction between elements and not-elements (this would involve quantitative measurements), and on the classification of not-elements into mixtures and compounds. He would then proceed to classify compounds into acids, bases, and salts, working through well-chosen examples of each class. He would learn by his own experiments what is meant by "the replaceable hydrogen of acids," and by the classification of acids in accordance with their "basicity." He would study classes of elements, and so get a real grasp of the reasons for placing certain elements in one class, and of the principles of chemical classification. After hearing in the lecture-room about the properties of the members of a class of elements—say the iron class—he would at once go into the laboratory and himself prepare typical similar compounds of these elements. He would then turn to the conditions of chemical action; he would practically learn what an ordinary chemical equation teaches, and he would be impressed, by the results of his own experiments, with the importance of determining the conditions under which chemical changes occur, and with the many and varied facts regarding even every-day chemical changes which are not expressed in our chemical notation. The study of the conditions of chemical change would lead on to the study of affinity, of dissociation, and of allied subjects.

Turning again to the study of composition, the student would make determinations of the equivalent weights of a series of similar compounds, and also of several elements; he would determine the molecular weights of a few gaseous bodies, and the atomic weights of one or two elements. He would then proceed to study experimentally some of the methods whereby light is thrown on the constitutions of compounds. For instance, he would determine the specific volumes of a series of carbon compounds, the rates of etherification of a series of alcohols, and the nature of the products of the reaction of such a compound as phosphorus pentachloride with carbon compounds belonging to different classes but showing similarities of composition. Thus the molecular and atomic theory would become a reality to him. Finally, he would be required to repeat an investigation before undertaking himself to advance into the realm of the unknown.

In such a course as this the student would study a series of carefully selected facts; his reading and laboratory work would go hand in hand, each would react upon the other, and so he would be saved from the danger of attempting to draw a distinction between two things which are truly one—chemistry and chemical philosophy.

In selecting the facts and reasoning to be placed before the student of chemistry I think we should now finally abandon the method of treating the elements individually, and rather consider them in groups or classes. If this is done the student soon acquires a fair knowledge of chemical facts; he learns the compositions and properties of groups of similar compounds, he traces some of the connexions between changes of composition and of properties in analogous compounds. By considering the elements in groups the artificial difference between rare and common elements disappears, and this, I think, is a decided gain. The learner also begins to recognize that there are reasons for classing certain elements and compounds together; he sees that order and law pervade the vast domain of chemistry; he connects the constant *atomic weight* with the position of each element in the scheme of classification; and so he gains a basis on which he may rest the superstructure of facts as they are presented to him. This method of treatment inspires the learner with the hope that it is possible to get a firm hold of the subject he is studying. The method is progressive: principles are seen to arise out of the classes of facts considered; each event examined appears as at once the cause and the consequence of other events; generalizing on facts accompanies the acquisition of the facts themselves.

But if the student is expected to learn the properties of each element and its compounds, proceeding from element to element, he generally completely fails to grasp the connexion between similar elements; indeed, he usually and not unnaturally inquires why he should be burdened by these details, which seem to him only unmeaning facts: if he knows and can repeat the properties of half-a-dozen elements, what the better is he for knowing and being able to repeat the properties of a dozen more? The additional facts do not seem to help him to a knowledge of chemistry. And so he either despairs of finding any guiding light in the maze of facts, or he falls into the error of supposing that the maze in which he is wandering without a clue is chemistry. It is the old school method again, which teaches the uselessness of knowledge. When we look back on our school days do we not regret the hours wasted on learning rules and exceptions to rules? Do we not remember how hopeless it all was? how little we advanced? how we spent a year on this subject and a year on that, and failed to gain a grip of any? Did we not sometimes believe that every branch of knowledge was merely a collection of rules? Do not then let us teach science as we learned grammar; else the burden we attempt to bind on the shoulders of our pupils will be more grievous even than that which we bore ourselves in our youth.

The third point on which I would insist is that chemistry should be taught so as to impress the student with the importance of the subject, and with the fact that chemistry is a living and growing part of the scientific study of nature. A teacher of any branch of natural science must thoroughly believe in his subject; and he must have a vivid and active imagination. The subject he has to teach is so immense that, unless his imagination is clear and active, he forms blurred and inaccurate images of the natural occurrences which he wishes to put before his pupils, and so presents them with poor distorted pictures in place of the vivid and vivifying realities of nature.

I see no way of impressing chemical students with the greatness of the subject of their study other than that of constantly keeping before them the many-sidedness of the problems they are trying to solve; of constantly showing how even the smallest, and apparently quite detached, fact is really in living connexion with the whole science; and from time to time of reminding them that the subject of their study is but one part of natural science and is closely connected with many other branches of scientific investigation. But these things can be done only by the teachers and the taught constantly working side by side in the laboratory at some of those apparently simple chemical problems which branch off in many directions, and thus together gaining a real grasp of the many-sidedness of the subject they are studying. The student is thus convinced that, although "there are no boundaries in nature," yet it is necessary for us to draw boundary lines; he also learns the importance of those very facts which, when separated from each other and from the principles that bind them together, retain only a negative value. Science is more than knowledge, and we must make our students realize this. To be in touch with nature is what we aim at; and knowledge alone will not gain this end. We are striving for a real, living, imaginative, acquaintance with the laws of the universe, in order that our lives may thus become "rich and full and satisfying through realities and not through dreams."

Finally, I think that fear of the examiner acts very prejudicially on much of the chemical teaching of this country, more especially on the teaching in schools. For after all—I speak as an examiner—even the youngest examiner is fallible. He is not so very terrible a person as some teachers seem to suppose. Not unfrequently he is a foolish person who knows but little chemistry, and has recourse to text-books for good tips.

If it is really chemistry that is taught, a good examiner will soon find out that the students have learnt the real thing, and a bad examiner will perhaps be incited to leave his rules and definitions, and attempt to gain some real knowledge of the subject in which he examines.

I think that much more care ought to be exercised in selecting those who are to examine the results of the chemical teaching given in schools. Even the seats of the higher learning are scarcely yet impressed with the really tremendous importance of sound scientific teaching; and they still too often wish to get examiners who will set papers for schools in half-a-dozen subjects, instead of selecting men who have made special study of special branches of science, and asking them to examine in their own subjects.

I have not directly insisted much on the importance of research. Of course the aim of all scientific teaching must be to train up men competent to investigate nature for themselves. But unless the men are properly trained, and are taught by the examples as well as by the precepts of their teachers what true scientific research is, they will only add a few more facts to that vast gathering of these "brute beasts of the intellectual domain" which is so often but so falsely called chemistry; and they will persuade themselves that in doing this they are advancing the scientific study of nature.

I would sum up what I have tried to say in a few words. The teaching of chemistry is still too much under the trammels of the old scholastic methods; it stands too much in fear of books, and rules, and definitions. The teacher who is in earnest about his work must break through rules, he must "swallow all formulæ," he must go constantly to nature and take his pupils with him; and his reward will be great.

BOTANY OF THE RIUKIU (LOOCHOO) ISLANDS.

WITHIN the last score of years much light has been thrown upon our knowledge of the flora of Japan. We still, however, know little about the flora of the groups of islands which lie scattered off the coast of her southern boundary. It is true that some botanical collections made during the last few years have shown a certain insight into the flora of some of the archipelago known as the Riukiu or Loochoo Islands, but it is equally true that most of the islands remain as yet absolutely uninvestigated. Since careful studies of the materials, both literature and specimens, scanty as they are, have shown that the flora of the Riukiu Islands form obviously the connecting link between that of Japan, on the one hand, and, on the other, those of South-Eastern China and the Indo-Malayan region through the islands of Hong Kong and Formosa, it seems necessary to take a clear view of the flora of the Riukiu Islands where the boundary lines of those of the two above main regions overlap. Hence it may be worth while to offer a brief summary of our present knowledge of the flora of the Riukiu Islands, taken not only from the materials already presented to the scientific world, but also from those works which have been brought out by the hands of native botanists of Eastern Asia, which have not yet been accessible to most of the Western men of science.

We observe that the earliest records of the plants of the Riukiu Islands are found in that section of the work entitled "Chuzan Denshinroku," or "Records of the Riukiu Islands," which deals with the natural products of that archipelago. These records are detailed in an extended form in another work, the "Riukiu Sambutsushi," or, "The Natural Products of Riukiu." But, as to the works which treat exclusively of the botany of the Riukiu Islands, reference should be made to the "Riukiu Sōmoku Shin Dsu" ("Illustrations of the Plants of Riukiu") and to "Shitsumon Honzō" ("Queries on the Botany of the Riukiu Islands"). The latter, on account of the excellence of its illustrations of plants, and also on account of some interesting facts connected with the preparation of the work, demands here special mention. The author, Go Keishi, a physician of the Island of Okinawa, collected during many years not only the plants of the island in which he lived, but also specimens from the Isles of Takara and Yokō, both of which are situated near 29° N. lat. and 129° E. long. He drew figures, and gave brief descriptions, of these plants, accompanied by dried specimens, forming a collection of about seventy or eighty species of plants at each time, and sent them annually, between 1781–85, to China, in order to acquire further information about those plants which he considered to be doubtful. No fewer than forty-five Chinese representative physicians and herbalists of that time, in various parts of the country, who studied these collections, added information; they also suggested queries, many of which, however, were vague and perplexing. The results thus accumulated formed, in 1789, four volumes, exclusive of a supplementary one. These were divided into sections, "Nai-hen" and "Gwai-hen," referring to those plants used in medicine for internal and external purposes respectively. Each of these two sections is again subdivided into four parts. The work was afterwards published, about 1835, by the order of the then Prince of Satsuma, of the province of Kiusiu, but appears to me to have been printed in Yédo (Tokio). The excellence of the illustrations throughout the work makes it easy for botanists to recognize the characters of plants which are represented in the work. To take an instance, it is interesting to learn in this work, that *Epimedium macranthum*, with its violaceous variety, is found in the Riukiu Islands.[1] There is additional interest in the illustration of a species of *Balanophora*, a genus of the order now known to occur from tropical parts of Asia to nearly 34° N. lat. of Japan, through Hong Kong and the Riukiu Islands; which I have lately treated elsewhere (Journ. Linnean Society, Bot., vol. xxiv.).

Now, to examine the Western literature of the flora before us. Though no small number of plants, recorded as indigenous to the Riukiu Islands, are mentioned in botanical literature,[2] the first compact exposition of the flora, drawn up under the careful examination of a number of specialists, is a memoir which appeared in the fourth and fifth volumes of *Engler's Botanische Jahrbücher*, published in 1883 and 1885 with the title "Beiträge zur Flora des südlichen Japan und der Liukiu-Inseln." This memoir, based principally upon the specimens collected by Döderlein and Tashiro in Ohshima, enumerates ninety-five species of plants from that part of the Riukiu Islands. Of these, three species are shown to be new, one of which, *Asplenium Döderleinii*, belongs to Vascular Cryptogams, while the remaining two, *Sceleria Döderleinii* and *Cinnamomum Döderleinii*, represent respectively Mono- and Di-cotyledons. Among these ninety-five species, sixty were previously known to occur in Japan, and also in other parts of Eastern Asia, the remaining thirty-five being unknown in Japan. Of these thirty-five species, sixteen are known to occur in China and the Indian Archipelago, seven in Australia, and the rest in Malacca, Ceylon, Himalaya, and other places. It must, however, be remembered that a Leguminous plant, *Lotus australis*, collected in the Riukiu Islands, though previously known in Australia, has not yet been discovered in any of the transitional regions.

Early in 1886, Maximowicz published, in the *Bullétin* of the Academy of Science of St. Petersburg, the sixth part of his work on the plants of Eastern Asia, which added no less than fifty species of Riukiu plants to those contained in *Engler's Jahrbücher*. The main portion of these plants were collected by Tashiro, who, working among other groups of the islands, made a preliminary contribution to our knowledge of the flora of Miyako-sima, a small isle lying in 45° N. lat. and 125° E. long. The new species here described from Riukiu Islands are eight, viz. *Euonymus Tashiroi*, *Galactia Tashiroi*, *Erythræa japonica*, *Anœctochilus Tashiroi*, *Premna staminea*, *P. glabra*, *Rhododendron Tashiroi*, and *Webera retusa*. The first five species are endemic to the Riukiu Islands, while the last three are found as well in the adjacent isles of Kiusiu, and in the Ogasawara Islands.[3] I may here remark that a new genus of Rubiaceous plant, established by Ahlburg (in *Bot. Zeitg.* 1878, p. 113), under the name *Aucubæphyllum Liukiuense*, which has remained doubtful, will probably be identified with *Psycotria elliptica*.

Lastly, Forbes and Hemsley's "Index Floræ Sinensis," which is intended to include all plants known in China proper, Corea, and their adjacent islands, but excluding Japan, does nevertheless embrace the flora of Riukiu Islands within its scope. As only the first two parts of this important work have made their appearance, it will perhaps be better not to draw any conclusion at present; still it may be of some interest to point out that we are now practically furnished with our first knowledge of the specimens of plants collected in the Riukiu Islands by Charles Wright and a few other botanists, and deposited in those two great botanical store-houses, the Kew Herbarium and the British (Natural History) Museum.

We cannot but feel how imperfect is our knowledge of the flora of the Riukiu Islands. We should attach much importance to its further investigation. For careful examination of the southern group of the Riukiu Islands will probably bring out, to some extent, the relations between the floras of China and Japan, and between those of the latter and the Philippine Islands, the Indian Archipelago, and perhaps even that of Australia. It will be understood that this southern group of islands, known as Yayeyama, which is situated about 24° N. lat. and between 123° and 124° E. long., remains as yet absolutely uninvestigated. The Yayeyama group consists of nine isles—namely, Ishigaki, Iriomoté, Taketomi, Kobama, Hatoma, Kuro-shima, Arakusuku, Hateruma, and Yonakuni; besides which there are three smaller ones—Uchi-Hanaré, Soto-Hanaré, and Kayama-shima, the two latter being the only uninhabited isles in the group. Hateruma, which is situated in 24° 4′ N. lat., is known as the southern extremity of the Japanese Empire. We rejoice to be able to state that our friend, Y. Tashiro, who had previously made some important collections of the plants of the Riukiu Islands, has, by the commission of the Japanese Government, lately extended his researches to this Yayeyama group, where he resided during 1885 and 1886. We hope that the collection resulting from his incessant labours will be intrusted to competent hands, and that an adequate account of it will soon be published.

Cambridge. TOKUTARO ITO.

[1] Up to the time of the publication of my paper on the "Berberidaceæ of Japan" (Journ. Linn. Soc. 1887, vol. xxii.), I found that no one had collected specimens of this plant in the Riukiu Islands.

[2] Hooker and Arnott's "Botany of Beechy's Voyage" contains some account of the plants collected in the Riukiu Islands.

[3] Generally known as the Bonin Islands, which is probably a corruption of *Munin*-jima, *i.e.* destitute of man, another Japanese name for the islands.

NOTES.

AN interesting collection of specimens has just been received at the Natural History Branch of the British Museum, Cromwell Road, from Emin Pasha. They were despatched from Wadelai in November last, *viâ* Zanzibar, through the kind assistance of Mr. Mackay, of the Church Missionary Society in Uganda, and have arrived at their destination in good condition. The collection consists of skins of birds and mammals, butterflies, and some anthropological objects, and, when worked out by the officers of the Museum, will be described in detail at one of the meetings of the Zoological Society during the ensuing session. In a letter received a few days ago by Prof. Flower, dated Wadelai, April 15, Emin Pasha speaks of a further consignment of specimens (chiefly ethnological), as being ready for despatch to the Museum on the first opportunity.

IN 1884 Mr. John Ball, F.R.S., published a paper in the Journal of the Linnean Society (vol. xviii. pp. 203–240) giving the first comprehensive account of the flora of North Patagonia. This was based on a collection obtained from him during his travels in South America from M. Georges Claraz, a Swiss gentleman who had passed several years chiefly at Bahia Blanca. Mr. J. L. Williams-Andrews has now sent to Kew a beautifully-preserved collection made by him in the same region during the years 1881–85. The excellence of the specimens is the more remarkable as the majority of them have travelled more than six hundred miles on horseback. Mr. Williams-Andrews writes to Kew :—"The Indians are certainly a very fast-decreasing race, and at the present day cannot exceed two thousand in number. The combs, or rather brushes, mentioned [by Mr. Ball in his paper, p. 225] are formed of a species of very tough grass, not of roots. The use of vegetable dyes is also rapidly dying out amongst them, though they still make a considerable quantity of textile fabrics, of which I have numerous examples ; the same is to be said of their silver ornaments." Vice-Consul Goodhall, of Bahia Blanca, has taken much trouble to obtain information about the plants used by the Indians for dyeing purposes. He has unfortunately failed in obtaining any trustworthy specimens, as "the Indians are most jealous about affording any information on the subject." Mr. Ronald Bridgett, Consul at Buenos Ayres, has sent to Kew some articles dyed by the Indians, in which the greens and yellows are native dyes made from roots and plants. These have been sent to the Chemistry and Dyeing Department of the Bradford Technical College. A number of Indian ornaments, also obtained by Mr. Consul Bridgett, have been forwarded to the Ethnographical Department of the British Museum.

A HIGHLY ingenious modification of Cowper's writing telegraph has been shown at the American Exhibition, by Mr. J. H. Robertson, an American electrician. The movement of a pen at the sending station varies the resistance of two electric circuits along which two currents are flowing. These varying currents act upon two coils at the receiving station so as to impart motion in two directions to a pen filled with ink, so that the resultant motion of this pen exactly reproduces the movement of the writing pen at the sending station. Mr. Robertson has replaced Mr. Cowper's resistance coils by a series of thin carbon disks, which vary their resistance with variation of pressure, as was discovered by Edison and utilized in his carbon telephone transmitter. He has also improved the receiving portion, and has made the apparatus very practical. It is being commercially worked out in the United States, and we shall watch its progress with much interest. It forms a really beautiful system of written messages, and is decidedly simpler than any previous system of facsimile telegraphy. It is very doubtful whether there is a demand for such a system, for the operation is necessarily slow.

THE session of the International Hygienic Congress at Vienna was closed on Sunday, when it was finally decided that the next session should be held in London in 1891. The meetings were remarkably successful, and did much to enlighten the public as to the nature of the questions which are now being discussed by students of hygienic laws. On Wednesday, September 28, interest was centred chiefly in the Third Section, where the circumstances under which cholera is disseminated were considered. Prof. Max Gruber, of the Vienna University, who gave an account of the incidents of cholera in Austria during the years 1885 and 1886, stated that he could find no evidence of water having played any part in disseminating the disease during that period. He believed that cholera was disseminated by human intercourse, and this experience, he said, coincided with that of English observers. On the other hand, Dr. Spattuzzi, of Naples, attributed the absolute immunity from cholera enjoyed by Naples during 1885 and 1886, and the comparatively small extent of the disease during the present year, to the excellent water supply provided in 1884. Prof. Pettenkofer made some interesting statements on the influences which, in his opinion, locality and season have on the spread of cholera. In support of his views he referred to experiences in India, where each province has its own time of year when the disease is more prevalent, but he also freely admitted the effects of pilgrimages and fairs in spreading the disease. In the course of the debate Prof. Pettenkofer again took occasion to pay a high tribute to England for the measures adopted for the prevention of cholera ; and M. Proust, of Paris, expressed himself in the same sense. Thursday was devoted to excursions and the visiting of public institutions in Vienna. On Friday Sir Douglas Galton, who presided over the First Section, offered some valuable remarks on the treatment of infectious fevers. He showed that in London much had been done by the system of isolating small-pox and scarlet-fever patients quickly, by taking them to a ship-hospital, or to hospitals remote from dwelling-houses. "But it is most undesirable," he added, "that in these isolated hospitals too many patients should be concentrated in one ward. The principle should be smaller wards, of four to six patients at most, and great simplicity of construction with ample aëration." Sir Douglas expressed the opinion that the bodies of patients who die of infectious fevers should be burned, and in this view he was supported by Sir Spencer Wells, who said that the good done by giving the people pure air and water, wholesome food, and proper dwellings must to a large extent be counteracted by the continual presence of thousands of putrefying bodies in and around centres of population. On Saturday much interest was excited by the proceedings of the Third Section, when the question of preventive inoculation against rabies was discussed. At the final meeting on Sunday morning the usual votes of thanks were passed, and Prof. Ludwig, the President, said that all the objects of the Congress had been attained. Dr. Roth, of London, expressed a hope that the "protectorate" of the next Congress would be undertaken by the Prince of Wales.

SIR SPENCER WELLS was entertained at a grand banquet on Friday evening last by some of the leading surgeons and professors of Austria-Hungary and by a deputation of the younger surgeons and students. Only one toast was given—that of "The Guest of the Evening." It was proposed by Prof. Breisky, who said that he and many others present were indebted to the teaching of Sir Spencer Wells for much of their success. Replying to this toast, Sir Spencer Wells spoke of the changes which have taken place in surgery during the last thirty-five years, and of the great results accomplished by wise sanitary legislation. "If," he said, "we had in England full power, a competent Minister of Public Health, and an efficient staff of health officers and engineers, the present death-rate of London—that is, 19 in a thousand—might certainly be reduced to 14, or probably to 12."

MR. F. A. BATHER AND MR. G. W. GREGORY have been appointed assistants in the Department of Geology of the British Museum (Natural History), to fill the vacancies caused by the resignations of Mr. William Davies and Mr. Robert Etheridge, Junior.

ON Saturday last the foundation-stone of the Walker Engineering Laboratories in connexion with University College, Liverpool, was laid by Sir James Poole, Mayor of the city. Sir A. B. Walker's original gift to the institution was a sum of £15,000, but Sir James Poole was able to announce that Sir Andrew had generously increased the amount to £20,000.

BY his will, dated August 5, 1887, Mr. Richard Quain, F.R.S., who died on September 15, has bequeathed almost the whole of his fortune, amounting to about £75,000, to University College, London, subject to certain annuities to family connexions. The College will at once benefit to the extent of about £60,000. The annual income is to be applied to "the promotion and encouragement in connexion with University College, London, of general education in modern languages (especially the English language and composition in that language) and in natural science." The trustees, Lord Justice Fry, Sir William Jenner, Mr. George Brodrick, the Warden of Merton, and Sir George Young, are authorized to carry out the testator's wishes either by salaries "or other payments to those who teach, as by endowing professorships, or by pecuniary aid to those who are being taught, or by endowing scholarships, or fellowships, or in any other manner which the trustees may in their absolute discretion think proper," and they are requested to place themselves in communication with the Council of University College with a view to preparing a scheme for carrying out the objects of the bequest. The testator desires that in any statement of the foregoing bequest the name of his brother, the late Sir John Richard Quain, shall be associated with his name.

A SEVERE shock of earthquake was felt throughout Greece at 1 o'clock on Tuesday morning, the 4th inst., the strongest disturbance being on the northern and southern shores of the Gulf of Corinth.

MR. D. NUTT will publish shortly a new and thoroughly revised edition of Brenicker's "Logarithms, with Supplementary Tables of Natural Functions and Circular Measures of Angles to each Minute of Arc," by Prof. Alfred Lodge, of Cooper's Hill College.

MESSRS. MARCUS WARD AND CO. will issue in a few days "Teneriffe and its Six Satellites," a new work of travel in two volumes by Mrs. Olivia M. Stone, author of "Norway in June." Together with a narrative of wanderings through the seven inhabited islands, Mrs. Stone gives an historical account of the past race of inhabitants, and she draws attention to the value of the archipelago as a health resort. She has also something to say about the flora, and tables of temperature are appended.

THE third scientific voyage of Prince Albert of Monaco, in his schooner, the *Hirondelle* (200 tons), terminated at the end of August, when the vessel came back to Lorient. The Prince was accompanied by Prof. Pouchet, who made a special study of currents; and by M. Guerne, whose work was zoological. Leaving early in June, they went to the Azores, where three weeks were spent in dredging, &c. A newly-captured sperm-whale was examined. The fauna of the lakes at the bottom of craters was studied by M. Guerne. The Gulf Stream was then crossed, and a thousand of the Prince's floats were thrown out. At St. John's, Newfoundland, researches were continued. The vessel was then directed northward along the coast, but bad weather put an end to the project. In returning to Europe the party encountered a violent storm, in which grave damage was averted by the use of oil. The voyage is considered a great success. A noteworthy feature of it is the carrying on of productive and difficult dredging operations entirely without steam.

EXPERIMENTS have been recently made on the Seine, in the stoppage of steamers in motion, by means of a "cable-anchor" invented by M. Pagan. This is a cable having on it a series of canvas cones, which open out by the action of the water, and close again when drawn in the opposite way. The steamer *Corsaire*, running 13 knots, was stopped each time by the apparatus in seven or eight seconds, and in a space of 26 to 30 feet at the most. For comparison, the steamer, running full speed, was stopped in the usual way, by reversal of the engines. This took at least thirty-four seconds, and the space was 350 to 360 feet. It would thus appear that M. Pagan's apparatus effects the result in less than a tenth of the space, and a fourth of the time, of the ordinary method.

IN clinical practice it is often desirable to be able to measure in a simple and direct way the speed with which nerve-impressions are conveyed. An electric chronometer for this purpose by Dr. D'Arsonval, is described in a recent number of *La Lumière Électrique*. An axis with small terminal plate is driven round uniformly by clockwork, making one turn per second. Opposite the plate is another plate connected with the axis of a pointer on a dial. These axes are independent while a current passes through a small electro-magnet holding the second plate; but when this current ceases, a spring brings the latter in contact with the moving plate and the pointer is carried round till the current flows again. The patient to be examined having shut his eyes, the doctor applies to a part of the body a simple instrument which by this application breaks circuit, so that the pointer begins to travel. The patient is required, immediately on feeling the pressure, to press a button, which makes the circuit again, and the pointer stops. The interval can then be determined in hundredths of a second. The velocity of sensations in different parts of the body can thus be compared very rapidly. It is proved that different sensations (pressure, heat, pricking, cold, electricity, &c.) are transmitted with different velocity, and some diseases abolish some while exalting others, &c.

PROF. MUSHKETOFF'S account of his explorations in the Caspian steppes contains some interesting remarks on the work done by marmots (*Spermophylus eversmanii*) in the modification of the surface of the steppe. They made their appearance in the region only a few years ago, but their heaps of earth already cover hundreds of square miles. Like earthworms, they must therefore be regarded as a factor of some importance in modifying the surface of the soil. Their heaps of earth have an average length of 3½ metres, and a width of 2½ metres, with an average height of from 30 to 50 centimetres, and it was found that on each 2 square metres there were no less than five, seven, or even eight heaps, each of which represented at least 2 cubic metres of earth removed. It may be safely asserted that on each square kilometre of surface no less than 30,000 cubic metres of earth have been brought to the surface owing to their activity. Their influence on vegetation is also well worthy of notice.

AT a recent meeting of the Paris Biological Society, M. Mégnin gave an account of a peculiar disease which is very prevalent at present among hares in Alsace. It is a parasitic disease, a sort of pulmonary tuberculosis, caused by the presence, in the lungs, of *Strongylus commutatus* (*Filaria pulmonalis* of Frölich). The same disease was noticed in Thuringia in 1864.

AN introductory lecture at St. Mary's Hospital was delivered on Monday last by Mr. A. Critchett, Ophthalmic Surgeon to and Lecturer at St. Mary's Hospital. Speaking of the studies in which he himself is chiefly interested, Mr. Critchett said he was old enough to remember the introduction of the ophthalmoscope by that great teacher and thinker Helmholtz, who, he rejoiced to say, yet lives to witness the priceless boon which his discovery has conferred upon the human race. It was difficult for those who are now familiar with its use to conceive the wondering eagerness with which the original workers sought, by the aid of their new weapon, to bring to light those numerous hidden diseases of the eye which had till then been only partially and most imperfectly recognized. Numerous modifications of the instrument have since been introduced, and among the most recent and most useful improvements has been the ingenious adaptation of the electric light to the ophthalmoscope by Mr. Juler. After alluding to the labours of his late father, of the venerated Nestor of English ophthalmic surgery, Sir William Bowman, and of the much lamented Von Graefe, the lecturer reminded his hearers of the colossal work which had been achieved by Prof. Donders, who had opened out a new world for thought and investigation, and had elevated the study of practical optics to the dignity of a science.

A REMARKABLE series of experiments upon the synthesis of water by weight is described by Dr. E. H. Keiser in the current number of the *Berichte* of the German Chemical Society, in which not only has a known weight of oxygen in the form of copper oxide been employed, but has also been made to combine with an actually weighed quantity of hydrogen. In the well-known experiments of Dumas it will be remembered that an indefinite quantity of hydrogen was employed, the loss of oxygen by the copper oxide and the weight of water formed furnishing the only data obtainable by the then possible experimental methods. But Dr. Keiser has succeeded in weighing his hydrogen by taking advantage of its peculiar property of being occluded by the metal palladium; it is shown that a piece of palladium 100 grammes in weight will readily take up between 0·6 and 0·7 grammes of hydrogen, when heated in a stream of the gas to about 150°. The palladium-hydrogen compound formed is perfectly stable at ordinary temperatures, and may be preserved unchanged in a vessel filled with hydrogen gas; but on heating this *quasi*-alloy the gas is slowly driven out again, and by weighing before and after the heating, the weight of hydrogen expelled may be accurately determined. This weighed quantity of hydrogen gas was then passed over heated copper oxide, and the weight of water formed determined in the usual manner. But Dr. Keiser has gone further than merely synthesize water by the most direct means possible, he has refined the process so far as to be enabled to employ it as a direct means of determining the atomic weight of oxygen. The minutest precautions were taken against error, and the purification of the hydrogen carried out in a most thorough manner, with the unexpected result that the atomic weight of oxygen is most probably slightly lower than 15·96 and more nearly 15·87. It is interesting to be reminded that Dumas states in his memoir:—"Of all analyses, that of water involves the most uncertainty. It is true that one part of hydrogen combines with eight parts of oxygen to form water, and nothing could be more exact than the analysis of water, if one were able to weigh the hydrogen and the water formed by its combustion. But the experiment is not possible in this form." However, owing to the ingenuity of Dr. Keiser, this happy result has now been achieved.

THE *Times* of September 22 contained an interesting account of the Troglodyte remains in Southern Morocco. The difficulties placed in the way of exploration in Morocco have prevented an examination hitherto of these remains. The Troglodyte caves are situated near Ain Tarsil, a village some three days' ride to the south-west of the city of Morocco, near the borders of the province of Haha. At the village the scenery undergoes a complete change. Instead of the dreary plains one comes across curious hills divided by great ravines, not unlike the cañons of California, in one of which is situated the strange city of the Troglodytes. The gorge is a narrow one, the cliffs on either side rising almost perpendicularly from the bottom of the deep valley; after progressing some little way along this valley the first caves are sighted. They are cut in the solid rock at a great height from the ground, and are in some places single, in others in tiers of two or three, one above the other. The entrances are small, varying from 3½ to 4½ feet in height and about 3 feet in breadth. In places where the rock has fallen away the entrances are faced with masonry of a neat and orderly type, and in one or two cases where the natural formation of the rock necessitated exceptionally large entrances this masonry served also the purpose of dividing the door into two parts, one of which no doubt served as a window. As to the means of access to these caves, the writer discusses three theories, all possible, but one alone of which is probable. The first is that the face of the cliff has since the formation of the caves been worn away by wind and water, and so crags and projecting pieces of rock that once rendered access possible have disappeared; secondly, the Troglodytes may have been so active as to be able to get up perpendicular smooth rock; the third, that they used ropes and ladders. He dismisses the first as unjustified by anything we know of Morocco, and inconsistent with the existence of various tiers of cliffs; and as to the second, although it is stated that these cave-dwellers were swift of foot as a horse, the shape of the cliff renders climbing an impossibility. The walls of rock are perfectly smooth from the bottom to the height of the caves, which is 200 to 300 feet in many cases, and it is only in a few places that there are ledges sufficiently wide for birds of prey. In favour of the ladder theory there is the circumstance that in the doorways of many of the caves there still remains, a few inches above the floor, and crossing from lintel to lintel, a bar of wood some six inches in thickness, which must have acted as a roller for ladders. The freshness of the wood is surprising, but in the Karli Caves in the Western Ghauts there is a roof which is considered by experts to be coeval with the caves themselves. The writer was able to enter the caves in only one or two cases, a landslip having built a pile of earth and stones that rendered access possible. Passing through the low, narrow doorway, the visitors found themselves in an oblong room 15 feet by 7, leading from which at either end were smaller chambers. The wall of the larger room adjacent to the cliff was pierced with three windows; each of the smaller ones also possessed a window. The walls were rough, but the floor and ceiling were well smoothed and cut. The height of each was about 5 feet 2 inches. In no cases were bones discovered, only a little broken pottery and one or two doubtful flint-heads alone being found. No doubt much greater success will await the explorer who manages to overcome Moorish prejudice and bigotry, and by means of ropes and ladders enters the upper tiers of caves which have lain in their present state since the old race died out or took to more rational abodes. The Troglodytes cannot have been such savages as they are usually considered: their excavations are hollowed out skilfully, and their abodes show signs of great labour and some idea of care and comfort.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented by Miss Barker; two Arctic Foxes (*Canis lagopus*) from the Faroe Islands, presented by Mr. T. Nordenfelt; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. R. Taylor; a Corn-crake (*Crex pratensis*), British, presented by Mr. Howard Bunn; a Ring Dove (*Columba palumbus*), British, deposited; a Sumatran Wild Dog (*Canis javanicus* ♀) from Sumatra, purchased; a Wedge-tailed Eagle (*Aquila audax*) from Australia, received in exchange; a Mule Deer (*Cariacus macrotis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

FLAMSTEED'S STARS "OBSERVED BUT NOT EXISTING."—Prof. C. H. F. Peters, in a paper published in vol. iii. of the Memoirs of the National Academy of Sciences of Washington, has discussed Flamsteed's observations of the twenty-two stars which, it will be remembered, Baily, in his "Account of the Rev. John Flamsteed," entered in a list with the above heading, which, as he explains, means stars "of which the observations appear to be accurately recorded, but which still cannot now be found in the heavens." As the disappearance of so many stars in comparatively so short an interval of time appeared to Prof. Peters to be rather improbable, it seemed to him to be desirable that these cases should be scrutinized with the additional means of identification provided by the *Durchmusterung*, which of course was not available at the time of Baily's publication. Acting, then, on the assumption that some otherwise plausible error in Flamsteed's entry of his observation which leads to a modern star-place is to be held much more reasonable than the vague acquiescence in a supposed disappearance of the star, Prof. Peters considers that (making allowance for the probable error of a position in Flamsteed's Catalogue) he has succeeded in finding for every case at least a probable explanation. One of the observations in question—that of the object No. 1647 in Baily's "Flamsteed"—turns out to have been an observation of the planet Uranus, an explanation which was suggested by Argelander but rejected by Baily. The agreement, however, of the position of the object observed by Flamsteed with the place of Uranus computed from Newcomb's Tables is so close as to leave no doubt of its identity.

CORRIGENDA IN VARIOUS STAR-CATALOGUES.—The paper following the above in vol. iii. of the Memoirs of the National Academy of Sciences is likewise by Prof. Peters, and contains a very extensive list of corrigenda to various star catalogues, the great majority of which have hitherto been unknown. Following Argelander's example, Prof. Peters has not been content with simply detecting an error, but has in most cases turned to the original observations, when accessible, to discover the source of error. The catalogues in which the corrections, nearly 700 in all, have been made, are: Oeltzen's Catalogue of Argelander's southern zones, Vol. vi. of the Bonn observations, Weisse's Catalogue of Bessel's zones, between Decl. – 15° and + 15°, and between + 15° and + 45°, Rümker's Catalogue of 12,000 stars (original catalogue and the new series), Schjellerup's Catalogue, Baily's Lalande, Yarnall's Catalogue (second edition), the Glasgow Catalogue of 6415 stars, Moesta's observations at Santiago de Chile, and the Geneva observations, 1842-49. The corrigenda which Prof. Peters has discovered for the Harvard zones have been published in the Annals of the Harvard College Observatory, vol. xiii. pp. 188–208, and are not given here; those for the Washington zones have been communicated to Prof. Holden, and those for Lamont's publications to the astronomers of the Munich Observatory. Prof. Peters certainly deserves the hearty thanks of all astronomers for the very essential service he has rendered in the detection and publication of these errors in their standards.

Besides these lists of errata, Prof. Peters also gives in the same memoir a list of 191 "Anonymous" stars in Yarnall's Catalogue, which he has identified with stars in other catalogues.

THE "SATELLITE" OF VENUS.—One of the standing enigmas of astronomical history has been the number of observations, the great majority of which were made in the years 1761 and 1764, by astronomers of reputation, of some small object close to Venus which was supposed to be a satellite of the planet. The fact, however, that the observations were consistent with no possible orbit, and that no trace of the body has been seen for more than a century, were conclusive proofs that it was not a real satellite, and many theories were started to account for the observations, but all of them were open to one or more fatal objections. The problem, however, seems now to have been fairly solved at last. M. Stroobant, in a paper which he has recently communicated to the Académie Royale de Belgique, gives evidence to show that in a large number of instances the object supposed to be a satellite was actually a star. He was led to this conclusion from the result of an inquiry into the observation of Rœdkiœr and Boserup, August 4, 1761, in which a star, as well as the "satellite," was mentioned as being near the planet. Curious to find out what star this was, he reduced its place and found it to be χ_3 Orionis. It then immediately struck him that the so-called satellite occupied the place of χ_4 Orionis. An attentive examination of the other observations showed that in many cases these also were probably of stars; that of Horrebow's on January 3, 1768, was unquestionably an observation of θ Libræ. Not only did the star occupy the precise place indicated for the "satellite," but the motion of Venus was such as to produce just the apparent motion ascribed to it. Several other observations can be almost as clearly referred to some star or other; the chief objection to such identification—that some of the stars in question are too faint to have been seen near the planet—being overruled by M. Stroobant's own observations, he having found that with a telescope of 6 inches aperture a star of the eighth or even of the ninth magnitude could be seen in the immediate neighbourhood of Venus. In order to present the whole question as fully as possible in one view, M. Stroobant has exhibited all the thirty-three observations in tabular form, has given all the various particulars required referring to Venus, together with an abstract of all the different theories hitherto broached respecting the true nature of the "satellite," and has reprinted the original observations themselves, whilst his own identifications with stars are illustrated by a neat series of little star-maps. Only one series, those of March 1764 (printed by a curious typographical error as Mars 1861), by Rœdkiœr remain without at least a plausible explanation, and it is possible that in this case it may be found that one of the brighter minor planets was sufficiently near to Venus to be seen in the same field with her. At all events, for the great proportion of the observations M. Stroobant has fairly cleared up the mystery which has perplexed astronomers so long.

THE LEANDER MCCORMICK OBSERVATORY.—From Prof. Ormond Stone's Report for the year ending June 1, 1887, recently received, we learn that the 26-inch refractor has been chiefly employed as heretofore in observations of nebulæ. 351 observations of miscellaneous nebulæ have been made during the year, as well as a large number of sketches, and 270 nebulæ have been discovered, which are supposed not to have been hitherto detected. Efforts have also been made to determine with as much accuracy as possible the positions of a select list of nebulæ in order that materials may be accumulated for the determination of their proper motions. Special attention has been paid to the nebula in Orion. Holding that it is not to photography but to photometry that we must look for the earliest possible evidence of change in this object, Prof. Stone has repeatedly examined the brighter portion of the nebula for the purpose of determining the relative brightness of the various condensations of which it is composed. The region "A" preceding the trapezium has been especially observed, and the brightness of its condensations compared with one another and with portions of the "Huyghenian" region. Estimates have also been made of the relative brightness of the stars in the brighter portion of the nebula in order to trace if possible any connexion which may exist between them and the nebula.

THE NICE OBSERVATORY.—M. Perrotin has just published the second volume of the "Annals of the Nice Observatory"; the first, which will contain a description of the Observatory and of its instruments, has not yet appeared, but is in preparation. The present work contains six sections, of which the first is devoted to the determination of the difference of longitude between Paris and Nice, and between Nice and Milan, which M. Perrotin set about making immediately on the foundation of the Observatory and his appointment to the Directorate. In this work he was joined by Commandant Bassot, who observed at the Observatory of the Dépôt de la Guerre, Montsouris, during the first part of the operations, and then exchanged places and instruments with M. Perrotin, the better to eliminate personal errors; M. Celoria, of the Brera Observatory, Milan, co-operating with M. Perrotin in the determination of the Nice-Milan longitude. The observations were made in the autumn of 1881, and the final results agreed very closely with the Milan-Paris longitude which had been determined in the preceding July and August by Colonel Perrier and M. Celoria, as the following figures will show:—

	h. m. s.	h. m. s.
Paris-Nice ...	0 19 51·513 ± 0·01	0 27 25·325
Nice-Milan ...	0 7 33·812 ± 0·01	
Paris-Milan (direct)		0 27 25·315

The Montsouris instrument being 0·288s. to the west of the meridian of Paris, the longitude of the pillar upon which the Nice meridian instrument was mounted is 0h. 19m. 51·225s. east of Paris.

The second section contains the determination of the provisional latitude of the Observatory, which was found to be 43° 43′ 16″·9; the pillar of the small meridian instrument being still, as for the longitude, the place from whence the observations were made. The third section contains a fine series of micrometric measures of double stars, made by M. Perrotin with an equatorial, by Eichens and Gautier, of 0·38 metre aperture and 7 metres focal length; the observations of comets and planets, which were published as made in the *Comptes rendus* of the Paris Academy, and which occupy the fourth section, being made with the same instrument. Some important notes on solar spectroscopy by M. Thollon follow in the fifth section, and include several remarkable observations of solar storms, a correspondence with M. Faye on the interpretation to be attached to the displacements and contortions of the spectral lines, and a study of the B and D groups in the solar spectrum. The concluding section contains notes by MM. Thollon and Puiseaux on the total solar eclipse of May 17, 1882; on the transit of Venus, 1882, by M. Thollon; on the remarkable crepuscular glows and "coronæ" of 1883–84, by MM. Perrotin and Thollon; and elements of Comet 1885 II. (Barnard) and of Minor Planet No. 252 (Clementina), by M. Charlois. The volume is illustrated by seven beautifully finished plates; one of which, viz. that to illustrate M. Thollon's paper on the group, affords an example of the fullness of information and beauty of execution of M. Thollon's drawings of the solar spectrum which are now being engraved for publication in the forthcoming third volume of the "Annales de l'Observatoire de Nice."

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 OCTOBER 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 October 9

Sun rises, 6h. 15m.; souths, 11h. 47m. 20·0s.; sets, 17h. 19m.; decl. on meridian, 6° 15′ S.: Sidereal Time at Sunset, 18h. 31m.

Moon (at Last Quarter October 10, 5h.) rises, 21h. 6m.*; souths, 5h. 6m.; sets, 13h. 9m.; decl. on meridian, 19° 54′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	8 10	12 58	17 46	14 30 S.
Venus	4 9	10 5	16 1	1 34 S.
Mars	1 32	8 48	16 4	13 49 N.
Jupiter	8 27	13 19	18 11	13 52 S.
Saturn	23 31*	7 20	15 9	19 16 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. °	°
11	ζ′ Cancri	4½	3 48	4 16	343	299
12	π′ Cancri	6½	5 57	near approach	323	—
13	Regulus	1½	4 44	5 50	34	225
14	χ Leonis	5	5 15	near approach	305	—

Oct.	h.	
10	13	Venus stationary.
11	15	Saturn in conjunction with and 1° 20′ north of the Moon.
13	8	Mars in conjunction with and 0° 19′ north of the Moon.
14	8	Mercury in conjunction with and 2° 58′ south of Jupiter.
14	14	Venus in conjunction with and 7° 52′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Oct. 13,	4 32	*m*
ζ Geminorum	6 57·4	20 44 N.	,, 9,	21 0	*m*
			,, 14,	21 0	*M*
S Canis Minoris	7 26·6	8 34 N.	,, 13,		*M*
S Herculis	16 46·8	15 8 N.	,, 14,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 12,	3 11	*m*
		and at intervals of		20 8	
X Sagittarii	17 40·5	27 47 S.	Oct. 13,	0 0	*m*
			,, 15,	21 0	*M*
U Sagittarii	18 25·2	19 12 S.	,, 11,	0 0	*m*
			,, 13,	23 0	*M*
η Aquilæ	19 46·7	0 43 N.	,, 12,	21 0	*m*
S Sagittæ	19 50·9	16 20 N.	,, 11,	22 0	*m*
			,, 14,	22 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near γ Persei	44	55 N.	Slow.
,, 41 Arietis	45	26 N.	Swift.
,, τ Geminorum	103	33 N.	Swift; streaks.
	135	80 N.	Swift; streaks.

GEOGRAPHICAL NOTES.

M. Serrano's recent expedition, and the second expedition which was organized by the Chilian Government in order to determine the watershed between the east and west coast of South America, have settled the most interesting fact that the high chain of the Andes in these regions does not form the watershed between the Atlantic and Pacific Oceans, but that it lies somewhat further east of it, on a plain about 500 metres high. The rivers which rise here and flow towards the Pacific have their source in small lakes, and pass through the Cordilleras in narrow gorges very difficult to penetrate. The land from the eastern slope to the watershed which forms, according to Chilian reckoning, the boundary between Chili and the Argentine Republic, is pampa, and well adapted for cattle-breeding.

Consul Plumacher, of Maracaibo, in his last report says that the peninsula of Goajira, which forms the extreme north-western part of Venezuela, is chiefly remarkable for its entire abandonment into the hands of the Indians of the same name, who have succeeded up to the present day in preserving their absolute independence, recognizing no authority except that of their own chiefs. They are divided into different clans, or tribes, all, however, being of the same race, with similar language and customs, and the different divisions now existing are developments of individual families of the same general stock. The Venezuelan Government has contented itself with placing a military post on the frontier for the protection of the whites who, attracted by the fine grazing country, have established cattle-farms and small settlements in the neighbourhood. In spite of this precaution, the Indians at times combine in numbers of several hundreds, and make a raid into the civilized territory, retreating to their own domain with the plunder. The Indians know but little of agriculture, but engage largely in the breeding of cattle. Maize and vegetables are cultivated on a small scale, and cotton, which grows wild in some localities, gives exceptional returns when any attention is paid to its culture. The customs of the Goajiras are singular and interesting, and it is noticeable that their laws and usages have remained the same from time immemorial. One of their most striking customs is a complicated system of what is called by them "payment of tears and blood," and this is the principal cause of conflict between the clans. Among all savages revenge is a sacred duty, and as, according to Goajira ethics, an entire tribe is supposed to be responsible in the aggregate and individually for the acts of one of its own members, a trifling affair in the beginning may produce grave consequences ultimately. This is one of the reasons why it is dangerous for white men to enter the Goajira territory, as the Indians make no distinction of nationality, but consider all who are not of themselves as belonging to one great family, all the members of which are responsible for a real or fancied outrage committed by an individual, and any of whom are to be considered to a certain extent as a hostage for the conduct of the rest. By the payment of the compensation of tears and blood, any injury inflicted may be condoned, it being noticed that it is not the aggrieved individual who demands this payment, but his relatives, especially those on his mother's side, who are supposed to be of closer relationship than the family of his father. If an Indian accidentally wounds himself, breaks a limb, or meets with any similar accident, his mother's family immediately demand of him the "payment of blood," on the theory that, as his blood is also their own, he has no right to shed it without compensation. The relatives of the father also claim the payment of their tears, which is of less value. Even the friends who may have witnessed the accident are entitled to compensation for the grief into which they are plunged at seeing their companion suffer. To such an extreme is this system carried out, that should a child die in the absence of one of its parents, the one can demand from the other payment for the tears supposed to be shed over the occurrence.

Much attention has been attracted in Australia by the results achieved by Mr. Theodore Bevan in his recent exploring expedition in New Guinea (see Nature, August 11, p. 351). From a letter addressed to the *Times* by Mr. Thomas Bevan (September 27) we learn that the New South Wales Government have placed at Mr. Theodore Bevan's disposal a suitable steam-launch for further investigation, while the Queensland Government have allowed him the services of a thoroughly competent surveyor, and have offered the use of the steamship *Albatross* to tow the launch over to New Guinea waters. An influential committee has been formed at Sydney for the purpose of promoting Mr. Bevan's work. It was expected that the new Expedition would start in the course of September. Mr. Bevan will carry on his investigations between 200 and 300 miles to the north-west of Port Moresby, and at a still greater distance from the site of the explorations now being made on the Owen Stanley Range by the Victorian branch of the Royal Geographical Society.

Another advance has been made by Australia towards the fitting out of an Antarctic Expedition. The Agent-General for

Victoria, Sir Graham Berry, has addressed a letter to Sir Erasmus Ommanney, informing him that, in accordance with instructions, he has asked Her Majesty's Government if they would contribute the sum of £5000 towards an Antarctic exploring expedition, provided the Australian colonies contributed a similar sum. Sir Graham has received (September 2) a letter from the Colonial Office, stating that the subject is now under the consideration of Her Majesty's Government. Not only for the sake of promoting science, but also the good feeling and bond of union which should exist between mother-country and colonies; let us hope the answer will be favourable. Here at least is a common work, for the benefit and honour of both. If the reply is favourable, the Agent-General is instructed to communicate with Sir Allen Young, with the view of ascertaining on what terms he would take the command of such an Expedition. If there is any obstacle in the way of a money grant, why should not a suitable vessel be placed at the disposal of Australia?

LIEUTENANT VAN GELE has started for Bangala Station, under instructions from the head-quarters of the Congo Free State at Brussels, for the purpose of solving the problem as to the connexion, if any, which exists between the Wellé and the Mobangi. It is clear that Mr. Stanley does not mean to face this problem, as it was hoped he would do.

METEOROLOGICAL NOTES.

THE new Chief Signal Officer of the United States is making some sweeping changes in the meteorological service. We regret that the series of simultaneous meteorological observations taken at noon, Greenwich time, which began in 1875, at the instigation of the Vienna Meteorological Congress, is to be given up at the close of the present year, from lack of funds. This service has developed from a comparatively limited work to one of great magnitude, covering almost the whole of the northern, and part of the southern, hemisphere. For some time the observations were reduced, and published in the form of daily bulletins and maps, but the continued reduction of the amount at the disposal of the Chief Signal Officer rendered it necessary to give up this great and useful publication, and to limit the work to the issue of a monthly "Summary and Review of International Meteorological Observations," containing the monthly means of all the observations, with explanatory text and maps of the average isobars, isotherms, winds, and tracks of areas of low pressure. This valuable publication will be continued up to December 1887, to complete the data for ten consecutive years in a shape convenient for further research. General Greely states that it is further intended to publish charts of the average monthly pressure and temperature for each month of the year, based on ten years' international observations.

FOR some years Prof. Cleveland Abbe has been engaged, under the superintendence of the Chief Signal Officer of the United States, in the preparation of a general bibliography of meteorology, which has been very largely contributed to by Mr. Symons, by Dr. Hellmann of Berlin, and others; the number of books and pamphlets now catalogued amounts to about 52,000. Prof. Abbe stated, at the recent meeting of the British Association, that the work is now practically complete, and ready for publication. The General Committee of the Association fully recognized the high importance of the work, and expressed a hope that its publication by the Signal Office would speedily render it accessible to all nations.

THE last number of the *Annuaire de la Société Météorologique de France* for April and May contains two interesting papers. (1) On the distance of the arc of the aurora borealis from the ground, deduced from the variation of its angular velocity, or from its breadth, by M. Carlheim-Gyllenskjöld. The author states that the observations made during the Swedish expedition to Cape Thorsden prove that the angular velocity of the movement of the arc increases according to a regular law as the arc rises from the horizon towards the zenith, and that its more or less rapid change depends chiefly on the vertical elevation of the arc above the ground. The formula employed in the calculation is fully explained, and the result arrived at is that the mean height of the aurora borealis is from 30 to 45 miles above the earth, which agrees very closely with the results obtained at Ice Fjord by the Swedish Expedition. (2) A paper by M. G. Guilbert on the prediction of clouds and their succession throughout the day. The author finds that the first arrival of clouds, their movement over us, and their disappearance below the horizon are not left to chance, but on the contrary follow a regular order which renders prediction possible. Several examples are given of the connexion between the succession of the clouds and barometric depressions. The same journal also contains a communication by M. G. Tissandier on an extraordinary decrease of temperature observed in a captive balloon, on January 15 last, near the Champ-de-Mars. The wind was very strong from north-east, and the temperature at the ground was 24°·8 F. at 1h. 30m. p.m., while at about 330 feet it fell to 20°·3. At 1h. 50m. a second ascent of nearly 600 feet was made, where the temperature was 19°·1, showing an unusual diminution in the upper regions, especially as the weather at the time was very cloudy.

THE *Annuaire de l'Observatoire de Montsouris*, near Paris, for the year 1887, has been somewhat late in publication, apparently owing to recent changes in the management of the Observatory. M. Marié-Davy, who had charge of it since 1873, has retired, and from January 1 last the Observatory has ceased to be a Government establishment, and has been taken over by the Municipal Council of Paris. The work of the Observatory is, as before, divided under three heads: (1) Meteorology properly so called, and its application to agriculture and hygiene, together with magnetism and electricity; (2) chemical analysis of the air and of the rain-water collected at Montsouris; (3) microscopic study of the organic dust held in suspension in the air and water, each of these services being intrusted to a separate scientific man under the supervision of a special Commission. The *Annuaire* contains elaborate discussions under each of these heads; the temperature ob ervations date from 1699, and rainfall observations extend from 1689 to 1886; those prior to 1873 were taken at the Paris Observatory. The highest shade temperature last year was 91°·0 on July 21, and the lowest 18°·1 on January 24; the mean for the year was 52°·0. The thermometer screen is an open stand sheltered at top and sides, unlike those used in this country, and the year dates from October or December, being what is called the agricultural or meteorological year; this want of uniformity renders it difficult to compare the observations with others. The greatest monthly rainfall was in June, being 4·57 inches, and the least in February, 0·71 inches. The apparatus used in the different investigations is clearly illustrated.

PROF. HUGO MEYER discusses, in the *Nachrichten der k. Gesellschaft d. Wissenschaften* of Göttingen (No. 9, 1887), the thunder-storms at that place during the years 1857-80. The discussions of thunder-storms have hitherto mostly been for large areas, hence the results of a long series of observations referring to a single place have a special interest. The observations now in question were carefully made by M. Listing, and are preserved in the Physical Institute at Göttingen. They show, with regard to the yearly period, two principal maxima: the first occurring about the beginning of July, being later than at many other places—for instance at Prague and Munich, which have their second maximum about that time; the second maximum at Göttingen being about the middle of August. These observations also show two secondary maxima of thunder-storm frequency, one in the spring (April 1-10) and another in the autumn (September 28 to October 7): the first being a period of unusually rapid increase of temperature; and the second, one of a relatively slight fall of temperature; such a late autumn maximum being of rare occurrence. With regard to the daily period, two maxima occur in all months, one at the warmest part of the day, and one at midnight. In the winter half-year both the maxima occur some hours earlier than in the summer half-year, and the afternoon maximum in winter is divided into two parts. The occurrence of these double maxima, both in the yearly and daily periods, has been previously pointed out by Prof. von Bezold with regard to the thunder-storms in Bavaria. The tables show that thunder-storms at Göttingen only come from between N.W., through N., and round to S.E. in the warm daily and yearly periods, which tends to prove that they are heat thunder-storms. The cyclonic thunder-storms come almost exclusively from a westerly and south-westerly direction. The yearly march of thunder-storm frequency at Göttingen and various other places for the eight principal points of the compass is clearly shown by graphical representations, in the form of wind-roses; the mean direction of motion of all the storms at Göttingen is nearly from S. 68° W.

THE *American Meteorological Journal* for August contains an important article by Prof. W. Ferrel, on the relation of the pressure to the velocity of the wind. He points out that the formula generally used by English and American engineers and meteorologists, and which seems to have come down from a preceding century, is undoubtedly very erroneous. The formula, viz. $p = 0{\cdot}005\ v^2$, is used at all altitudes and for all temperatures, without regard to the varying densities of the air. The true theoretical formula—that is, one that would hold good in case of no viscosity of the air—is given at p. 302 of his "Recent Advances in Meteorology" (NATURE, July 14, p. 255). For an average temperature of, say, 15° C., and air of the standard pressure of 760 millimetres, this formula becomes $p = 0{\cdot}00255\ v^2$, which gives the ratio 1 : 1·96 between the two constants, from which it follows that the velocities usually deduced from pressures should be very considerably increased. The author also objects to the use of the constant 3 which is employed in the reductions of wind-velocities obtained from a Robinson's anemometer of the Kew pattern, and which is about one-fourth too large, except for low velocities, as is shown by recent experiments by Stokes and Whipple in this country, and by others abroad. The same journal also contains interesting articles on the comparison of rain-gauges, by F. Pike, and on tornadoes, by H. Allen. The latter recommends the adoption of the term "low area," or "helicone," instead of cyclone, which he thinks should be applied to West Indian storms only.

THE results of rain and river observations made in New South Wales and part of Queensland during 1886, published by the Government Astronomer for New South Wales, contain a large quantity of valuable statistics on the distribution of rain, the heights of rivers, and evaporation. The number of stations in New South Wales has increased from 641 in 1885 to 772 in 1886, yet there are many parts of the colony still unrepresented. The Report is accompanied by a map, showing very plainly by means of black spots of various sizes the increase in the amount of rainfall as we go northwards into tropical regions, until Innishowen, in Queensland, caps the list with 176 inches. The greatest average rainfall in New South Wales is only 64 inches, at Antony, just under a very high mountain range, and next to this Port Macquarie, 60 inches. The mean rainfall for the whole colony amounted to 26·04 inches in 1886, being 11 per cent. more than the average for the past twelve years.

THE Meteorological Council have issued a new edition of their "Fishery Barometer Manual." The first edition of this work was published by Admiral FitzRoy about thirty years ago, and was freely distributed by the Board of Trade to small ports and fishing-stations supplied with public barometers. This useful practice of supplying barometers to fishing-stations has been continued to the present time, nearly 170 barometers having been erected, in addition to those issued by the Royal National Lifeboat Institution. The present Manual contains much additional elementary information likely to be of use to the fishermen, and refers briefly to the recent advances in the development of weather prediction, especially by means of daily charts. Reference is also made to the telegrams now received daily from America, and to the warnings issued by the *New York Herald* Service. The Manual also contains a table showing the distribution of gales on our coasts during fifteen years, from which it appears that November is generally more stormy than December, and that the maximum storminess in March, which is especially marked in North-East England, entirely disappears in South-West Ireland and South-West England.

WE had occasion recently (NATURE, June 23, p. 184) to refer to the active steps taken by Mr. Clement L. Wragge in promoting the meteorological service in Queensland, and we have now to record a further development by the publication of daily weather charts for Australasia. The charts are drawn for 8 a.m. daily, giving isobars, wind direction and force, and the temperature and humidity of the air. Rainfall is represented by dots of various sizes, while other phenomena, such as dust-storms, fog, hail, &c., are shown by appropriate symbols, and there is also a synopsis of the existing weather. The charts will be of great utility in the study of the weather of the Australian colonies.

WE are pleased to notify the publication of a Monthly Weather Record for the Mauritius, the first issue of which, for January last, has been received. The Record, which is after the style of the United States Weather Review, but without plates, contains the results of observations taken at the Royal Alfred Observatory, together with the means and extremes of temperature at four other stations, rainfall observations taken at fifty-five stations, observations taken at Rodrigues and the Seychelles, and observations taken on board ships in the Indian Ocean. The Observatory of Mauritius stands on a plain near Port Louis, three miles from the west coast, 179 feet above the sea-level. From west-south-west through west to north there is an uninterrupted view of the sea, and from north through east to south-east the ground generally slopes to the summit of the Piton, four miles distant, and 917 feet above the sea. Between south-east and south-west there is a chain of mountains, the highest peak of which bears nearly six miles due south, and has an altitude of 2874 feet above the sea. Among the miscellaneous observations it is noted that the tail of a comet (supposed at first to be Barnard's comet) was seen on January 20, and three subsequent evenings from various parts of the island.

THE BRITISH ASSOCIATION.

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE.

On the Magnetization of Iron in Strong Fields, by Prof. Ewing, F.R.S., and Mr. W. Low. Read by Prof. Ewing.—In the experiments described iron was subjected to very intense magnetization by placing a narrow neck between two massive pole-pieces. In this way values of magnetic induction higher than those previously reached had been attained. Through the kindness of Prof. Tait the large electro-magnet of the Edinburgh University had been transferred to University College, Dundee, and by its means the induction was pushed up to the value of 38,000 C.G.S. units. There seemed, indeed, to be no limit to the value attainable, and so the neck was then turned down to about one-sixth of its previous diameter, and the induction was forced up to 45,000. By turning the neck still further and annealing it, the highest value of 45,350 was reached. An attempt was made to determine the strength of the magnetic field in the immediate neighbourhood of the neck. The quantity $\frac{B - \text{field}}{4\pi}$, where B was the magnetic induction, was found to change from 1680 in an experiment where B was 24,700, to 1420 in the case of the highest value of B attained. This would favour the idea that the intensity of magnetization has a limit. But it is difficult to be quite sure that the field in the immediate neighbourhood of the neck is the same as in the neck itself.

In order to overcome this difficulty the field in the air round the neck was explored by means of three or four coils wound one on top of the other. This will show if the field is varying fast near the iron. If not, it would be natural to assume that the field is much the same as in the iron, because in the median plane there is no surface magnetism.

On Some Points in Electrolysis and Electric Conduction, by Prof. G. Wiedemann.—Before proceeding to the discussion of electrolysis the author wished to congratulate the Association on the appointment of a Committee to investigate this important subject, and further to congratulate the Committee on having Prof. Lodge to direct their labours. He had read with great interest the able report on electrolysis which had been some time ago presented to them by Prof. Armstrong. His own communication would contain much that was old, and something that was new. There was a difficulty in the definition of an electrolyte. Some people say an electrolyte is a salt. Some say it is a binary compound. But what is a binary compound? It is something which can be decomposed into two parts. But water-free hydrochloric acid does not conduct. Nevertheless it can be decomposed into two parts. Whether the water plays a part in decomposition is still an open question, although Kohlrausch thinks he has shown that in very dilute solutions the water does take part. The resistance of an electrolyte is measured by the work done in the wandering of the ions. It had been said that his view was that the viscosity is proportional to the resistance. This is not quite correctly stated. There are to be considered (1) friction of the ions in the liquid, (2) friction of the salt in the liquid, (3) friction of the whole liquid on the walls of the vessel. (3) may be avoided, and therefore we can omit it. The main thing considered has been the friction of the ions in the liquid. Kohlrausch has lately taken very dilute solutions, and can only find the friction of the salts and not that of the ions present here, which agrees with his theory. A difficulty in this connexion is, that in very dilute solutions the impurities of the water conduct better than the salt. There may be double decompositions between those impurities and the salt. Further we know that many salts decompose in water, *e.g.* magnesium salts. Again HCl separates from a solution of ammonium chloride, and here we have acid and base separate in the solution. He did not know how to avoid this difficulty, and must content himself with pointing out the existence of it. Then another question is, What is it that is decomposed in the decomposition of salts? Is the process a simple molecular decomposition? We may ask further how hydrides are decomposed. But it is generally assumed that in the liquid it is only the salt which is decomposed. His son had sent a paper on solutions of chloride of copper. He observed that there is a change of colour in very dilute solutions; and we may be sure that in those solutions the salt has combined with the water. We cannot say whether a salt in solution is alone electrolyzed, or the salt in combination with the water. A relation between conductivity and chemical constitution had recently been obtained by Mr. Hartwig in his laboratory. He found that with rising concentration the conductivity of solutions of acids attains a maximum earlier the more carbon they contain. In regard to the friction of the salt in the liquid there is no doubt that the undecomposed salt in the liquid has a certain influence, and work must be done to produce motion of the salt in the liquid.

Prof. Quincke said that he agreed with the views of Prof. Wiedemann as to the influence of secondary decompositions in the liquid. It was difficult to distinguish between secondary and primary decompositions.

Prof. Fitzgerald, F.R.S., read a paper by Mr. F. Trouton and himself *On the Accuracy of Ohm's Law in Electrolysis.*—To avoid the difficulties due to heating of the liquid by the current the method which Chrystal and Maxwell used for solid conductors was employed, but the alternation had to be more rapid. The amount of accuracy obtained was approximately 1/2000 per cent.; and up to this Ohm's law was verified.

On the general question of electrolysis Prof. Fitzgerald said that the usual reasoning was that if the *atoms* are to be dragged asunder this requires finite E.M.F. On the whole, however, no work is done; and therefore, he contended, whatever theory may be adopted, it cannot require a finite E.M.F. to detach atoms. He believed that by this the whole Williamson-Clausius hypothesis was swept away. There were no separate atoms in the liquid. If in the case of HCl there were separate atoms of hydrogen in the liquid, surely some of them would escape from the surface of the liquid.

Prof. S. P. Thompson read a communication from Prof. von Helmholtz on *Further Researches concerning the Electrolysis of Water.*—Prof. Helmholtz has been working at the question, whether, when you electrolyze water at different pressures, it needs different electromotive forces. He found that in water which was originally free from gases the smallest E.M.F. will send a current through. He likens the difficulty which there is in getting gas to develop in an electrolytic cell originally quite free from gas to the difficulty which is experienced in getting a perfectly clean liquid to boil.

His apparatus consists of a U-tube, bent over at one end, and there blown into two bulbs, which contain the electrolyte. The electrodes are fused into the glass. One limb of the U-tube is open. From the other, which is in connexion with the bulbs containing the electrolyte, there comes off a side tube through which mercury poured into the open limb can escape, and so exhaust the space over the electrolyte to any required extent. An air-bubble is left in the large bulb above the electrolyte.

In another apparatus there was no air, and the mixed gases collected. With 1·79 volts at atmospheric pressure a balance was obtained, and the mixed gases did not increase. He finally fixes upon the superior limit of the E.M.F. with atmospheric pressure at 1·775 volt.

Experiments on the possible Electrolytic Decomposition of Alloys, by Prof. Roberts-Austen, F.R.S.—Experiments were made on gold-lead and silver-lead alloys. The results are absolutely negative. No electrolytic action whatever could be found, although cupellation would certainly have detected a variation of 1/100 per cent. in the composition.

Dr. Gladstone and Prof. Wiedemann were able to confirm the result from experiments performed by other methods in their own laboratories.

Sir W. Thomson said it was a most important discovery.

Experiments on the Speeds of Ions, by Prof. Lodge, F.R.S.—These experiments are still going on. The object is to determine directly the speeds of the ions in a liquid. The current is sent through a tube of liquid which contains some detecting substance.

At first something to give a precipitate was used, the advance of which could be timed. But this has the disadvantage of removing the substance from the tube, because the current does not affect it when it becomes solid. Now he uses fluid detectors—such as some of the aniline bodies—to detect the advance of acidity or alkalinity. Thus, for example, he may have the tube filled with solution of sodium chloride, with a trace of caustic soda, and a body which is coloured in alkaline solution, but which loses its colour when the alkalinity disappears. If now, in the course of the electrolysis, ions from the substance being electrolyzed which will unite with the Na of the caustic soda travel along the tube, they will cause the alkalinity to disappear, and the rate at which this change travels can be measured.

The composition of the liquid, however, does not remain constant, and therefore we get a broken slope of potential in the tube, because the bad-conducting alkali is turned into the good-conducting acid. This difficulty is got over by making the principal ingredient in the measuring tube the same as the product of the action for any given case. A small addition to its amount is therefore of no consequence.

The theory of Kohlrausch with regard to the speed of ions was shown to be in accordance with the results.

On Chemical Action in a Magnetic Field, by Prof. H. A. Rowland.—It had been observed by his colleague, Prof. Remsen, that if a thin plate of iron be placed between the poles of an electro-magnet and then acted on by $CuSO_4$, the copper was deposited in lines very similar to the equipotential lines. Around each pole was a clear space where the iron was not acted on at all. This part of the field is of course the part where the rate of variation of the square of the magnetic field is greatest; and it occurred to Prof. Rowland that the want of action of the sulphate of copper in this position was due to the attraction of the magnet on the iron. With the help of Mr. L. Bell he had carried out experiments on the point.

Between the poles of a powerful electro-magnet was placed a glass beaker containing the liquid whose action upon iron it was desired to test. Nitric acid generally acted very well; so did sulphate of copper, and almost any salt which would deposit metal on iron. In the liquid were immersed two pieces of iron, one of which was pointed. The greater part of each piece was covered with wax, and what was exposed to the liquid was a point in the one case and a plane surface in the other. They were connected through a galvanometer, and a current was obtained which was not reversed on reversing the direction of the current of the electro-magnet. This indicated that the point was protected from the action of the liquid.

On the Electro-deposition of Alloys, by Prof. S. P. Thompson.—In a mixture of metals which is electrolyzed, the most negative metal comes down first. Prof. Thompson made a series of experiments on solutions of zinc and copper in cyanide of potash solution of different strengths. The electromotive force was measured for each strength of the cyanide of potash solution. The curves representing the E.M.F. for copper and zinc were found to cut at a certain strength of the KCN solution. Beyond this strength copper became positive to zinc. In the ordinary brassing solution he found that it depended on the temperature whether zinc was positive to copper or copper to zinc.

On the Action of the Solvent in Electrolytic Conduction, by T. C. Fitzpatrick. This paper was communicated by Mr. W. N. Shaw.—Mr. Fitzpatrick found that although methyl alcohol has greater conductivity than water, yet a solution of calcium chloride in the former liquid is a worse conductor than an aqueous solution. He found similar results for calcium nitrate, lithium chloride, and lithium nitrate solutions. Solutions in ethylic alcohol were also used. He was much impressed with the idea that electrolysis is the electrolysis of molecular aggregates.

The next paper was by Prof. S. P. Thompson, on the *Industrial Electro-deposition of Platinum.* He exhibited specimens illustrating a new process.

The Princeton Eclipse Expedition, by Prof. C. A. Young.—The expedition had its origin in his desire to repeat observations made by him seventeen years ago, as objection had been taken, not to the accuracy of the observations themselves, but to the conclusions he had drawn from them. Prof. Libby was to do the photography. Photographs of the corona were to be taken; and they were anxious to determine whether there are true dark lines in the corona or not. The place selected for the observations lay 150 or 160 miles to the north-west of Moscow. It is needless to say that on the morning of the eclipse it rained, and hardly anything could be done. They made an attempt to determine the end of totality by the amount of light. The diminution of the light was gradual, but after totality there was a sudden burst.

Prof. L. Weber, of Breslau, described photo-metric measurements made during the eclipse at Breslau; and then read a paper on *Observations of Atmospheric Electricity.* Prof. Weber said that the increase of potential seemed to be a linear function of the height; but the presence of dust in the air disturbed this relation. The earth represents a surface of equipotential, and the other surfaces of equipotential are parallel, but come closer together above the mountain-tops.

Prof. Schuster said that, granting that the earth has a given potential at any moment, the convection-currents in the air would tend to reduce this, or to equalize the potential within the earth itself.

Prof. Everett remarked that wherever electricity is carried down by raindrops, an inequality of potential will be caused; and evaporation would also cause inequalities.

Prof. Rowland said that observations had been made during the last four years at his laboratory by the U.S. Signal Service. He did not see how the raindrops could disturb the distribution of potential much. If the earth is electrified most of the electricity would be on the outside of the atmosphere. He therefore looks for some other theory, and has given one in the *Phil. Mag.*, viz. that the earth would naturally be uniformly electrified if it were not for currents of air in the upper atmosphere, which will carry the electricity of the atmosphere towards the poles, making auroras there. At the equator, therefore, a space must be left which has to be filled up with electricity, and this takes place by thunderstorms. Accordingly there is a circulation of electricity. In this connexion it is to be remembered that thunderstorms are most common about the equator.

The Hygrometry of Ben Nevis, by Mr. H. N. Dickson.—This paper gives an account of observations which were undertaken for the purpose of testing the applicability at high-level stations of existing tables and formulæ for calculating the dew-point and humidity from the readings of wet- and dry-bulb thermometers. The construction of the direct hygrometer used, that of Prof. Chrystal, is described, and the action of the wet and dry bulbs under different meteorological conditions is examined in considerable detail; the results showing that for investigations of this kind a great range of humidity is necessary, the indications of the wet and dry bulbs being very uncertain when the difference between them is small.

The reduction of the observations is performed, in the first place, by a graphic method, from which the following expression is reduced: $f' - f'' = (t - t')k$, f' being the vapour-pressure at the temperature t' of the wet bulb, f'' that at the temperature of the dew-point, and t the air temperature. The truth of the above equation being assumed, the values of the quantity k are next found by direct calculation from the observations. A sudden large change takes place in its value at the freezing-point, and a similar, though much smaller, discontinuity is shown to occur when the wet bulb stands between 39° and 40°.

The Different Varieties of Thunderstorms, and a Scheme for their Systematic Observation in Great Britain, by the Hon. R. Abercromby.—The writer said that there were three well-defined types of thunderstorms in this country: (1) squall thunderstorms, *i.e.* simply a squall associated with thunder and lightning; (2) a very common form which occurs in secondary cyclones: the nature of this class needed investigation; (3) far the most curious class was that which might be called line-thunderstorms, because their shape was a long narrow belt sometimes 200 or 300 miles long and only 4 or 5 broad. They move broadside on, and are usually preceded by a squall of extreme violence. He explained a scheme for the future systematic study of thunderstorms, and invited the co-operation of volunteer observers.

Sir W. Thomson said that the natural history of thunderstorms was less known than any other part of meteorology, and that Mr. Abercromby's scheme would be likely to give much information on the subject.

On the Magnetization of Hadfield's Manganese Steel in Strong Fields, by Prof. J. A. Ewing, F.R.S., and William Low.—Messrs. Hadfield, of Sheffield, manufacture a steel containing about 12 per cent. of manganese and 0·8 per cent. of carbon, which possesses many remarkable qualities. Prominent amongst these, as the experiments of Hopkinson, Bottomley, and Barrett have shown, is a singular absence of magnetic susceptibility. Hopkinson, by applying a magnetic force, $\mathfrak{H}$, of 244 C.G.S. units to a specimen of this metal produced a magnetic induction, $\mathfrak{B}$, of only 310 C.G.S. units; in other words, the permeability μ was 1·27, and the intensity of magnetization $\mathfrak{I}$ was a little over 5 units.

The experiments made it clear that even under magnetic forces extending to 10,000 C.G.S. units the resistance which this manganese steel offers to being magnetized suffers no break-down in any way comparable to that which occurs in wrought iron, cast iron, or ordinary steel at a very early stage in the magnetizing process. On the contrary, the permeability is approximately constant under large and small forces.

The conclusion has some practical interest. It has been suggested that this steel should be used for the bed-plates of dynamos and in other situations where a metal is wanted that will not divert the lines of induction from neighbouring spaces. In such cases the magnetic forces to which manganese steel would be subjected would certainly lie below the limit to which the force has been raised in these experiments. We may therefore conclude that in these uses of the material it may be counted upon to exhibit a magnetic permeability only fractionally greater than that of copper, or brass, or air.

On the Influence of a Plane of Transverse Section on the Magnetic Permeability of an Iron Bar, by Prof. J. A. Ewing, F.R.S., and William Low.—It has been remarked by Prof. J. J. Thomson and Mr. H. F. Newall that when an iron bar is cut across and the cut ends are brought into contact, the magnetic permeability is notably reduced (Cambridge Phil. Soc. Proc., February 1887). The attention of the authors was directed to the matter by finding the same phenomenon present itself in experiments on the magnetization by the "isthmus" method; and they proceeded to examine the effect by an application of the method Hopkinson has used to measure magnetic permeability ("Magnetization of Iron," Phil. Trans., Part 2, 1885). A round bar, nearly half a square centimetre in section, and 13 centimetres long, had its ends united by a massive wrought-iron yoke to reduce it to a condition approximating to endlessness, and its magnetization by various magnetic forces was examined, both when free from stress and when compressed by a load of 226 kilos. per square centimetre. It was then cut in the lathe and the halves placed in contact, and the magnetization again examined with and without load. It was next cut into four parts, and finally into eight parts, and magnetized in each case. Every new plane of section caused a notable loss of permeability. The following are the maximum values of the permeability in each case:—

Solid bar	1220
Bar cut in two ...	980
Bar cut in four ...	640
Bar cut in eight ...	400

Next another bar was tested, first when solid, next with one cut finished in the lathe, and finally with the cut surfaces faced true by scraping and comparing them with a Whitworth plane. So long as the bar was not compressed its magnetic permeability was nearly the same, whether the ends were left roughly finished or were faced true. But when load was applied the effect of facing the ends was remarkable: the faced bar then behaved as a solid bar would, while the bar with rough cut ends still showed a decided defect of permeability as compared with a solid bar.

This made it seem highly probable that the whole effect was due to a film of air between the cut faces. Applying Hopkinson's method to calculate the thickness this film would need to have in order to account for the observed increase of magnetic resistance, the authors find its thickness is only about 1/35 of a millimetre when the magnetic force is 10 C.G.S. units, and diminishes to about 1/70 of a millimetre when the force is 50 C.G.S. In the case of the bar cut into four or eight parts, each cut has an effect equivalent to the introduction of a film of this thickness. The authors conclude that in all probability the whole phenomenon is due to the surfaces being separated by these short distances.

On the Magnetic Properties of Gases, by Prof. Quincke, Ph.D.—A few years ago he invented what he called a magnetic manometer. It consists of a bent tube, of which one limb is much wider than the other. In the wide limb is the gas to be experimented on. The narrow limb and the connecting horizontal piece contain liquid, and the difference of level of the liquid in the narrow limb produced by the magnetic field is what is measured.

The magnetic pressure per unit of area is given by the formula—

$$p = \frac{R}{8\pi} H_1^2.$$

If h be the difference of level of the liquid in the two limbs, *i.e.* the hydrostatic pressure, we have—

$$h\sigma = \frac{R - R_1}{8\pi} H_1^2.$$

The smallest diamagnetic constant for the gases experimented on was found to be that of hydrogen. Oxygen had the highest. He compares his results with Faraday's, and finds that they agree substantially, the differences being probably due to impurities.

Final Value of the B.A. Unit of Electrical Resistance as determined by the American Committee, by Prof. H. A. Rowland.—His determination in 1876 gave 1 B.A. unit = ·9878 ohm. For his present determination the apparatus was on a very large scale. He employed both the Kirchoff and the Lorenz method. By the former method he got a final value of ·98646 ± 40, by the latter a value of ·9864 ± 18; so that the latter method has a probable error of less than a half that of the former. His value for the resistance of 100 cubic centimetres of mercury came out ·95349 B.A. units.

Lord Rayleigh said that the results showed that the absolute determination of the B.A. unit by various experiments agreed much better than the comparison with the mercury standard. This was exactly the opposite of what he would have expected. Prof. Rowland had suggested that one cause of the difference between their determinations of the mercury standard might be that in the American experiments the tubes had been mechanically wiped, so that there was no chance of dust remaining in them. He hardly thought, however, that this was likely. The want of uniformity in the diameter of the tube might possibly have an effect.

On Induction between Wires and Wires, by W. H. Preece, F.R.S.—A continuation of a subject brought before the Association last year, when it was shown that electro-magnetic disturbances extended to distances much greater than was imagined, and that effects were observed across many miles of country. Experiments were made on the banks of the Severn and Mersey, on the Portcawl Sands of South Wales, in the fields in the neighbourhood of Cardiff, on the roads and railways of Oxfordshire, Worcestershire, and Shropshire, in the air and under water, in the corridor of the General Post Office in London; and the law was formulated that the distance depended directly on the strength of the currents inducing the disturbance and on the length of the wires opposed to each other, and inversely on the square of the distance separating them, and on the electrical resistance of the disturbed wire.

The influence of 1 mile of wire carrying 1 ampere of current can apparently extend to a distance of 1·9 mile. The law is given by the following formula:—

$$C_2 = M\frac{C_1 l}{d^2 r_2},$$

where C_1 is the primary current, C_2 the secondary, l the length of the wires opposed to each other, d the distance separating them, r_2 the resistance of the secondary circuit. When these quantities are represented in C.G.S. units, M equals ·005. The current induced by 1 mile, of 1 ampere at 1 mile distant is $1·3 \times 10^{-13}$ ampere. A current is still perceptible at 1·9 mile distant; hence we can calculate that a Bell telephone requires six ten-thousand-millionths of a milliampere, or, in figures, ·0000000006 milliampere, to be audible.

One curious result of these inquiries is that the disturbances are transmitted equally well through water and the earth as through air, and hence our cables are disturbed as well as our land wires. Communication with coal-pits is possible, though nothing but the earth intervenes.

On the Effect of Continental Lands in altering the Level of the adjoining Oceans, by Prof. Edward Hull, F.R.S., Director of the Geological Survey of Ireland.—The effect of the attraction of continental lands upon the oceanic waters adjoining seems to have been very much overlooked by British physical geographers. That some slight effect arises in the direction of elevating the surface of the ocean in proximity to the coast is generally admitted, but the amount of rise is considered to be small, perhaps insignificant. The prevalence of these views was attributed by the author to the widespread influence of Lyell's hypothesis of the uniformity of the ocean-surface all over the globe.

The author proceeded to discuss the effect of continental lands, showing that this was in the first instance divisible under two principal heads: The effect (1) of the unsubmerged, and (2) of the submerged masses. In the former case, where the mass rose above the surface, one component of the attraction acted in a more or less vertical direction; in the second case, all in a lateral direction; but both had the effect of elevating the surface of the ocean. The horizontal distance to which the vertical effect extended owing to the curvature of the earth's surface was then considered: and it was shown that, where continental lands rise from a deep ocean, the effect of the lateral attraction far exceeds that of the vertical attraction of the unsubmerged mass. Prof. Stokes has furnished the author with a hypothetical case, in which the elevation of the ocean was estimated to reach 400 feet above the mean geodetic surface of the earth.

For the purposes of illustration three cases were selected, viz.:—

(1) The table-land of Mexico,	between lats.	18° and 26° N.
(2) The table-land of Bolivia,	,,	19° and 26° S.
(3) The Andes of Chili,	,,	26° and 35° S.

The mean elevations, distances from the ocean, and extent having been determined, and the mean density of the crust being taken at 2·6 for emergent, and 1·6 for unsubmerged land, the results of the attraction of the mountain masses in each case were as follows:—

(1) Mexico, 780 feet; (2) Bolivia, 2160 feet; (3) Chili, 1580 feet.

The total calculated rise of the ocean-waters at a distance of 900 miles from the coast in lat. 10° S. would amount to 2568 feet.

The above results, which are probably rather under than over estimates, fall considerably short of those to be drawn from Suess and Fischer's formula, but are probably much in excess of the views held by British physical geographers generally; and the conclusion was drawn that if the same processes of reasoning and calculation were applied to all parts of the world, it would be found that the ocean waters were piled up to a greater or less extent all along our continental coasts, producing very important alterations in the terrestrial configuration as compared with an imaginary ellipsoidal, or geodetic, surface, to which all these changes of level must necessarily be referred.

On a Standard Lamp, by Prof. A. A. Vernon Harcourt, F.R.S.—At one of the meetings of this Section last year a lamp devised by the author for producing a constant amount of light was shown and described by Mr. W. S. Rawson. The lamp now exhibited serves the same purpose, but is simpler in principle, more easily adjusted, and less affected by draughts. It consists of a glass reservoir with tubulure and stopper of the form and size of a large spirit-lamp, mounted on a metal stand provided with levelling screws. The wick can be turned up and down in the normal manner within a long tube attached to the body of the lamp. Round this tube is a wider tube 100 × 25 mm., and the two being joined together above and below by flat plates constitute the burner of the lamp. When the burner becomes warm by conduction of heat from the flame of the lamp, the pentane in the wick volatilizes and burns at a considerable distance above the point to which the wick is turned down. Thus the size, or texture, or quality of the wick does not affect the flame. Around the burner and the lower part of the flame is another cylinder open at both ends and contracted above the burner to a tube 21 mm. in diameter. A similar tube forms the lower part of an upper chimney, which is enlarged above to a diameter of 25 mm. The upper part of the flame is concealed by this chimney, excepting where a narrow slit, 10×3 mm., on each side shows the tip of the flame and enables its height to be regulated. Through the interval between the two chimneys the flame shines, and the light which it gives is the same whenever the tip of the flame is visible through the slit, whether towards the lower or the upper end. The two chimneys are attached together by two curved metal bands sufficiently removed from the flame on either side not to affect it. The attachment of the bands to the lower chimney are adjustable, so that the opening through which the central parts of the flame are seen may be made larger or smaller. By means of small cylindrical blocks, whose thickness is accurately gauged, the width of the opening may be set either to that at which the light emitted is one candle, or, if a greater or smaller light is desired, a candle and a half or half a candle. The liquid with which the lamp is fed is pentane, obtained in a manner already described from American petroleum.

Mr. W. N. Shaw read a paper by Mr. J. T. Bottomley on *Expansion by Heat of Wires under Pulling Stress.* The wires were two fine copper wires. One of them carried about half its breaking weight and the other about a tenth of its breaking weight. The wires were suspended in a tube, a scale being attached to one, and a pointer moving over the scale to the other. Thermometers were inserted into the tube at various points, and the wires were heated by passing steam into the tube. It was found that the more heavily weighted wire extended much more than the lightly-weighted one. An amount of permanent elongation remained, but more in the heavily-strained wire. Each time the heating was done there was more and more permanent elongation, and ultimately one of the wires was broken under less than its breaking load in the normal state. Further experiments were made with wires which had been hardened, and the final result is that the coefficient of expansibility for heat of copper wire strained by a certain weight is greater than that of similar wire less heavily weighted.

Experiments on Electrolysis and Electrolytic Polarization, by W. W. Haldane Gee, Henry Holden, and Charles H. Lees.—This is a preliminary notice of experiments that are in progress in the Owens College Physical Laboratory. The experiments fall under four heads: (*A*) electrolysis under pressure; (*B*) time-rate of fall of polarization in closed circuit; (*C*) irreciprocal conduction; (*D*) the production of an oily fluid in electrolysis with palladium electrodes.

A. Numerous experiments have been made in order to determine the variation of the resistance of polarization of a sealed voltameter in which dilute sulphuric acid was electrolyzed between platinum wire electrodes, it being thus subjected to the pressure (up to 200 atmospheres) of the evolved gases. It was found that the resistance markedly decreased, and the polarization decreased slightly. These changes may, however, it is thought, be due to change of temperature, the influence of which would appear from later experiments not to have been fully eliminated. In two cases no change whatever was perceived: (1) when two platinum *plates* were used as electrodes, and (2) when two voltameters were connected together forming a sealed vessel, one voltameter being used to increase the pressure, while observations were made on the other. As it has not been possible to obtain glass tubes sufficiently strong for the high pressures desired, an apparatus of gun-metal has been constructed. This apparatus, which is fitted with a Bourdon's gauge recording to six tons on the square inch, may also be arranged for pressure experiments in general by attaching to it, by means of a strong metal tube, a suitable receiver. In two of the experiments, when the pressure had reached between 200 and 300 atmospheres, the evolved oxygen and hydrogen gases combined with explosion, although precautions had been used to prevent the gases from coming into contact with the platinum, except in the liquid.

B. The object of this research was to try to learn the parts played by the various portions of the evolved gases: (1) that occluded by the electrodes; (2) that deposited on them; and (3) that contained in the liquid in influencing polarization. The method employed was to vary the conditions under control, *e.g.* time of changing, density of current, &c., and to observe the time-rate of the fall of the polarization thus produced in closed circuit. It was found to be very difficult to apply this method, because though the conditions under control were kept as constant as possible, yet the time-rates of fall in two successive observations were often different. This was thought to be due to the insufficient cleaning of the electrodes between each experiment, and various methods were tried to remedy it, with the general result that the more perfect the cleaning became the more regular did the curves giving the time-rate of fall of the polarization become, but still the inconsistencies were not wholly removed. Heating the electrodes by the electrical current seemed preferable to the other methods of heating.

C. Whilst electrolyzing strong sulphuric acid between platinum electrodes, it was noticed that when the current density at the anode had exceeded a certain value decomposition apparently ceased. The value of the anode current-density necessary to produce this phenomenon is increased by diminishing the concentration or increasing the temperature of the acid (thus diminishing the viscosity), and is diminished by cleaning the electrodes. It was found that this great diminution of the current was not caused by the formation of an opposing E.M.F., but by a sudden increase of from 500 to 50,000 ohms in the resistance of the circuit. That the insulating condition occurs at the anode is shown by successively replacing the kathode and the anode by clean plates; in the first case the stoppage of the current persists, in the second case the current is readily conducted. The cause seems to be a sheath of oxygen bubbles which firmly adhere to the anode when the insulating condition is formed. The film is removed by momentarily breaking the circuit, or short-circuiting the voltameter, or reversing the current, or by replacing the anode by a clean plate.

D. During the electrolysis of various liquids between palladium electrodes it has been observed that a dense-looking liquid streams from *one* of the electrodes (the anode in dilute sulphuric acid, the kathode in caustic soda) *after a reversal of the current.* The liquid seems to be a compound of oxygen and hydrogen, presumably hydroxyl.

On the Vortex-Theory of the Luminiferous Ether, by Prof. Sir W. Thomson, F.R.S.—"In endeavouring to investigate turbulent motion of water for my communication on that subject to this Section, I have found a solution (many times tried for within the last twenty years) of the problem—to construct, by giving vortex-motion to an incompressible viscid fluid, a medium which shall transmit waves of laminar motion as the luminiferous ether transmits waves of light. Let *xav*, *xzav*, *xyzav* denote space-averages, linear, surface, and solid, through infinitely great spaces." After defining and illustrating this method of averages by examples, and remarking in passing that a general property of it is that

$$xav \frac{dQ}{dx} = 0,$$

where Q is any quantity which is finite for infinitely great values of x, he proceeded thus:—

Suppose now the motion to be homogeneously distributed through all space. This implies that the centres of inertia of all great volumes of the fluid have equal parallel motions, if any motions at all. Conveniently, therefore, we take our reference-lines, OX, OY, OZ, as fixed relatively to the centres of inertia of three (and therefore of all) centres of inertia of large volumes; in other words, we assume no translatory motion of the fluid as a whole. This makes zero of every large average of u, and of v, and of w; u, v, and w being the velocity-components; and we may write as the general expression for nullity of translational movements in large volumes—

$$0 = \text{ave. } u = \text{ave. } v = \text{ave. } w,$$

where ave. denotes the average through any great length of straight or curved line, or area of plane or curved surface, or through any great volume of space. In terms of this generalized notation of averages, homogeneousness implies—

$$\text{ave. } u^2 = U^2, \text{ ave. } v^2 = V^2, \text{ ave. } w^2 = W^2,$$
$$\text{ave. } vw = A^2, \text{ ave. } wu = B^2, \text{ ave. } uv = C^2,$$

where U, V, W, A, B, C are six velocities independent of the positions of the spaces in which the averages are taken. These equations are, however, infinitely short of implying, though implied by, homogeneousness.

Suppose now the distribution of motion to be isotropic. This implies, but is infinitely more than is implied by, the following equations in terms of the above notation, with further notation, R, to denote what we shall call *the average velocity* of the turbulent motion—

$$U^2 = V^2 = W^2 \tfrac{1}{3}R^2,$$
$$0 = A = B = C.$$

Large questions now present themselves as to transformations which a distribution of turbulent motion would experience in an infinite liquid left to itself with any distribution given to it initially. If the initial distribution be homogeneous through all large volumes of space, except a certain large finite space, S, through which there is initially either no motion or turbulent motion, homogeneous or not, but not homogeneous with the motion through the surrounding space, will the fluid which at any time is within S acquire more and more nearly as time advances the same homogeneous distribution of motion as that of the surrounding space, till ultimately the motion is homogeneous throughout? Probably, I think I may say certainly, yes—at all events for a large class of cases.

But can it be that this equalization comes to pass through smaller and smaller spaces as time advances? In other words, will any given distribution, homogeneous on a large enough scale, become more and more *fine-grained* as time advances? Probably *yes* for some initial distributions; probably *no* for others. Probably *yes*, for vortex-motion given continuously through all of one large portion of the fluid while all the rest is irrotational. Probably *no* for the initial motion given in the shape of equal and similar Helmholtz rings, of proportions suitable for stability, and each of overall diameter considerably smaller than the average distance from nearest neighbour. Probably also *no*, though the rings be of very different volumes and vorticities. But probably *yes* if the diameters of the rings or of many of them, be not small in comparison with distances from neighbours, or if the individual rings, each an endless slender filament, be entangled or nearly entangled among one another.

Again a question: If the initial distribution be *homogeneous and æolotropic*, will it become more and more isotropic as time advances, and *ultimately quite isotropic?* Probably *yes* for any random initial distribution, whether of continuous rotationally-moving fluid or of separate finite vortex-rings. Possibly *no* for some symmetrical initial distribution of vortex-rings, conceivably stable; though it does not seem probable that there is any such stability.

If the initial distribution be homogeneous and isotropic (and therefore utterly *random* in respect to direction) will it remain so? Certainly *yes*.

We shall now suppose the initial motion to consist of a laminar motion $[f(y), 0, 0]$ superimposed on a homogeneous and isotropic distribution $(\mathfrak{u}_0, v_0, w_0)$; so that we have—

$$\text{when } t = 0,\ u = f(y) + \mathfrak{u}_0,\ v = v_0,\ w = w_0;$$

and we shall endeavour to find such a function, $f(y, t)$, that at any time, t, the velocity-components shall be—

$$f(y, t) + \mathfrak{u},\ v,\ w,$$

where $\mathfrak{u}$, v, w are quantities of each of which every large enough average is zero.

With this assumption the equations of motion yield the following—

$$\frac{df(y, t)}{dt} = -\ xzav\ \frac{d(\mathfrak{u}v)}{dy}.$$

It is to be remarked that this result involves no isotropy, no homogeneousness in respect to y; and only homogeneousness of *régime* with respect to y and z, with no translational motion.

The translational component of the motion is wholly represented by $f(y, t)$, and, so far as our establishment of the above equation is concerned, may be of any magnitude, great or small relatively to velocity-components of the turbulent motion. It is a fundamental formula in the theory of the turbulent motion of water between two planes; and I had found it in endeavouring to treat mathematically my brother Prof. James Thomson's theory of the "Flow of Water in Uniform *Régime* in Rivers and other Open Channels" (Proceedings of the Royal Society, August 15, 1878). In endeavouring to advance a step towards the law of distribution of the laminar motion at different depths, I was surprised to discover the law of propagation as of distortional waves in an elastic solid, which constitutes the conclusion of my present communication—

$$\frac{d}{dt}\ xzav\ (\mathfrak{u}v = -\ \tfrac{2}{9}R^2\frac{df(y, t)}{dy}.$$

Eliminating the first member from this equation, by the former, we find—

$$\frac{d^2f}{dt^2} = \tfrac{2}{9}R^2\frac{d^2f}{dt^2}.$$

Thus we have the very remarkable result that laminar disturbance is propagated according to the well-known mode of waves of distortion in a homogeneous elastic solid; and that the velocity of propagation is $\frac{\sqrt{2}}{3}$ R, or about ·47 of the average velocity of the turbulent motion of the fluid. This might seem to go far towards giving probability to the vortex-theory of the luminiferous ether.

But a difficulty remains unsolved: a possible rearrangement of vortices within each wave, giving rise to dissipation of the wave-energy.

The mathematical investigation appears in full in the October number of the *Philosophical Magazine*, with some slight farther considerations regarding this virtual viscosity, and the question of what, if any, distribution of vortices can either have no tendency to the vitiating rearrangement, or can, with the requisite fine-grainedness, be slow enough in the vitiating rearrangement to allow the propagation of waves of light to go on through a hundred million million miles of space, or a million times the earth's distance from the sun.

The Committee of the Section reported that at a meeting of the Committee it had been resolved, on the motion of Prof. Gustav Wiedemann, of Leipzig, seconded by Sir William Thomson:—"That this Committee of the Mathematical and Physical Science Section of the British Association hereby convey to Dr. Joule their sense of the great loss sustained by the Section in consequence of his inability to take part in this meeting of the British Association in his native city, and express their sincere regret at the cause of this loss, and their hearty sympathy with him in his illness. The Committee take this opportunity of recording their appreciation of the splendid work of this most painstaking and conscientious seeker after truth, who, with his discoveries, has led the way in the greatest advance in knowledge made in this age, and, by his life, has conferred on mankind a precious example for their admiration and imitation."

SCIENTIFIC SERIALS.

American Journal of Science, August.—History of the changes in the Mount Loa craters (continued), by James D. Dana. In this paper the history of Kilauea is continued from January 1840 to the end of 1886, during which period sufficient facts were accumulated for a widened and apparently final explanation of the method of filling the pit. The eruptions of 1849, 1855, 1868, and 1886 are fully described, and the whole subject is illustrated with maps of the burning mountain at various dates during the period under consideration.—On some phenomena of binocular vision (continued), by Joseph Le Conte. In this paper, the twelfth of the series, the author deals with certain peculiarities of the phantom images formed by binocular combination of regular figures. The phenomena here described, none of which have hitherto been satisfactorily accounted for, are all explained by the law of corresponding points, justly regarded as the most fundamental law of binocular vision.—Chemical integration, by T. Sterry Hunt. In this paper the author deals more fully with several points connected with chemical metamorphosis, which were more briefly noticed in his recently published work, entitled "A New Basis for Chemistry."—Studies in the mica group, by F. W. Clarke. In this paper the author deals with specimens of muscovite from Alexander County, North Carolina; of lepidomelane from Baltimore and Litchfield, Maine; of iron biotite from Auburn, Maine; and of iron mica from near Pike's Peak.

SOCIETIES AND ACADEMIES.

London.

Institution of Mechanical Engineers, September 30.—Mr. E. H. Carbutt, President, in the chair.—A supplementary paper by Major Thomas English, R.E., on the initial condensation in a steam cylinder, was read and discussed in connexion with the paper by the same author on the distribution of heat in a stationary steam-engine, read at the spring meeting on May 17, an abstract of which has already appeared in Nature (vol. xxxvi. p. 115). The supplementary experiments were carried out in a portable engine of ordinary type, the cylinder of which was jacketed on the cylindrical portion but not at the ends. The steam was admitted directly from the boiler into the steam chest, and the quantity required for each experiment being small compared with the capacity of the boiler, no question of priming or condensation before admission can arise. The connecting-rod was disconnected, and the piston was rigidly blocked at the end of the stroke furthest from the crank, the interior of the cylinder surrounding the piston-rod being entirely filled up with wood and iron packing. The steam passage between the valve seat and the end of the cylinder next the crank was also solidly filled up; and the port itself was closed by a brass plate scraped down to the level of the valve seat. The port admitting steam to the end of the cylinder furthest from the crank was left open; and the crank shaft, eccentric, and valve were driven by another engine. The steam pressure in the boiler was maintained at a uniform amount, and the regulator was kept open during a trial. The steam was measured by connecting the exhaust port with a surface condenser and collecting the resulting water. The results of the experiments appeared to indicate that the net initial condensation, or excess of condensation, over re-evaporation by the clearance surface varies directly as the initial density, and inversely as the square root of the number of revolutions per unit of time. The paper was discussed, and was followed by one on irrigating machinery on the Pacific coast, by Mr. John Richards, which dealt very fully with the forms of pumps required for the various services to be performed. The discussion of this paper was adjourned.

PARIS.

Academy of Sciences, September 26.—M. Hervé Mangon in the chair.—On the recent waterspout in Lake Geneva, by M. H. Faye. In reply to M. Ch. Dufour's letter stating that several persons had noticed an *ascending* gyratory movement in the waterspout that swept over Lake Geneva on August 19, the author points out that, although the movement is really descending, as he holds against most meteorologists, there is nothing remarkable in this apparent contradiction, which is due to a purely optical illusion on the part of the observers. In the same way the spirals of a vice or screw, placed vertically to a horizontal base, when turned in the reverse direction, seem to the spectator to ascend along the line of the main axis, presenting the appearance of continually retiring from the base upwards, and burying itself in the handle or top cross-piece. The cause of the illusion is simple enough. Each anterior semi-spiral is successively replaced, as the screw revolves, by the posterior half, which, being at a higher level, the visible half-spirals, taken separately and together, seem to ascend. So with waterspouts, which, as already repeatedly explained, never ascend, but always descend, being the result of forces having their existence in the upper atmospheric regions.—On the measurement of the forces brought into play in the flight of a bird, by M. Marey. Anatomy shows that nearly all the muscles acting on the wing serve to lower it, while the kinematic data drawn from photo-chronography show that during this lowering of the wing the mass of the bird is upheld against gravity and propelled forward against the resistance of the air, the result being flight. The author here studies these two elements of the motor power separately, whence may ultimately be deduced the sum total of the motor power.—Remarks accompanying the presentation of vol. xiii. of the "Mémorial du Dépôt de la Guerre," by General Perrier. This volume is occupied exclusively with the operations connected with the extension of the geodetic and astronomic lines from Spain to Algeria.—Observations of Brooks's comet (August 24), made at the Observatory of Algiers with the 0·50m. telescope, by MM. Trépied, Rambaud, and Sy. The observations extend over the period from September 10 to 16, and give the positions of six comparison stars of the eighth and ninth magnitudes.—Observations of the same comet at the Observatory of Lyons with the 0·18m. Brunner equatorial, by M. Le Cadet. The observations cover the period from September 13 to September 21.—Positions of Barnard's comet (of May 12, 1887) and of Palisa's new asteroid (September 21, 1887), measured at the Observatory of Besançon, by M. Gruey. The observations of the comet run from June 13 to July 23. Those of the asteroid were taken with the 8-inch equatorial on September 23.—On the relative distances of the planets in relation to the sun, and on the distances of the periodical comets, by M. Delauney. The planetary distances being represented by the formula $D = 86^{1\cdot0669^n}$, where n receives the successive values 1, 2, 3, 4, ... , the unity of distance is the semi-diameter of the sun, and if this unity be changed and the distance be taken, for instance, of the earth from the sun, the formula becomes $D = 0\cdot0032680 \times 86^{1\cdot0669^n}$. The calculation shows that with this same unity the mean distances of the six known periodical comets from the centre of the sun may be one presented by the analogous formula $D = 1\cdot8940 \times 1\cdot1511^{2^n}$. Further considerations show that there exists a gap in the series corresponding to $n = 1$, and that seven comets may be regarded as forming a single group analogous to the minor planets of the solar system. The distances increase so rapidly with n that for $n = 6$ we get 15,455, corresponding to a periodicity of nearly 2,000,000 years. Other considerations lead to the inference that the periodical comets appear to be produced by the cosmic matter of the zodiacal light.—Researches on the spheroidal state, by M. E. Gossart. The author here seeks to determine by calculation and experiment the meridional semi-section of any liquid drop whatsoever in a state of calefaction on a horizontal plaque. It is shown that there exists a characteristic form of the spheroidal state, which may easily be represented graphically according to a given scale. The measurements of the various elements of these curves may furnish useful information on the capillary constant.—On the distillation of citric acid with glycerine, MM. Ph. de Clermont and P. Chautard. The product of the process here described presents absolutely the same properties as the pyruvine obtained by distilling a mixture of tartaric acid with glycerine, although it seems difficult to explain how the same substance should result from the distillation, in the presence of glycerine, of an acid such as citric acid, which differs so greatly from tartaric acid.—On the development and structure of young Orobanches, by M. Maurice Hovelacque. Since M. Caspary's observations on the germination of the Orobanches (*O. cruenta*, *O. ramosa*, *O. minor*, *O. Hederæ*), dating from 1854, nothing was published on the subject till its study was resumed by Koch in 1883, the results being published in a comprehensive memoir recently issued by him. In the present communication M. Hovelacque indicates several important points where his own observations differ considerably from the conclusions of the learned German botanist.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Exercises in Quantitative Chemical Analysis, including Gas Analysis: W. Dittmar (Hodge).—Weather Charts and Storm Warnings, 3rd edition: R. H. Scott (Longmans).—Proceedings and Transactions of the Royal Society of Canada for 1886, vol. iv. (Dawson, Montreal).—Report of the Voyage of H.M.S. *Challenger*, vol. xxi. 2 Parts, Zoology.—An Elementary Treatise on Kinematics and Dynamics: J. G. Macgregor (Macmillan).—Key to Todhunter's Conic Sections: Edited by C. W. Bourne (Macmillan).—Handbuch der Palæontologie, 1 Abth. Palæozoologie, 3 Band, 1 Liefg. (Williams and Norgate).

CONTENTS.

THURSDAY, OCTOBER 13, 1887.

THE SCENERY OF SCOTLAND.

The Scenery of Scotland viewed in Connexion with its Physical Geology. By Archibald Geikie, LL.D., F.R.S. (London: Macmillan, 1887.)

THAT truth is great, and that it will in the end carry the day, we are assured by the most venerable and most hackneyed of aphorisms. We profess implicit belief in the doctrine, but, as we survey the history of the growth of opinion, our faith is apt to be rudely shaken when we note countless instances of the marvellous persistence and vitality of notions, however erroneous they may be, when they have once become firmly rooted in the minds of men. It is at such times hard to help doubting whether the vaunted power of knowledge is always competent to sweep away the dead weight of prejudice and obstinacy which cumbers the approaches to the abode of truth. The infancy of every science furnishes illustrations of the tenacity with which even men of science cling to preconceived beliefs, none perhaps more striking than that supplied by the branch of geology which the volume before us is intended to illustrate.

Well-nigh a century has slipped away since Hutton enunciated the doctrine that the surface features of the land are in the main due to the carving and sculpturing action of denudation, and gave reasons for his belief which are now held to be unanswerable by nearly every geologist. But his proved to be a veritable voice crying in the wilderness. Scrope reiterated the truth and enforced it by fresh examples, notably those furnished by Auvergne; but his testimony availed not to charm ears still deaf or unwilling to be convinced. Even Ramsay, afterwards a most strenuous champion of the doctrine, failed to see the whole truth when he wrote his classical memoir on "The Denudation of the South-West of England." Jukes struck the right note in his memorable paper on "The River-Valleys of the South of Ireland"; Ramsay, the year following, gave precision and definite shape to the theory, which had so long a birth-throe, in his "Physical Geology and Geography of Great Britain"; Foster and Topley showed how this theory furnished a rational explanation of the growth of the puzzling physical geography of the Weald of Kent and Sussex; and Whitaker summed up the evidence in its favour in a paper singularly exhaustive in its facts and lucid in its arguments. Nothing perhaps shows more forcibly the difficulty of persuading mankind even to listen to views which seem new to them, than the fact that Whitaker's singularly temperate and unaggressive memoir was refused a place in their Quarterly Journal by the Council of the Geological Society of London. The writer may perhaps claim to have added his mite when, following in the steps of these pioneers, he pointed out how the striking escarpments and dip-slopes of the Millstone Grit moors in Derbyshire and Yorkshire have arisen.

It was when the controversy was at its height that Dr. A. Geikie furnished a weighty and memorable contribution to it in his work on "The Scenery of Scotland viewed in Connexion with its Physical Geology." He then gave no uncertain sound as far as his own convictions went, but he admitted in his preface that the views to which he had been led ran directly counter to what were at that time the prevailing impressions on the subject of the book, and that he was prepared to find them disputed or perhaps thrown aside as mere dreaming. Now, after a lapse of twenty-two years, during which many a young geologist has been hungering for access to the book long out of print, a second edition appears, and the author is able to state that these very views are accepted as part of the general stock of geological knowledge. How largely this result is due to his own steady and powerful advocacy all geologists are aware; but he gracefully reminds us that we also owe much to the labours of those American geologists who have found in the Western Territories such convincing instances of the work of denudation in shaping the surface, and have further brought these instances to our doors by means of the admirable illustrations of them which they have supplied in such profusion, and which the American Government distribute so liberally among the geologists of the whole world.

The first part of Dr. Geikie's book deals with land-sculpture in general, and describes the working of Nature's sculpturing tools. It is possible that, here and in the corresponding portions of other geological works, sufficient stress is not laid on the paramount importance of frost among those denuding agents which are generally classed together as "sub-aërial." We might almost say that the results of its work exceed in importance those of all the other denuding forces put together. Such was the impression made upon me when it was once my lot to spend an autumn and winter at St. Bees. South of the bold scarp of St. Bees Head the coast is formed by a line of low cliffs of Boulder Clay, and on a strip of smooth sand at the foot of these it was my practice to take my daily "constitutional." The summer had been hot and dry, and the clay was abundantly cracked; the autumn was a time of incessant and often heavy rain. This almost continuous downpour produced but little destruction; streams of mud stained every here and there the clean sand, but the amount carried down was insignificant. Then came one night's frost, and the beach next day was a sight not easily forgotten. Huge masses of clay, some half as big as a small cottage, cumbered the shore all along; that single night's frost wrought more havoc than the deluge of rain which had been pouring down during the preceding three months almost without cessation.

Having cleared the ground by a preliminary exposition of the principles that are to be our guide, the author takes us away to the Highlands. He insists on the fact that there is nothing in Scotland that can be called a mountain chain in the scientific sense of the expression, and enforces, both by verbal description and apt pictorial illustration, the truth that, when from some commanding height we look over the wild tumbled sea of the Highland hills, it becomes forcibly borne in upon us that they nearly all rise to about the same height. The conclusions to be drawn are that the country was first of all worn down by denudation to an approximately uniform level, and that the valleys are merely ditches dug out by sub-aërial denuding agents across this old table-land. No visitor to the Highlands, who has on a clear day from some point of vantage looked around over the landscape that lies spread below him, can for a moment gainsay the fact, whatever may be his opinion as to the conclusions drawn from it: by nearly all geologists they are accepted as the only reasonable explanation. But we need not go to the Highlands to find instances of old table-lands, which have been trenched by sub-aërial denudation; and perhaps some examples on a smaller scale may be more easily appreciated by beginners. An admirable case is found in the south-west of England. Anyone who walks up from Bristol on to Clifton and Durdham Downs cannot fail to be struck by the remarkable evenness of their surface; after a long pull up a succession of steep inclines you find yourself all at once on a plateau as flat as an alluvial meadow. By dropping down into the gorge of the Avon, an equally good instance of a river-trench cut through the plateau is supplied, and at the same time proof is furnished that the present surface, so suggestive of level-bedded strata underneath, really cuts sheer across beds tilted at high angles, broken by faults, and bent into complicated folds. The flat top runs on along the line of limestone hills that connect Bristol with Clevedon; it is specially noticeable around Clevedon; and after we have crossed a broad depression occupied by softer Coal Measures and Secondary rocks, we find it reappearing in the flat-topped Mendips. Nor is it confined to the exposed portions of the limestone area. Where that rock is covered by Secondary strata, shafts sunk in making tunnels and other excavations show that the newer beds rest on an almost level surface of Palæozoic rocks. Over the whole country there can be traced, whether exposed or hidden, the clearest remnants of an old pre-Triassic table-land.

We also find in the Avon gorge that the limestone has undergone disturbances smaller in amount, but identical in kind, with the gigantic displacements of Sutherland and Ross-shire, of which a short account is given in the present volume, and which are most graphically depicted in a longitudinal section by Mr. Peach. Anyone who wishes to understand what "thrust-planes" are, will find here very good miniature examples. An announcement of the greatest interest is made by the author while touching on this part of his subject. He states that the general assemblage of the organisms in the Durness Limestone recalls none of the Lower Silurian formation of Wales, but rather some of the still older groups of the Lower Palæozoic series of Canada. This reminds us that Lapworth finds the best palæontological parallel to his Moffat series in North America. To speculate on the geographical distribution of animals at so distant a time is risky work, but we may be tempted to conjecture that one great life province included both Scotland and North America, while Wales and Central Europe formed parts of another. Maybe the buried Archæan ridge, the tops of which stick up through the Secondary rocks of the centre of England, formed a portion of the barrier between the two.

But to return to the book before us. Having made good his contention that there was a time when the whole of what is now the Highlands was a broad undulating table-land, and that all its manifold diversity of feature has been carved out by sub-aërial denudation, the author takes us to the hills, the valleys, and the lakes, and enforces his conclusion with a wealth of illustration and a series of word-pictures of the most vivid character, which can be appreciated only by a study of the work itself. The Southern Uplands he treats in a similar manner, and incidentally he puts in a strong plea for a district which possesses much beauty of an unobtrusive kind, but which is apt to be condemned as bare and monotonous.

The Midland Valley is next brought before our notice. How delightful is even a railway ride on a bright sunny day over this charming country! It is in the main a land of broad rolling hills and wide valleys, well wooded and well watered, pleasant to the eye from its brightness and its richness. But, if this were all, there would be, it must be confessed, somewhat of a sameness about it that might be accounted tiresome. But it is redeemed from any risk of monotony by numerous ranges or groups of hills, of moderate elevation, but high enough to tower well above the general level, of ruggeder and wilder forms than the flowing contours of the body of the country, and many of them peaky and mountainous in outline. These are formed of the products of the old volcanoes which were once sprinkled so thickly over the district: their rocks, being harder than the sedimentary beds among which they occur, have been able better to hold out against denudation and have therefore not been worn down to so low a level. A very similar little tract is found in West Shropshire, where the Wrekin, Caer Caradoc, the Cardington Hills, and some other hill-groups—none very high, but all mountains in miniature—introduce a delicious diversity in the rich pastoral country of the Severn valley.

So far we have looked at the book solely from a scientific point of view, and, if no more were said, a suspicion might arise in the minds of the readers of this notice that perhaps the work was a trifle dry. But a very slight acquaintance with the book itself will dispel any misgiving on this head. No one has done more than the author to elucidate the geology of Scotland, but he knows and loves his fatherland too well to look upon it merely as a field for geological research. Legend and history, old ballad and modern poetry, have all been pressed into his service, and he interweaves into his narrative allusion and quotation in a way that enlivens even the most technical parts of the volume. The chapter on "The Influence of the Physical Features of Scotland upon the People" shows well what a vast amount of human interest attaches even to so special a science as geology.

If the intending tourist in Scotland will before he starts read enough of this book to enable him to comprehend its general drift and line of argument, and if he will then take the book with him and study on the ground such of its illustrations and examples as lie on his road, he will ever after thank the author for having supplied him with a new pleasure. One great charm of travel in Scotland is that it is ever leading us through scenes rich in historic associations. The enjoyment derived from such a source may be vastly enhanced by the aid of this book; for he who has mastered its contents will feel an interest not in the events of the human epoch only, but will see in every peak, hill-side, valley, and lake that he passes the monuments of a history which carries him back through that long vista of the ages which geology has opened out to us.

A. H. GREEN.

OUR BOOK SHELF.

Longmans' Shilling Geography. (Longmans, 1887.)

THIS book is dated 1887. It would have disgraced 1862. On the first three pages a volcano is defined as "a mountain from which smoke, flames, ashes, and lava are thrown," the words "continent" and "hemisphere" are treated as synonymous, and the unqualified statement is made that Yorkshire is the basin of the River Ouse. On p. 94 the tributary States of India are styled independent, and on p. 137 the warm water which drifts towards the Pole is placed "low" (deep ?). The mass of the book is of the old vicious type—composed of lists of names and disconnected remarks; and what remains is a disorderly compilation of statements intended to be scientific, but from which the essential point is often omitted. The maps are numerous, but not of a high order. On some of them dials are inserted, showing relative time; but why this should be done for European countries where the difference of time from Greenwich time is counted in minutes, and yet not for the United States, India, or Australia, is not quite obvious. The book is careless and ignorant, and its plan radically bad. We hope that teachers will not be deceived by the title, and imagine that they have here a shortened form of "Longmans' School Geography" by Chisholm. H. J. M.

Les Plantes des Champs et des Bois. Par Gaston Bonnier. (Paris, 1887.)

IT is an accusation which has been justly brought against the botanists of this country that they habitually write in an austere style, which will repel rather than attract the general public; it would be difficult to point to any among the younger men representative of the science who have taken the trouble to please or interest the laity. It is true Miss Plues and Mrs. Gatty have made the attempt, but theirs are books which date many years back. The French are much less open to this charge, having a peculiar and in some cases even a dangerous aptitude for dressing science in popular colours. The "Vegetable World" of Figuier, well known to us from its English translation, has done good service in the past, and now Prof. Bonnier has produced a popular book, made attractive by numerous illustrations, and written in a style which will be readily followed by those who as yet know nothing of the science of botany.

The plan of the book is well adapted to the object before its author: an introduction of some 50 pages suffices for the definition of terms, and of the fundamental points in organography, together with a brief sketch of classification. Armed with this limited but sufficient knowledge, the reader may enter upon his studies in the field. The author divides these into four parts, according to the season of the year, and starting with spring. The description of the plants likely to be found is so arranged as to form a series of progressive lessons, and when autumn is reached the attentive reader will have acquired a fair knowledge of the external form and relationships of many common plants, both Phanerogamic and Cryptogamic. The book is not, and does not pretend to be, any contribution to the sum of knowledge; nevertheless, by means of the easy text and suitable illustrations, the effort of its author to make the rudiments of his science acceptable to the eye, as well as to the understanding, of the general public, should meet with the success it well deserves. F. O. B.

The Hand-book of Jamaica for 1887-88. By A. C. Sinclair and Laurence R. Fyfe. (London: Edward Stanford, 1887.)

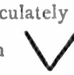
THE compilers of this "Hand-book" have brought together a great mass of trustworthy and useful information about Jamaica. A good description of the island is followed by an historical sketch, after which comes a chronological history, brought down to June 30 last. Then we find all the necessary facts about the political constitution, the revenue, and expenditure, the various departments of the public service, and many other subjects. The articles which appeared in previous editions have been revised, and a good deal of new matter has been introduced. Most persons who may have occasion to refer to the volume will be glad to find in it an account (reprinted from the *Jamaica Gazette*) of the cyclone of last year, by Mr. Maxwell Hall, the list of medicinal and economic plants of the colony prepared by Mr. Fawcett, and the list of sugar-canes prepared by Messrs. Fawcett and Morris.

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. No notice is taken of anonymous communications.*

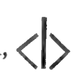
[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Natural History of the Roman Numerals.

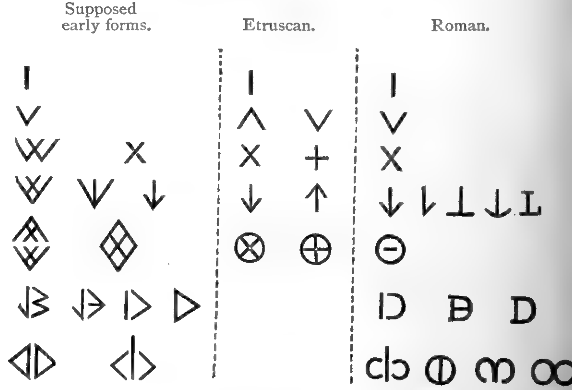
THAT the Roman numerals, in their primitive forms, articulately symbolized a quinary notation based on the hand sign , is the view which the following observations are intended to explain.

A system of enumeration which arose naturally in the progress of the race would be moulded by the kind of expedient adopted in learning to count, by the methods employed in communicating numbers, and by the difficulty of retaining in the memory more than a very few similar signs or sounds repeated in succession. It is not generally doubted that primitive man learned to count, like the child, on his fingers, first on the one hand, and then on the other. The first stage of numeration was thus reached at five, the second at five and five. Numbers were thought of as represented by fingers and hands. From mental helps these bodily members naturally passed into communicative signs: the uplifted finger or fingers, the outspread hand or hands. This would be followed by the use of numerical language. At first only three numbers would have names, there would be a name for one, for five, and for double five. In communicating numbers four times would seem to have been the limit within which the same sign or sound could be repeated in succession without risk of confusion. If this influence alone had been at work, a new name and sign would have been reached at five, at five times five, and at five times five times five, and a perfect and consistent quinary scale would have been the result. But to the primitive man the two hands together would as naturally represent in thought and in communication two fives as the single hand would five ones; so that after double five the next stage would be five double fives, or five times the outspread hands. For this a fresh sound and a new sign would have to be found. It would be vain to conjecture the name, but surely not unreasonable to suppose that this sign would be made by some manner of placing the hand between the feet.

Having reached this point, and in so doing exhausted the simple bodily signs which would naturally be made use of, recourse would be had to marks drawn with the finger upon soft earth or sand. The first written symbols would almost certainly be numerals: nor is it unlikely that from their use arose the idea of an alphabet, and from their shapes the first forms which letters assumed. And these shapes could be nothing else than imitations of the gesture signs. The finger sign would give the stroke |, the five fingers in the unity of the hand would be represented by five strokes converging together, ; this, again, would be doubled and conjoined, , to resemble the combined hands, and tripled, , to imitate the sign for five times double five—the hand added to the outspread feet. The instinct in the primitive mind to represent concretely in the symbol all that was contained in the idea would sooner or later have to give way to the desire after facility and clearness. All that was found unnecessary to the distinctness of the figure would be done away with, strokes would be dropped or shortened, and complex forms would be made simple. In process of time, therefore,  became V;  first  and then X; and , after coalescing in , was simplified into . Before this last change took place, however, it would appear that a double-fifty form was employed, , unifying in , and rounding into  the Etruscan hundred. It was probably because this double-fifty form was used that no need was felt for a name or sign to represent five times fifty, and that a new form only arose when five times double fifty was reached. By this time the simplified fifty, , had become contracted into ↓, and it was plain that a simpler figure could be got by representing double-five times fifty  than by any way of symbolizing five times double fifty. This figure, , would naturally change into , then be altered into , and become joined into one in ▷. By the simple process of doubling this symbol, the next higher form,  or , was attained. This figure does not stand for a thousand, but for five hundred and five hundred. If CIↃ had been, as is generally supposed, a unity meaning 1000, the doubled length of the stroke would have been unintelligible, and the multiple forms would have been CIↃↃ, CIↃↃↃ, and so on, and not CCIↃↃ, CCCIↃↃↃ, and so on.

With the double five hundred, , we have probably reached the limit of what may be called the primitive numerical notation—the notation of a people who spoke of, counted by, and had symbols for ones, fives, double fives, fifties, double fifties, double-five times fifty, and twice double-five times fifty. And it was in the main with this cumbrous system of enumeration that the Romans were content during their whole existence as a people, hardly making any advance beyond substituting a hundred form, C, for the double fifty, and a thousand form, M, for the double five hundred.

TABULAR VIEW.



Glasgow University, September 23. J. LYMBURN.

The "Sky-Coloured Clouds."

THE last distinct display of these that I have seen this season was during the night of July 29–30, and there was a very slight display, if really one, on August 9. They seem only to be visible for a short period before and after the summer solstice. I have looked up all the recorded dates to be found in NATURE and elsewhere when these clouds have been seen, either by myself or others, and the following are the first and last noted dates each year: 1885, June 8, July 7; 1886, May 28, August 12; 1887, June 18, July 30—but a suspected display of the phenomenon was observed four days earlier (June 14), and an equally doubtful one (mentioned before) as late as August 9.

It does not appear to me possible to attribute their luminosity to anything but direct solar illumination. Mr. Rowan in his letter (NATURE, vol. xxxvi. p. 245) thinks otherwise, though in an earlier letter (vol. xxxiv. p. 192) he seems to express a similar opinion to mine. With regard to Prof. C. Piazzi Smyth's letter on the subject (vol. xxxiv. p. 311) I have had a conversation with him since that letter was written, and he said he did not intend to convey the idea (which it had done to Mr. Rowan as well as to myself) that the auroral line appertained to the spectrum of the clouds; and my observations with the spectroscope quite agree with his belief that they are not self-luminous, for I have failed to see any bright line. Prof. Smyth says the spectrum of these clouds is purely that of twilight.

The earliest observations on these clouds would appear to be Mr. Rowan's. Some authoritative assertion of when they were first seen would be very interesting in considering their cause. I do not know that any one else perceived them before 1885, but Mr. Rowan, writing in 1886, says they had occupied his attention during the previous *two or three* years—if two years, the date, 1884, would be after the Krakatão eruption, and would add to the plausibility of their suggested connexion therewith; but if three years, it would disprove such connexion.

Sunderland, September 30. T. W. BACKHOUSE.

A Light Fog.

AT Blowing Rock, Watauga Co., N.C., a part of the main chain of the Blue Ridge Mountains, at this point possessing an elevation of about 4000 feet above the sea, on the night of the 6th inst., while the writer was crossing a causeway through a mill-pond a light fog, obscuring objects at a hundred yards, covered the water. The moon, a little past the full, produced upon the bank of fog a very distinct bow. The bow was luminous white, without any trace of colour, about 2° in breadth. The ends apparently rested on the water, the entire arc being reflected in the water. The segment of fog within the bow was faintly lit up, the lighting up being distinctly seen by contrast with the fog outside the bow. At the same time on turning and looking at the moon it was seen surrounded by a corona about 2° in diameter (four times the moon's diameter), the colours—fairly bright—being in order, going from the moon's limb, yellow, orange, red. A little later, the same night, on the brow of the ridge, in the faint mist which rose in masses, the bow was again seen vividly, against a background of trees; the bow being within 40 paces of the observer.

W. G. BROWN.

Washington and Lee University, Lexington, Va.
September 23.

Destruction of Young Fish.

MAY I call your attention to the wholesale destruction of young fish which is carried on to a great extent round our coast? A few facts may not be out of place. Recently I have been visiting a small fishing village on the east coast, and have carefully noted the amount of young fish rejected by the fishermen on their return from trawling and shrimping. For example, from 4¼ pecks of shrimps no less than 793 flat-fish (dabs, soles, and turbots) were thrown on the beach useless; to this must be added about 200 whiting and an amount of young cod, herring, and skate beyond my power to count. Surely something can be done to remedy this! It is well known to the fishermen that the net does not injure the fish; so that before landing, if the net was roughly examined, all young fish could be thrown into the water again.

DAVID WILSON-BARKER.

66 Gloucester Crescent, Regent's Park, N.W.

ON HAMILTON'S NUMBERS.

FOLLOWING in the footsteps of Hamilton in his Report to the British Association, contained in the Proceedings for the year 1836, we may arrive at a solution, in a certain sense the simplest, of a problem in algebra the origin of which reaches back to Tschirnhausen, born 1651, deceased 1708. Every tyro knows how a quadratic equation, and all equations of a superior degree thereto, may be transformed into another in which the second term is wanting. Tschirnhausen showed that a cubic equation, and all equations superior in degree to the cubic, might be deprived of their second and third terms by solving linear and quadratic equations. Then over a century later Bring, of the University of Lund, in 1786 showed that every equation of the 5th, or any higher degree, might be deprived of its first three terms by means of solving certain cubic, quadratic, and linear equations.[1] What, then, it may be asked, is the law of the progression of which the three first terms are 2, 3, 5? What is the lowest degree an equation can have in order that it may admit of being deprived of four consecutive terms by aid of equations of the 1st, 2nd, 3rd, and 4th degrees, or more generally of i consecutive terms by aid of equations of the 1st, 2nd, 3rd, . . . and ith degrees, *i.e.* by equations none of a higher degree than the ith?[2]

In the 100th volume of *Crelle's Journal* (1886) I have shown that the progression continued as far as the case of eight terms being abolished is as follows—

2, 3, 5, 10, 44, 905, 409181, 83762797734.

These, with the exception of the three first, are not exactly what I call Hamilton's numbers, but serve to lead up to them.

Hamilton's numbers are—

2, 3, 5, 11, 47, 923, 409619, 83763206255, . . .

I will endeavour to explain wherein the difference consists between the two series.

Whilst it is true that four terms may be abolished in an equation of the 10th degree without solving equations beyond the 4th degree, there is this difference in favour of equations of the 11th or any higher degree, viz. that *fewer* biquadratics will be required for them than in the case of an equation limited to the 10th degree. And so in general whether we take, as our inferior limit to the degree of the equation to be transformed, the ith number in the upper series or the ith number in the lower one—whilst in neither case it will be necessary to solve any equations of a degree exceeding i—the total system in the latter case will be of a simpler character than in the former.

The numbers which I have named in honour of Hamilton may be obtained by a process exhibited in the table below—

1	0	0	0	0	0	0	0	...
	1	1	1	1	1	1	1	...
		2	3	4	5	6	7	...
		1	5	9	14	20	27	...
			6	15	29	49	76	...
			5	21	50	99	175	...
			4	26	76	175	350	...
			3	30	106	281	631	...
			2	33	139	420	1051	...
			1	35	174	594	1645	...
				36	210	804	2449	...
				&c.	&c.	&c.	&c.	...

We may now isolate the greatest figure which occurs in each column, and in this may we obtain the numbers 1, 1, 2, 6, 36 . . . which I call hypotenusal numbers; then adding these numbers together and increasing each sum so obtained by unity we arrive at the so-called Hamilton's numbers, viz. 2, 3, 5, 11, 47. . . . Now the question arises as to how they may be calculated; for obviously the crude method above given will be impossible to carry out in practice beyond the first few numbers in the scale. The method of generating functions—of which the idea occurred first to my coadjutor Mr. James Hammond, which certainly ought not to, and probably in the long run could not, have escaped me—leads to a wonderfully beautiful law, by means of which these numbers may be derived successively each from those that go before, just as is the case with Bernouilli's numbers.

The simplest and best mode of proceeding is as

[1] In a letter to Leibnitz (1677), which I have not seen, and the *Acta Eruditorum* for 1683.

[2] How the elevation of the degree of the equation to be transformed makes it possible to abolish a greater number (μ) of terms by an auxiliary system of equations of degrees none exceeding μ will be understood if we consider the cases of a quintic and quartic.

Supposing $(x, 1)^5$ to be a given quintic, on writing

$$\alpha x^4 + \beta x^3 + \gamma x^2 + \delta x + \epsilon = 0$$

we obtain, by elimination of x, $(\alpha, \beta, \gamma, \delta, \epsilon)^5 = 0$, and any solution of his equation will enable us, by a well-known process, to find x by a linear equation.

If we select any letter, α, of the five we may equate it to a linear function of $y, \beta, \gamma, \delta, \epsilon$, so as to obtain

$$y^5 + (\beta, \gamma, \delta, \epsilon)^2 y^3 + (\beta, \gamma, \delta, \epsilon)^3 y^2 + (\beta, \gamma, \delta, \epsilon)^4 y + (\beta, \gamma, \delta, \epsilon)^5 = 0.$$

If in this equation we can find any system of ratios $\beta : \gamma : \delta : \epsilon$ such that $(\beta, \gamma, \delta, \epsilon)^2 = 0$, and $(\beta, \gamma, \delta, \epsilon)^3 = 0$, we can find y by solving a trinomial quintic, and therefore a system of admissible ratios $\alpha : \beta : \gamma : \delta : \epsilon$ becomes known.

All that is requisite therefore is to be able to obtain any point whatever of intersection of two given quadratic and cubic surfaces represented by $(\beta, \gamma, \delta, \epsilon)^2$ and $(\beta, \gamma, \delta, \epsilon)^3$ which obviously may be done by first finding a point (any point) in the quadratic surface (which only necessitates solving some quadratic equation or other); second, at this point drawing a right line (either one of a pair) lying on the surface, which may be effected by a well-known method involving only the solution of a quadratic; and third, finding any one of the three intersections of such line with the cubic surface.

Thus, then, by solving quadratic and cubic equations a quintic may be deprived of three consecutive terms. But not so a quartic; for in the case of a quartic we could not (with any real advantage) use a subsidiary equation of a higher degree than the 3rd. we should thus have only three letters, β, γ, δ, instead of four in the equation in y, and to make $(\beta, \gamma, \delta)^2 = 0$, $(\beta, \gamma, \delta)^3 = 0$, simultaneously, is the problem of finding an intersection of a quadratic and a cubic curve, which necessitates the solution of an equation of the 6th degree.

In the case of the quintic it may be well to notice that the ratios $\alpha : \beta : \gamma : \delta : \epsilon$ will not be all real, and consequently the trinomial quintic into which the original one has been transformed will not have its coefficients real, unless the quadric surface is a hyperboloid of one sheet (since it is only that species of quadric surfaces which contains real straight lines); and I have shown in my paper in Crelle that this is the case then, and then only, when the original quintic has four imaginary roots.

[2] If the number of equations of degrees $i, i-1, i-2, \ldots$ to be solved in the one case reckoned in DESCENDING order are $a, b, c, \ldots l, \ldots$ and in the other $a', b', c', \ldots l' \ldots$ respectively, if l, l' are the two first corresponding numbers which are not identical l' will be less than l.

follows:—Look at the successive lines of figures in the table and write

$$\begin{array}{rrrrrrl}
1 + 0.x + & 0.x^2 + & 0.x^3 + & 0.x^4 + & 0.x^5 + & \ldots\ldots & = F_0 \\
x + & x^2 + & x^3 + & x^4 + & x^5 + & \ldots\ldots & = {}^1F_0 = F_1 \\
 & 2x^2 + & 3x^3 + & 4x^4 + & 5x^5 + & \ldots\ldots & = {}^1F_1 = F_2 \\
 & x^2 + & 5x^3 + & 9x^4 + & 14x^5 + & \ldots\ldots & = {}^1F_2 \\
 & & 6x^3 + & 15x^4 + & 29x^5 + & \ldots\ldots & = {}^2F_2 = F_3 \\
 & & 5x^3 + & 21x^4 + & 50x^5 + & \ldots\ldots & = {}^1F_3 \\
 & & 4x^3 + & 26x^4 + & 76x^5 + & \ldots\ldots & = {}^2F_3 \\
 & & 3x^3 + & 30x^4 + & 106x^5 + & \ldots\ldots & = {}^3F_3 \\
 & & 2x^3 + & 33x^4 + & 139x^5 + & \ldots\ldots & = {}^4F_3 \\
 & & x^3 + & 35x^4 + & 174x^5 + & \ldots\ldots & = {}^5F_3 \\
 & & & 36x^4 + & 210x^5 + & \ldots\ldots & = {}^6F_3 = F_4
\end{array}$$

and so on.
Then evidently

$${}^1F_i = (1 - x)^{-1} F_i - x^i$$

and in general

$${}^{j+1}F_i = (1 - x)^{-1}\,{}^jF_i - x^i$$

and consequently calling the ith number in the hypotenusal series 1, 1, 2, 6, 36, ... a_i, and bearing in mind that ${}^{a_i}F_i = F_{i+1}$, we shall have

$$F_{i+1} - (1 - x)^{-a_i} F_i$$
$$= -x^i\{1 + (1 - x)^{-1} + (1 - x)^{-2} + \ldots + (1 - x)^{-a_i+1}\}$$
$$= x^{i-1}\{(1 - x) - (1 - x)^{-a_i+1}\}$$

which obviously regarded as an equation in differences of the 1st order in F_i, gives the means of expressing F_{i+1} as a function of x and $a_i, a_{i-1} \ldots . a_0$, and consequently must enable us to express all the coefficients in F_{i+1}, of which the first is the hypotenusal number a_{i+1}, in terms of all the hypotenusal numbers of lower order. But what is surprising and unexpected is that, as we shall in a moment see, the relation obtained is expressed by an immediate equation between the sums of the hypotenusals 1, 1, 2, 6, 36, ..., each increased by 2, *i.e.* by an equation between the *ipsissimi* numbers of Hamilton augmented by unity.

In fact, multiplying each side of the equation by $(1-x)^{S_i+1}$, where

$$S_{i+1} = a_0 + a_1 + a_2 + \ldots . + a_i,$$

(so that $S_1 = 1$, $S_2 = 2$, $S_3 = 4$, $S_4 = 10$, $S_5 = 46$, ...), it becomes

$$(1 - x)^{S_i+1} F_{i+1} - (1 - x)^{S_i} F_i$$
$$= x^{i-1} (1 - x)\{(1 - x)^{S_i+1} - (1 - x)^{S_i}\}$$

which equation, it may be noticed, proved for all values of i down to 1 may be extended also to $i = 0$, provided we make $S_0 = 0$.

Accordingly, giving i all values down to 0 inclusive, we shall easily obtain by addition

$$(1 - x)^{S_i} F_i = 1 + x^{i-2} (1 - x)^{S_i+1} - x^{-1}(1 - x)$$
$$+ x^{i-3} (1 - x)^{S_{i-1}+2} + x^{i-4} (1 - x)^{S_{i-2}+2} + \ldots .$$
$$+ x^{-1} (1 - x)^{S_1+2},$$

or which is the same thing

$$(1 - x)^{S_i} F_i - 2 + x^{-1} - x^{i-1} (1 - x)^{S_i+1}$$
$$= x^{i-2} (1 - x)^{S_i+2} + x^{i-3} (1 - x)^{S_{i-1}+2} + \ldots .$$
$$+ x^{-1} (1 - x)^{S_1+2}.$$

Hence, equating the coefficients of x^i and using $\beta^k q$ in general to signify $\frac{q(q-1) \ldots . (q-k+1)}{1.2 \ldots . k}$, if we call $1 + S_i = H_i$ we obtain

$$a_i + S_i + 1 = H_{i+1}$$
$$= \beta^2(H_i + 1) - \beta^3(H_{i-1} + 1) + \beta^4(H_{i-2} + 1) - \ldots .$$
$$+ (-)^{i+1} \beta^{i+1} (H_1 + 1)$$

And on calling $1 + H_i = E_i$ this equation becomes

$$1 - \beta E_{i+1} + \beta^2 E_i - \beta^3 E_{i-1} + \beta^4 E_{i-2} - \ldots .$$
$$+ (-)^{i+1} \beta^{i+1} E_1 = 0.$$

This relation between the sharpened Hamiltonian numbers (*i.e.* these numbers increased by a unit) is in a slightly different form from the result obtained by Hammond. By aid of this formula the values of the successive numbers can be calculated with wonderful facility. The series of them commencing with 1, which although not properly speaking a Hamiltonian number, belongs to the class $S_i + 1$, have been found to be

1, 2, 3, 5, 11, 47, 923, 409619, 83763206255,
350812590629085879817 1,
61534736870965787584485228092750775204331 67, ...

Thus *ex. gr.* having found $H_6 = 923$ and all the Hamiltonian numbers inferior to it, we have

$$H_7 = \frac{924 . 923}{1 . 2} - \frac{48 . 47 . 46}{1 . 2 . 3} + \frac{12 . 11 . 10 . 9}{1 . 2 . 3 . 4} - \frac{6 . 5 . 4 . 3 . 2}{1 . 2 . 3 . 4 . 5}$$
$$= 426426 - 17296 + 495 - 6$$
$$= 409619.$$

I have alluded to Bernouilli's numbers as a parallel case to that of Hamilton's in so far as they too are subject to a scale of relation by which each can be expressed in terms of those of a lower order than itself.

If we use

$$B_1,\ B_2,\ B_3, \ldots .$$

to signify as usual the Bernouillian numbers

$$\frac{1}{6},\ \frac{1}{30},\ \frac{1}{42}, \ldots .$$

and write

$$G_0 = -1,\ G_1 = -1,\ G_2 = (-4)B_1,\ G_3 = 0,$$
$$G_4 = (-4)^2 B_2,\ G_5 = 0,\ G_6 = (-4)^3 B_3,$$

and so on, the well-known scale of relation for Bernouilli's numbers may (provided only that n be odd) be written under the form

$$\Sigma_{k=0}^{k=n} (-)^k \beta^k n . G_{n-k} = 0.$$

If in this formula we suppress the n which intervenes between the operative symbol β^k and G_{n-k}, so that the former is brought into juxtaposition with and acts on the latter, it becomes identical with that which we have found for the sharpened Hamiltonian numbers.

Those who wish to pursue the subject further may consult my memoir "On the so-called Tschirnhausen Transformation" (*Crelle*, vol. c. pp. 465–86), another "On Hamilton's Numbers," by Mr. Hammond and myself conjointly, just published in the Philosophical Transactions, and an addition thereto about to be presented to the Royal Society, in which a large generalization of the theory discussed by Hamilton in his Report to the British Association for 1836, but not brought by him to perfection, is resolved with a completeness which leaves nothing to be desired.

J. J. SYLVESTER.

New College, Oxford, October 1.

MODERN VIEWS OF ELECTRICITY.[1]

PART I.

II.

FIRST you have an inextensible endless cord circulating over pulleys; this is to represent electricity flowing in a closed circuit. Electromotive forces are forces capable of moving the cord, and you may consider them applied either by a winch, or by a weight on the hook W. A battery cell corresponds to a small weight; an electric machine to a slow but powerful winch. Clamping the cord with the screw S corresponds to making the resistance of the circuit infinite. Instead of the cord, clamp, and driving

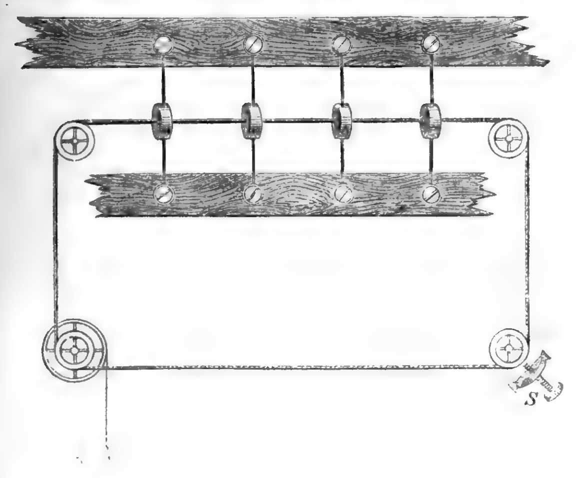


FIG. 5.—Mechanical analogy of a *metallic* circuit.

pulley, one might consider an endless pipe full of liquid with a stop-cock and a pump on it, but for many purposes the cord is sufficient and more simple. In Fig. 5, the only resistance to the motion is friction, and there is no tendency to spring back. Fixed beads are shown on the cord to typify atoms of matter, and they may be more or less rough to represent different specific resistances. If the cord be moved, heat is the only result.

Now pass to Fig. 6. Here the cord is the same as before

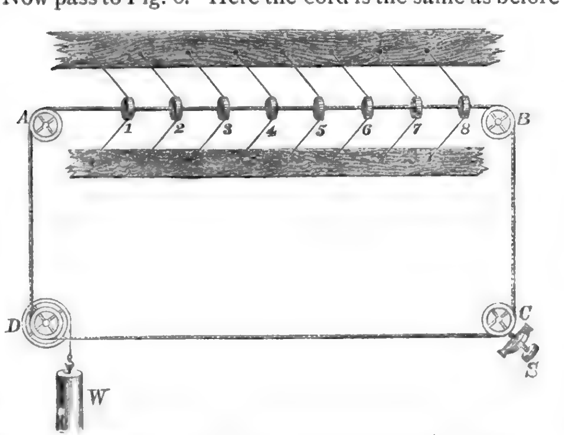


FIG. 6.—Mechanical analogy of a circuit partly *dielectric*: for instance, of a charged condenser. A is its positive coat, B its negative.

but the beads are firmly attached to it, so that if it moves they must move with it. They represent, therefore, the particles of an insulating substance. Nevertheless, their supports are not rigid—they do not prevent the cord moving at all; they allow what is called electric "*displacement*," not conduction; they can be displaced a little from their natural position, but they spring back again when the disturbing E.M.F. is removed. The beads in this figure are supposed to be supported by elastic threads: if the cord were replaced by a closed pipe full of water they would be replaced by elastic partitions.

Apply a given E.M.F. to this cord and a definite displacement is produced. One side gets more cord than usual—it is positively charged; the other gets less—it is negatively charged. If the applied E.M.F. exceeds a certain limit the strain is too great. The elastics break, and you have disruptive discharge with a spark. But even when the strain is only moderate some of the supports may yield viscously, or be imperfectly elastic and permit a gradual extra displacement of the cord, known to telegraphists as "soaking in."

When discharge is now allowed, it will not at once be complete; a large portion of the displacement will be at once recovered, but the rest will gradually "soak out" and cause residual discharges.

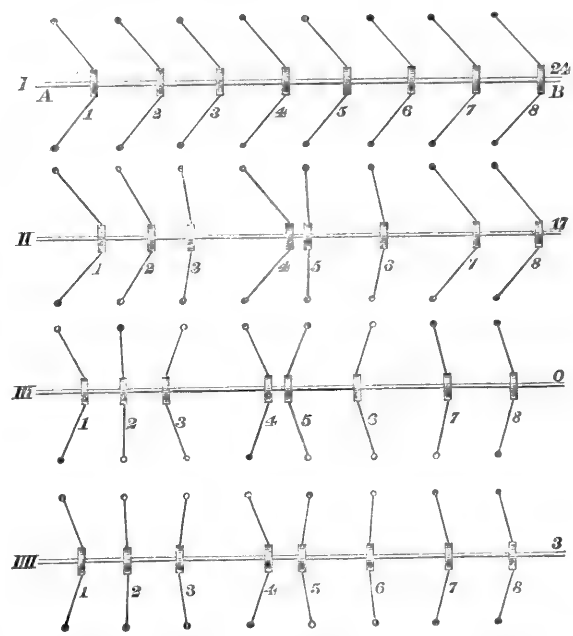


FIG. 7.—Stages in the discharge of a stratified condenser; showing one way in which the phenomena of "residual charge," "internal charge," and "soaking out" are produced.

If the dielectric is at all stratified in structure, so that some of the beads allow cord to slip through them—or yield more than others—then this residual charge effect will become very prominent.

Fig. 7 illustrates the various stages of a stratified dielectric, with layers of imperfect insulating power. I. represents a recent charge, of E.M.F. 24. II. represents the same after lapse of time, reduced to 17 by partial internal leakage; and shows internal charge. The circuit itself is supposed to have been perfectly insulating all the time. III. shows the first discharge; and IIII. shows the state attained after again waiting, viz. a residual charge with an E.M.F. 3 in the old direction.

Return, however, to the simple discharge, and see how it occurs. Will it take place as a simple sliding back of the beads to their old position? Yes, if the resistance of the circuit is great, but not otherwise. If the cord is fairly free the beads will fly past their mean position, overshooting their mark, then rebound, and so, after many quick oscillations, will finally settle down in their

[1] Expansion of a lecture delivered by Dr. Oliver Lodge, partly at the London Institution on January 1, 1885, and partly at the Midland Institute, Birmingham, November 15, 1886, but not hitherto published. Continued from p. 536.

natural position. Thus is represented the fact that the discharge of a Leyden jar is in general oscillatory; the apparently single and momentary spark, when analyzed in a very rapidly rotating mirror, turning out to really consist of a series of alternating flashes rapidly succeeding one another, and all over in the hundred-thousandth of a second or thereabouts. These oscillatory currents were predicted and calculated beforehand by Sir William Thomson; they were first observed experimentally by Feddersen. The oscillations continue until the energy stored up in the strained medium has all rubbed itself down into heat.

Fig. 8 shows part of an actual model of the kind.

To make the model represent *charge by induction* all that has to be done is to immerse a conductor into the polarized dielectric—in other words, to make one or more

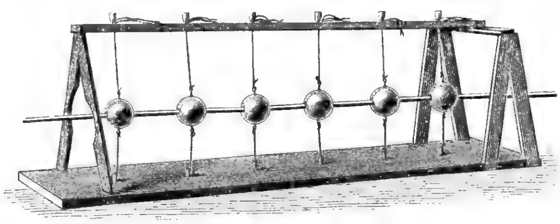


FIG. 8.—Partial model of a dielectric.

of the beads of the fixed and slippery conducting kind, the other beads on the cord being of the elastic and adhesive or insulating kind. Then when the displacement occurs it is plain that a deficiency of cord will exist on one side of the metallic layer and a surplus on the other, as shown in Fig. 9. This state of things corresponds exactly to the equal opposite induced charges on a conductor under induction, as in Fig. 3. If the strain on one side be relieved by letting the beads on that side slip back on the cord: that corresponds to touching the conductor to earth, as in Fig. 4. The other side has now to withstand the whole E.M.F., consequently the strain there and the charge there will have increased. Remove now the applied E.M.F., and the negative charge appears on both sides of the metal partition, either equally, or more markedly on that side which has fewest beads, *i.e.* which is nearest to other conductors.

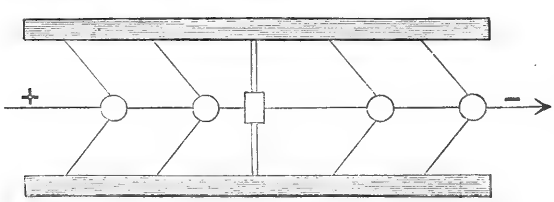


FIG. 9.—Metallic layer in the midst of a polarized dielectric, showing opposite charges "induced" on its surfaces. (Compare Fig. 3.)

Hydraulic Model of a Leyden Jar.—So much for the cord model, but I will now describe and explain an hydraulic model which illustrates the same sort of facts: some of them more plainly and directly than the cord model. Moreover, since all charging is essentially analogous to that of a Leyden jar, let us take a Leyden jar and make its hydrostatic analogue at once.

The form of jar most convenient to think of is one supported horizontally on an insulating stand, with pith ball electroscopes supplied to both inner and outer coatings.

To construct its hydraulic model, procure a thin india-rubber bag, such as are distended with gas at toy-shops; tie it over the mouth of a tube with a stop-cock, A, and insert the tube by means of a cork into a three-necked globular glass vessel or "receiver," as shown in the diagram, Fig. 10.

One of the other openings is to have another stop-cock tube, B; and the third opening is to be plugged with a cork as soon as the whole vessel, both inside and outside the bag, is completely full of water without air-bubbles.

This is the insulated Leyden jar: the bag represents the dielectric, and its inner and outer coatings are the spaces full of water.

Open gauge-tubes, *a* and *b*, must now be inserted in tubes A and B, to correspond to the electroscopes supplied to the jar; and a third bent tube, C, connecting the inner and outer coatings, will correspond to a discharger. Ordinarily, however, of course C will be shut.

A water-pump screwed on to A will represent an electric machine connected to inner coating; and the outer coating, B, should open into a tank, to represent the earth. The pump will naturally draw its supply of water from the same tank.

The bag being undistended, and the whole filled with water free from air, the level of the water in the two gauge-tubes will correspond with that in the tank; and this means that everything is at zero potential, *i.e.* the potential of the earth.

Now, C being shut, shut also B, open A, and work the pump. Instantly the level in the two gauges rises greatly and equally: you are trying to charge an insulated jar. Turn an electric machine connected to a real jar, and its two pith balls will similarly and equally rise.

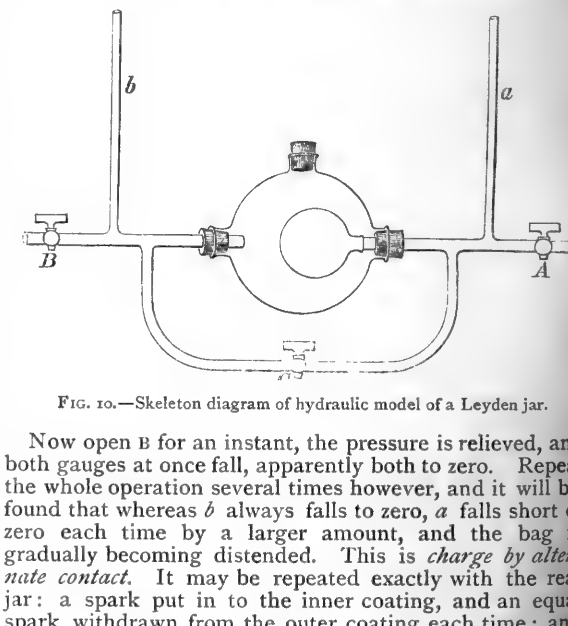


FIG. 10.—Skeleton diagram of hydraulic model of a Leyden jar.

Now open B for an instant, the pressure is relieved, and both gauges at once fall, apparently both to zero. Repeat the whole operation several times however, and it will be found that whereas *b* always falls to zero, *a* falls short of zero each time by a larger amount, and the bag is gradually becoming distended. This is *charge by alternate contact.* It may be repeated exactly with the real jar: a spark put in to the inner coating, and an equal spark withdrawn from the outer coating each time; and unless this outer spark is so withdrawn, the jar declines to charge: water (and electricity) being incompressible.

If B is left permanently open, the pump can be steadily worked, so as to distend the bag and raise the gauge *a* to its full height, *b* remaining at zero all the time, save for oscillatory disturbances.

Having got the jar charged, shut A, and remove the pump, connecting the end of A with the tank directly.

Now of course by the use of the discharger C the fluid can be transferred from inner to outer coat, the strain relieved, and the gauges equalized. But if this operation be performed while the jar is insulated, *i.e.* while A and B are both shut, the common level of the gauges after discharge is not zero, but a half-way level; and the effect of this is very noticeable if you touch an insulated Leyden jar after it has been discharged.

Instead of using the discharger C, however, we can proceed to discharge by alternate contact, and the operation is very instructive.

Start with the gauge b at zero, and the gauge a at high pressure. Open stop cock A; some water is squeezed out of inner coating, and the a gauge falls to zero, but the suck of the contracting bag on the outer coat pulls down

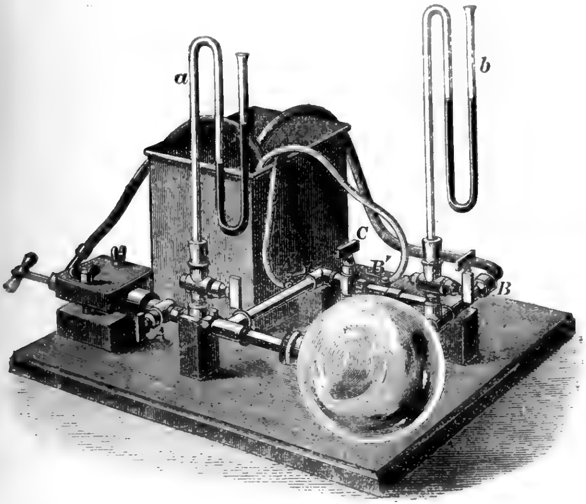


FIG. 11.—First actually constructed model Leyden jar, with mercury gauges for electrometers; the whole rigged up with things purchasable at a plumber's, except the pump.

the gauge b *below* zero, the descent of the two gauges being nearly equal.

Next shut A and open B; a little water flows in from

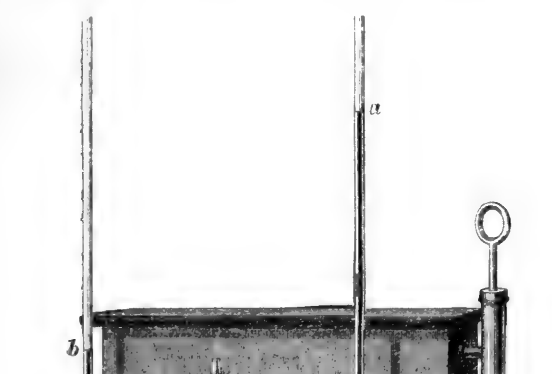


FIG. 12.—Latest form of hydraulic model of a Leyden jar with water gauges, the whole arranged vertically to be more conspicuous. The pump is a force-pump with a communication between top of barrel and tank to get rid of stray water.

the tank to still further relieve the strain of the bag, and both gauges rise; b to zero, a to something just short of its old position.

Now shut B and open A again: again the two gauges descend. Reverse the taps, and again they both rise; and so on until the bag has recovered its normal size. This is discharge by alternate contact, and exactly imitates the behaviour of an insulated charged Leyden jar whose inner and outer coats are alternately touched to earth. Its pith balls alternately rise with positive and with negative electricity, indicating potentials above and below zero.

Figs. 11 and 12 are taken from photographs of apparatus I have made to use as just described. The glass globe with the partially distended bag inside it, the pump, the tank, the gauges a and b, the stop-cocks A B C, will be easily recognized. Two extra stop-cocks, A' and B', leading direct to tank, are extra, and are to save having to disconnect pump and connect A direct, when exhibiting the effect of "discharge by alternate contact." But the tank is not sufficiently tall in Fig. 12; I have doubled its height since. The full height of the gauge-tubes is barely shown.

In any form of apparatus it is essential to fill the whole with water—pipes, globe, everything—before commencing to draw any moral from its behaviour. It is rather difficult to get rid of a large bubble of air from the top of the globe of Fig. 11, and though it is not of very much consequence in this place, the stop-cock in Fig. 12 is added to make its removal easy. The gauges in Fig. 11 may be replaced by others arranged as a lantern-slide, and connected by flexible tubing full of air.

I have explained thus fully the hydraulic illustration of Leyden jar phenomena, because these constitute the key to a great part of electrostatics. The illustration is not indeed a complete or perfect one by any means, but by combining with it a consideration of the endless cord models, and of what I have endeavoured to explain concerning conduction and insulation in general, a distinct step may be gained.

Think of electrical phenomena as produced by an all-permeating liquid embedded in a jelly; think of conductors as holes and pipes in this jelly, of an electrical machine as a pump, of charge as excess or defect, of attraction as due to strain, of discharge as bursting, of the discharge of a Leyden jar as a springing back or recoil oscillating till its energy has gone.

By thus thinking you will get a more real grasp of the subject and insight into the actual processes occurring in Nature—unknown though these may still strictly be—than if you employed the old ideas of action at a distance, or contented yourselves with no theory at all on which to link the facts. You will have made a step in the direction of the truth, but I must beg you to understand that it is only a step, that what modifications and additions will have to be made to it before it becomes a complete theory of electricity I am wholly unable to tell you. I am convinced they will be many, but I am also convinced that it is unwise to drift along among a host of complicated phenomena without guide other than that afforded by hard and rigid mathematical equations.

The mathematical theory of potential and the like has insured safe and certain progress, and enables mathematicians to dispense for the time being with theories of electricity and with mental imagery. Few, however, are the minds strong enough thus to dispense with all but the most formal and severe of mental aids: and none, I believe, to whom some mental picture of the actual processes would not be a help if it were safely available.

Such a representation I have endeavoured partially to lay before you to-night, and I hope, if I have succeeded in making myself at all intelligible, that those students of electricity who may be present will find it of some use and service. O. J. LODGE.

(To be continued.)

NEW FORM OF CONSTRUCTION OF OBJECT-GLASSES INTENDED FOR STELLAR PHOTOGRAPHY.

THE interest now generally taken in stellar photography will probably make it desirable that the object-glasses of telescopes ordinarily used for visual purposes should be so constructed as to be readily adapted to photographic use. As now commonly made, the correction for chromatic aberration by means of the flint-glass lens of a refracting telescope is too great to give satisfactory photographic images. The method of adapting such a telescope to photographic purposes which was employed by Mr. Rutherford, and more recently in the case of the great telescope of the Lick Observatory, is to provide an additional convex lens of long focus which may be mounted when photographs are to be taken, and removed when direct observation is desired. The objections to this method are the expense of the additional lens and the introduction of two more surfaces.

Another way of removing the excessive correction for chromatic aberration is to separate the flint- and crown-glass lenses, and to place the flint nearer the eye-piece. But when this is done, while the correction for colour becomes satisfactory, the spherical aberration is only partially corrected, the focal length of the central part of the object-glass being greater than that of the surrounding portions. This difficulty, however, may be surmounted by giving different curves to the two surfaces of the crown-glass lens, and reversing it when the lenses are separated.

In an object-glass of this construction, when used for direct observation, the two lenses are in contact, with the more convex side of the crown lens next the flint. For photographic use, the lenses are separated, and the more convex side of the crown-glass lens is turned outwards. In order to determine whether this principle of construction should be adopted in the case of a large object-glass to be made for experimental work undertaken with the aid of the Boyden Fund, recently transferred to the Observatory of Harvard College, for the purpose of obtaining astronomical observations at elevated stations, a small object-glass was made upon the new plan by Messrs. Alvan Clark and Sons. As this object-glass proved to be well adapted to its purpose, the larger object-glass above mentioned has been similarly constructed by the same makers; its aperture is 13 inches, and its focal length about 180 inches. Upon trial, it is found that the images formed by this object-glass are excellent when the instrument is used for direct observation, and that the photographic images are equally good when the lenses are separated 3 inches and the crown-glass reversed.

The curvature given to the surfaces of such an object-glass will depend upon the quality of the glass employed; in this particular instrument the radius of curvature for the less convex side of the crown-glass is 86 inches; for the more convex side, 77 inches; for the concave side of the flint, 73·8 inches; the side of the flint-glass which is turned towards the eye-piece is convex, and its radius of curvature is 1020 inches.

EDWARD C. PICKERING.

Harvard College Observatory, Cambridge, U.S., September 29.

WILLIAM S. SYMONDS.

WE have already announced the death, on September 15, at Cheltenham, of the Rev. William S. Symonds, Rector of Pendock, F.G.S., and a J.P. for the county of Worcestershire. Mr. Symonds, the eldest son of Mr. William Symonds, of Elsdon, Hereford, was born in Hereford in 1818, took his degree at Christ's College, Cambridge, in 1842, and in 1868 was presented to the Rectory of Pendock.

For several years past Mr. Symonds had suffered from heart-disease, and was compelled to withdraw from his parish duties, as well as from participation in those scientific pursuits in which he had long been actively occupied.

The intimate friend of Murchison, Lyell, and of other pioneers in geology, and frequently their companion in their excursions, he enjoyed exceptional opportunities for observing the facts relating to this science, and has contributed numerous papers, chiefly on the rocks and fossils of the west of England, to the scientific periodicals. Yet he sometimes engaged in discussions of a more general character. In the question of the reptiliferous sandstone of Elgin he took much interest. Before the meeting of the British Association at Aberdeen, in 1859, he visited the Elgin area, and, having worked over it with great care, was led to accept the views of Sir C. Lyell in opposition to those of Murchison; and in the discussions at the meeting strongly insisted on the view that the reptiliferous sandstones are of the New Red or Triassic, and not of the Old Red Sandstone age.

Mr. Symonds did not devote as much attention to palæontology as to physical geology, although even in this branch of the science he has left his mark, and has made numerous and valuable contributions from his collections of fossils to several local museums.

During the latter part of his life his interest was much concentrated on the phenomena of the glacial drifts, and the question of the antiquity of prehistoric man. In 1871 he communicated to the *Geological Magazine* (p. 433) a paper on the great Doward Caves, and in his "Severn Straits, or, Notes on Glacial Drifts," he gives his conclusions on this subject.

But it is perhaps chiefly as an earnest and eloquent expounder of the facts and principles of geology and cognate subjects that he will be remembered. He had a remarkable power of infusing his own spirit of research and love of Nature into his writings. Few can read his "Records of the Rocks," "Old Stones," and "Old Bones," or his delightful romances of "Malvern Chase" and "Hornby Castle"—tales of the Wars of the Roses and the Parliament—without recognizing this. In the formation and management of many of the field clubs of the west of England Mr. Symonds's ability was especially conspicuous. He was engaged, with the late Mr. Scobie, of Hereford, in the formation of the Woolhope Field Club, and was elected its President in 1854. He was the frequent companion of the late Sir William Govin in his travels, and took an active part in the management of the Cotteswold Club, of which Sir William was President.

By those who had the privilege of an intimate acquaintance with Mr. Symonds, he will ever be thought of and respected as an earnest and most uncompromising seeker after truth for its own sake. He considered that man is performing a most religious duty when he sits as a humble student at the feet of Nature to learn, as far as may be, the lessons which Nature's Creator would teach him. It was a faith, too, which, throughout the trying and painful complaint that clouded the last days of his life, never failed him, and which enabled him to look forward to, and speak of, the great change which was before him, with cheerful hope.

The number of papers which Mr. Symonds has from time to time contributed to various scientific journals appears to have been over forty.

NOTES.

WE reprint to-day from the *Times* an admirable letter by Mr. Samuel Smith on education in Germany. Mr. Smith gives a clear account of some important elements of the educational system which the Germans are gradually bringing to perfection;

and no one who reads what he has to say will be surprised that in many departments of industry our manufacturers are being surpassed by their Teutonic rivals. The letter contains nothing new; but the essential facts connected with this vital subject cannot be too often pressed on the attention of the public.

WE regret to announce the death of Mr. Joseph Baxendell, F.R.S., of the Observatory, Birkdale, Southport. He died at the age of seventy-two.

ACCORDING to a report which appears in the *Mouvement Géographique*, Kilimanjaro has at last been ascended to its summit. This has been done by a German traveller, M. Meyer, of Leipzig. We know that the mountain presents two great peaks:—Kimawenzi, 4973 metres in height, and Kibo, estimated by Mr. H. H. Johnston at 5745 metres. Mr. Johnston succeeded in ascending the latter to a height of 4950 metres. M. Meyer, it is stated, has succeeded in reaching the crater summit of Kibo, which he estimates at 6000 metres. Before giving absolute credence to these statements, it will be well to await the publication of details by M. Meyer.

IN an interesting article in the *Times* on Monday, on "The British Race-Types of To-day," it was stated that "for a generation or more the advocates of the view that the English are almost unmixed Teutons pressed their ideas upon the scientific and literary world with a persistence and learning which went far to produce conviction." Prof. Huxley, writing to the *Times* about this statement, maintains that, whatever may have been the case with "the literary world," scientific anthropologists were never misled by the "persistence and learning" of those who contended that the English are of almost purely Teutonic origin. "A score of years ago," says Prof. Huxley, "this question was hotly debated; and I do not think that, at that time, any of my anthropological colleagues would have found much fault with the propositions laid down in a paper 'On some Fixed Points of British Ethnology,' which was published in 1871, which propositions are in substance corroborated by the writer of your article."

CAPT. DICKINSON, who has been in charge of the Royal Society's boring at Zagazig, in the delta of the Nile, reports that a depth of 308 feet 6 inches has been reached. The soil from 190 feet has been sand, varying in coarseness, and sometimes mixed with stones. At 308 feet, 4 inches of blue clay was passed through.

THE *Nation* of September 22 prints a letter from Miss Mabel Loomis Todd on the Eclipse Expedition in Japan. Miss Todd's letter is dated Shirakawa, Japan, August 20. We regret to find that the weather was not more favourable for observers in Japan than for observers in Russia and Germany.

WE have received the fourth volume of the Proceedings and Transactions of the Royal Society of Canada. It contains the Proceedings and Transactions during the year 1886. Most of the papers are in English, but some are in French. Among the more important papers we may note "The Right Hand and Left-handedness," and "The Lost Atlantis," by Dr. Daniel Wilson; "The Genetic History of Crystalline Rocks," and "Supplement to 'A Natural System in Mineralogy,' &c.," by Dr. T. Sterry Hunt; "Some Points in which American Geological Science is indebted to Canada," and "On the Fossil Plants of the Laramie Formation of Canada," by Sir J. William Dawson; and "On certain Borings in Manitoba and the North-West Territory," by Dr. Geo. M. Dawson.

The October Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, presents the results of a careful inquiry made by Mr. Arthur Shipley, under the auspices of the Royal Gardens, into a disease affecting the onion crop at the Bermudas. Mr. Shipley shows that the disease is caused by a fungus, *Peronospora Schleideniana*, which lives parasitically upon the leaf of the onion plant; and that the atmospheric conditions which favour the progress of the disease are heavy dews or rains, followed by warm, moist, calm weather, and the absence of direct sunshine and cold winds. In favourable weather the progress of the disease is very rapid. The fungus lives in the tissues of the leaf, choking up the air-passages and absorbing the nutritive fluid formed in the cells. Its stem protrudes through the stomata of the leaf into the air. Its branches bear spores at their tips. The reproduction of the fungus is effected by means of these spores, which float about through the air, and also by means of certain special cells formed by the fungus and known as resting-spores. These pass the winter in the earth, and are capable of retaining the power of germination for two or three years. It is by their means that the disease is carried on from one season to another. Mr. Shipley offers various practical suggestions, which will no doubt be of considerable service to cultivators.

DR. SCHWEINFURTH, in co-operation with Prof. Ratzel, is about to publish through Brockhaus, of Leipzig, a collection of all the letters and other writings of Emin Pasha. Dr. Junker also, in association with M. Richard Buchta, who visited the Upper Nile region some years ago, is about to issue a work dealing with the events of recent years in the Soudan. The volume will be illustrated with two maps, and have portraits of Gordon, Emin, Gessi, Lupton, and Slattin. Dr. Junker has just finished his work of preparing at Gotha the great map of his explorations in the basin of the Wellé and the region of the Upper Nile. It has been drawn by Dr. Hassenstein in four sheets, which will appear in the beginning of next year in a supplemental part of *Petermann's Mitteilungen*.

AN elaborate work on palæontology ("Handbuch der Palæontologie") is being issued at Munich and Leipzig by R. Oldenbourg. The editor is Prof. Karl A. Zittel, of Munich, who is aided by Dr. A. Schenck, of Leipzig, and Mr. S. H. Scudder, of Boston. The work is divided into two parts, in the first of which palæontology is dealt with; in the second, palæophytology. The first part, when completed, will occupy three volumes; the second, one. The first two volumes have been published, and we have just received the first "Lieferung" of the third volume. The subject of this "Lieferung" (illustrated by 266 woodcuts) is "Vertebrata: Pisces." The subjects of the later "Lieferungen" of the same volume will be "Vertebrata: Amphibia, Reptilia, Aves, Mammalia." We may mention that a French translation, by Dr. Charles Barrois, is issued simultaneously with the original German work.

A WORK on "The Cultivated Oranges and Lemons, &c., of India and Ceylon," by Brigade-Surgeon E. Bonavia, M.D., of the Indian Medical Department, is about to be issued. The writer's object is mainly practical and economical, but he deals also with some questions of purely scientific interest. His researches have brought him into contact with every variety of Citrus in India and Ceylon, and he claims that he has been able to dispose of, or at any rate to throw new light on, certain disputed points, both botanical and historical, in connexion with this genus. The book will be accompanied by an atlas in foolscap size, consisting of 259 plates of outline drawings of all the varieties of Citrus to be found in India and Ceylon.

MESSRS. DULAU AND CO. have now ready for publication "A Chapter in the History of Meteorites," by the late Dr. Walter Flight, F.R.S.

A THIRD edition of "Weather Charts and Storm Warnings," by Mr. R. H. Scott, F.R.S., has just been issued. The writer's object in preparing the work was to convey some idea of the state of weather knowledge, so that readers might understand what was to be learned from a careful study of the weather charts in the newspapers and of the remarks appended to them.

The second edition has long been out of print. In the present edition the entire text has been revised, and some new chapters have been added, mainly relating to changes and improvements in the Meteorological Office work since the year 1876.

MESSRS. DE LA RUE AND CO. will publish shortly the second volume of "A Treatise on Electricity and Magnetism (Methods of Measurement and Applications)," by E. Mascart, Professor in the Collège de France, and Director of the Central Meteorological Bureau, and J. Joubert, Professor in the Collège Rollin. The work is translated by Dr. E. Atkinson, Professor of Experimental Science in the Staff College.

MESSRS. MACMILLAN will shortly publish a new edition of Huxley and Martin's well-known "Manual of Elementary Biology," considerably revised and extended by Messrs. Howes and Scott, Assistant Professors in the Normal School of Science and Royal School of Mines. To the new volume Prof. Huxley will contribute a preface.

A NEW way of utilizing dynamite has been lately devised by a French military engineer, M. Bonnetond. He uses the expansive force to drive out, for a brief period, the water from portions of wet ground in which foundations are to be made. The method is now in practice in the construction of a fortified *enceinte* at Lyons. A hole is first bored 10 or 12 feet deep, and about 1½ inch wide, in the wet ground. Into this is passed a string of cartridges of dynamite, which is then exploded. The water is thus driven far out beyond the sides of the cavity, over a yard wide, which is produced, and it does not reappear till after half an hour at least. The workmen thus have time to clear the cavity and introduce quickly-setting concrete. When the water returns it cannot injure the foundation. A rapid rate of progress is realized by this method.

A RECENT number of *La Nature* (September 17) has an illustrated account of a steam tricycle contrived by MM. Roger de Montais and L'Héritier, which will go 16 to 18 kilometres an hour with one person, and 14 to 16 with two. In front is a small boiler heated by petroleum, which gives off, it is said, no smoke nor smell, nor unpleasant heat. Under the seat is the petroleum reservoir, holding ten litres, enough to last ten hours, and behind is a water reservoir which holds thirty-four litres, allowing a two and a half hours' run without fresh supply. This water reservoir has one compartment for cold water, and another for water constantly heated by escape of steam; the latter feeding the vertical engine behind, and the former having steam turned into it at will.

IN a new galvanic battery, described by Herr Friedrichs in *Wiedemann's Annalen* (No. 9), a series of inverted bottle-shaped vessels have their necks connected by means of a horizontal tube, into which the exciting liquid (say dilute sulphuric acid and bichromate of potash) flows through a flexible tube from the lower part of a jar, which is raised or lowered to fill or empty the vessels. The liquid can also be let off through a cock at the further end of the connecting tube.

THE various Transatlantic and British fish hatched out this year by the National Fish Culture Association have prospered remarkably well at the establishment at Delaford Park. Some of the Californian rainbow-trout have grown to the extent of 4 inches, and the *S. fontinalis* produced from ova taken from two-year-old fish in the ponds have thriven in a similar manner. The landlocked salmon (*S. sebago*), despite their partiality to deep waters, are also doing well. The whitefish (*Coregonus albus*) have attained a length of 4 inches, and appear to be thriving better this year in consequence of their habitat having been deepened to suit their natural necessities. The cultivation of coarse fish is proceeding, a large quantity of fry bred from German carp being now in the ponds, besides other species.

AT the request of the Austrian Minister for Agriculture, Herr Putik has recently investigated the hydrography of Central Carniola, and given special attention to the Zirknitzer Lake. The phenomena of the periodical emptying and filling of this lake are very extraordinary; a gigantic cave, called Gilovca or Karlovca by the natives, and situated at the north-west corner of the lake, near Niederndorf, forms an outlet for the overflow. This cave lies at the foot of perpendicular rocks, and leads to a number of subterranean lakes, five of which Herr Putik has crossed. He is convinced that there are a great many more of these lakes.

IN the spring of 1885 a Society was founded in Hamburg for the establishment of plantations in Cameroon. Some 30,000 cocoa shrubs were planted, which are now 3½ to 4 feet high, and this year 40,000 more have been added.

ON Monday evenings in Michaelmas Term a course of lectures on "Climate and Weather" will be delivered by Mr. A. W. Clayden, at the City of London College, White Street, Moorfields, E.C. The lectures will be delivered in connection with the London Society for the Extension of University Teaching, and under the auspices of the Mitchell (City of London) Trustees.

A CORRESPONDENT of the *Indian Forester* communicates the following account of "a real weeping tree" to that periodical:—"On my way to and from the Mussoorie Library I have noticed for some days a small pool of water in the middle of the road just above 'Auchnagie.' It struck me as being something singular; and to-day when passing I noticed several drops of water fall into it; on looking up I saw that it was the sap from a branch high up on a tree that was falling into it; the drops were large and were falling at the rate of one a second. I afterwards noticed several other trees of the same kind on the roadside dropping sap from their branches in the same way. The tree is a large one, called by the natives Kágashi (*Cornus macrophylla ?*). In the spring, if the bark of this tree is wounded by an axe, the sap runs out of the wound in a great stream; some of it solidifies into a thick mucilage of a bright orange colour; it was from a broken branch that the sap was coming, broken most likely by the heavy fall of snow we had at the end of January. These trees are just bursting into leaf, but they have been weeping for the last ten days at least."

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Mr. H. R. Sherren; a Leopard (*Felis pardus*) from Ceylon, presented by Mr. W. Mauger; a Guilding's Amazon (*Chrysotis guildingi*) from St. Vincent, West Indies, presented by Mrs. Ellice; a Pheasant (*Phasianus colchicus*), British, presented by Mr. A. L. Sawer; a Common Toad (*Bufo vulgaris*) from Gloucestershire, presented by Mr. John Scovell; a Chinchilla (*Chinchilla lanigera*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

PROPER MOTION of LL 26481.—Mr. John Tebbutt (*Observatory*, 1887, October, p. 360), having found a considerable discordance between the places given for this star by Lalande and Lamont, asked Mr. Lenehan, the acting Government Astronomer at Sydney, to observe the star with the transit-circle of that Observatory. The result of his observations, as will be seen, shows that the star has a large proper motion in both elements:—

	Mean R.A. 1886·0. h. m. s.	Mean N.P.D. 1886·0. ° ′ ″
Lalande 1800	14 25 1·06	105 6 33·4
Lamont 1850	14 25 2·17	105 6 56·6
Lenehan 1887	14 25 2·67	105 7 14·2

THE WASHBURN OBSERVATORY.—The fifth volume of the Publications of the Washburn Observatory of the University of

Wisconsin, containing the record of the work done during the year and quarter ending April 1887, has recently been published. Pending the appointment of a successor to Prof. Holden, who resigned his position as Director of the Observatory in the winter of 1885, to assume the direction of the Lick Observatory, Dr. Davies, Professor of Physics in the University of Wisconsin, has had general charge of the Observatory, to which of course he has been able to devote only a limited amount of time and attention. It is no doubt owing to this circumstance that no very definite programme of work has been carried out since the completion of the observations and reductions necessary for clearing off the list of 303 fundamental stars undertaken by Prof. Holden for the *Astronomische Gesellschaft*. The assistants, Mr. Updegraff and Miss Lamb, have been principally employed in observations of fundamental stars for determination of latitude and discussion of the instrumental errors of the meridian-circle and of refraction. The large equatorial has been used for measures of double stars, observations of Sappho and of Comet *b* 1887 (Brooks). A considerable portion of the volume is devoted to an index of the stars occurring in Airy's Greenwich Catalogues, not found in Flamsteed, which has been prepared by Miss Lamb. This index, which will be very useful to practical astronomers, has been prepared according to the general plan suggested by Argelander in his review of the Greenwich second seven-year Catalogue (*Vierteljahrsschrift*, Bd. vi. Heft 2), and contains some 3000 entries. The computer is thus saved the labour of looking through all six catalogues when searching for a star which is tolerably sure to be in one of them, and as the catalogues are reduced to six different epochs, the labour attending such a search would be considerable. The star places in Miss Lamb's index are reduced to 1875·0.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *OCTOBER* 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 October 16

Sun rises, 6h. 27m. ; souths, 11h. 45m. 39·0s. ; sets, 17h. 4m. ; decl. on meridian, 8° 52′ S. : Sidereal Time at Sunset, 18h. 44m.

Moon (New on October 16, 23h.) rises, 5h. 22m. ; souths, 11h. 27m. ; sets, 17h. 21m. ; decl. on meridian, 2° 20′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	8 41	13 7	17 33	18 17 S.
Venus	3 37	9 40	15 43	0 9 S.
Mars	1 28	8 36	15 44	12 24 N.
Jupiter	8 8	12 57	17 46	14 21 S.
Saturn	23 6*	6 54	14 42	19 10 N.

* Indicates that the rising is that of the preceding evening.

Oct.	h.	
18	1	Jupiter in conjunction with and 4° 19′ south of the Moon.
18	9	Mercury in conjunction with and 7° 39′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Oct. 18,	4 11	*m*
Algol	3 0·8	40 31 N.	,, 21,	5 43	*m*
ζ Geminorum	6 57·4	20 44 N.	,, 20,	0 0	*m*
U Monocerotis	7 25·4	9 33 S.	,, 16,		*m*
U Coronæ	15 13·6	32 4 N.	,, 17,	4 15	*m*
R Scorpii	16 10·9	22 40 S.	,, 20,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 17,	3 57	*m*
			and at intervals of	20 8	
X Sagittarii	17 40·5	27 47 S.	Oct. 20,	0 0	*m*
W Sagittarii	17 57·8	29 35 S.	,, 17,	0 0	*m*
T Herculis	18 4·8	31 0 N.	,, 19,		*m*
β Lyræ	18 45·9	33 14 N.	,, 21,	0 0	m_2
R Lyræ	18 51·9	43 48 N.	,, 16,		*m*
η Aquilæ	19 46·7	0 43 N.	,, 20,	1 0	*m*
R Sagittæ	20 8·9	16 23 N.	,, 16,		*m*
S Delphini	20 37·9	16 41 N.	,, 17,		*m*
T Aquarii	20 44·0	5 34 S.	,, 17,		*m*
δ Cephei	22 25·0	57 50 N.	,, 18,	1 0	*m*

M signifies maximum ; *m* minimum ; m_2 secondary minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ξ′ Ceti	31°	8° N.	Slow ; with trains.
,, ν Orionis	90	15 N.	Swift ; streaks.
,, δ Geminorum	108	23 N.	Swift ; streaks.
,, κ Cephei	308	78 N.	Slow ; faint.

METEOROLOGICAL NOTES.

WE have just received the "Results of Meteorological and Magnetical Observations" made at Stonyhurst College during 1886, which maintains well the high character of previous issues. Pressure, at a height of 381 feet, fell, on December 8, to 27·350 inches, being absolutely the lowest hitherto noted at Stonyhurst. On the other hand, no excessively high readings occurred during the year, the maximum of each month being in each case topped by higher readings in previous years. For eight of the months temperature was under the normal, the deficiency being, in February, 5°·2 ; December, 4°·4 ; January, 3°·4 ; and March, 2°·3 ; and for the year, 1°·5. In October, temperature was 2°·9 above the average of the month. The annual rainfall was 5·18 inches above the mean, a result largely due to the heavy rains of January and May ; but in February and August the fall was much less than usual. The actual sunshine for the year was 30 per cent. of the possible sunshine, the highest monthly percentage being 40 in April, and the lowest 20 in January. The maximum sunshine for the day occurs from 10 a.m. to 3 p.m., the absolutely highest hour being from 1 to 2 p.m. Among the more special meteorological work undertaken by the College are observations of cirrus clouds, upper glows, and trees, shrubs, and flowering plants. For the fourth year an appendix is given, exhibiting with some fullness the results of the observations made at St. Ignatius's College, Malta, by the Rev. J. Scholes.

THE Meteorological Commission of Cape Colony have published their Report for the year 1886, containing summaries of observations taken at 28 stations, and rainfall observations at 268 stations, including 3 in Basutoland and 11 in Orange Free State. At the Royal Observatory the absolute maximum temperature in a Glaisher stand was 104°·0 in January, and the minimum 34°·5 in August. In a Stevenson screen the maximum was 101°·0, and the minimum 35°·0 (in July), plainly showing the effect of radiation on the former readings. The total rainfall measured on 77 days was 27·79 inches, of which 7·68 inches fell in June. Self-recording anemometers are erected at the Royal Observatory, Port Elizabeth, and East London, but no arrangements are made for reducing the curves. The Commission draw special attention to the interesting series of thirteen rainfall maps appended to the Report. These have been reduced from maps exhibited by Mr. Gamble at the Indian and Colonial Exhibition, and represent the average rainfall of the colony for each month and for the whole year.

IN the *Annalen der Hydrographie und maritimen Meteorologie* for September, Capt. D. Ruete gives the results of eighteen years' experience of the typhoons of the China seas, accompanied by rules for manœuvring in different cases, and by charts of the tracks of the typhoons in various seasons. The distinguishing features of the typhoon are its small diameter and its sharply-defined central calm, as compared with ordinary rotatory storms. They occur most frequently in August and September, less frequently in July and October, and more rarely still in May, June, and November. They do not extend into the higher regions of the atmosphere ; Knipping assumed a height of four nautical miles for the severe September typhoon of 1878. Their form is generally oval, but dependent on the contour of the coasts. The author finds that they travel slower when south of 30° N. than they do when north of that position, and that the faster they travel and the greater the area of the storm field, the more severe the typhoons are, and that the duration of the fall of the barometer gives no clue to the magnitude of the area of the storm, and further, that an approaching typhoon is shown less by the amount of the fall than by the general behaviour of the instrument. The regions most frequented by the typhoons extend from lat. 10° N. to 38° N. in the China seas, and to 50° N. in the Japan seas, and may be divided into four different seasons and districts, which are separately discussed in the paper.

At the meeting of the International Meteorological Committee at Paris in 1885, Dr. H. Wild, of St. Petersburg, and Prof. E. Mascart, of Paris, were requested to take the initiative in the publication of a series of international meteorological tables, to allow of a uniform method of reduction of the observations being everywhere carried out, and a specimen has now been printed and issued for criticisms. The work contains (1) a description of the tables and of their use, in English, French, and German; (2) equivalents of various units, and general tables used by different countries for meteorology, magnetism and electricity. The volume will occupy nearly 440 pages quarto, and will cost about 30*s*. The Committee request each country to contribute to the expense of publication by subscribing for a certain number of copies.

We have received the results of meteorological observations made in New South Wales during 1885, under the direction of H. C. Russell, B.A., F.R.S., the Government Astronomer, forming a compact octavo volume of 360 pages, with plates of meteorological curves for each month. The great increase in the number of rainfall and river stations has made it necessary to publish these observations in a separate volume. Some important additions have been made to the information given in past years, and the barometric curves have been so arranged as to show the various changes through all the stations at one view. The tables show (1) the most important data for the whole colony; (2) the data for each station separately; (3) the barometric curves; and (4) the monthly abstracts published during the year. Considerable attention has been paid to the amount of evaporation in various parts of the colony, and the quantities for some districts are found to be much below what has often been stated, depending very much upon the state of the soil. The year 1885 was one of serious drought, although the total rainfall for the year was only 10 per cent. below the average, owing to heavy rainstorms at the end of January. The total rainfall at Sydney was 39·78 inches measured on 145 days. The greatest fall was in June, and the least in August. The mean temperature for the year was 63°·9. The maximum in the shade was 97°·6 on November 5, and the minimum 40°·6 on June 30. The mean temperature of the whole colony for 1885 was 63°·0, or 1°·7 in excess of the mean for 15 years. The absence of cloud, and the drought, have had a decided effect upon the temperature of the whole colony, showing apparently a direct relation between rainfall and temperature.

THE PROBLEM OF THE HOP PLANT LOUSE (PHORODON HUMULI, Schrank) *IN EUROPE AND AMERICA.*[1]

The author has been for several years carrying on investigations with a view of ascertaining the full annual life-history of *Phorodon humuli*, and especially with a view of settling the hitherto moot question as to its winter life. The importance of the inquiry, both from the economic and the scientific sides, is self-evident. The hop crop, in all parts of Europe where it is grown, and especially in England, annually suffers more or less from the ravages of this its worst insect enemy, and in some years is a total failure. The same is true in North America, at least east of the Rocky Mountains, and last year the injuries of this Phorodon in the hop-growing regions of the State of New York were so great that many hop yards were abandoned and have since been ploughed up; while but 10 per cent. of an average crop was harvested. From the purely scientific side, entomologists, notwithstanding the great interest attaching to the subject, have been divided in opinion as to the identity, or specific relationship, of the hop Phorodon and one that occurs on Prunus, while the complete annual cycle of the insect's life has remained a mystery. After full and satisfactory investigations I have satisfied myself that, contrary to the prevailing impression among hop growers and previous investigators, the hop plant louse does not hibernate underground on the roots of the hop, nor in, on, or about anything in the hop yard; but that, upon the advent of the first severe frosts, the hop plant and the hop yards are entirely cleared of the species in any form. I find that all statements to the contrary in America are based on misapprehension, or mistaken identity of species, and I believe (though admitting the possibility of variation in this respect in milder climates) that the same will be found to hold true in England, where hibernation on the hop root has been accepted by high authority. The positive statements made about eggs being laid in autumn, whether on roots or upon the vines left in cutting, or which are carted away, are based on conjecture, and have been blindly copied without credit by one writer from another, a practice too common among second-hand writers on economic entomology.

[1] Abstract of a Paper read before Section D of the British Association for the Advancement of Science, at Manchester, by Prof. C. V. Riley, on September 3.

The conjectures of some of the best students of aphidology that *Phorodon humuli* had a form (*malaheb*, Fonsc.) living on Prunus, and that there was a consequent migration from one plant to the other, I have positively proved to be correct, by direct colonizing from Prunus to Humulus, and by continuous rearing from the original stem-mother hatched from the winter egg.

The observations have been made on growing plants and in vivaria at Washington, and checked by others made simultaneously in hop yards at Richfield Springs, N.Y. An incident may here be recorded as illustrating the effect of meteorological extremes upon Aphides. The extreme heat (over 100° F.) and dryness of July 17 and 18 killed every one of the insects under observation at Washington, entirely clearing the plants. The economic bearing of such exceptional phenomena, as also of the biological observations made, is readily understood.

The more important conclusions from the studies so far made are thus summed up in a paper which I had the honour to read before the American Association at its recent meeting in New York:—

(1) *Phorodon humuli* hibernates in the winter egg state, this egg being fastened to the twigs (generally the previous year's growth) of different varieties and species of Prunus, both wild and cultivated. The egg is difficult to detect, because it is covered with particles which resemble the bark in colour and appearance. It is usually laid singly, and when freed of disguising particles is seen to be ovoid and 0·04 mm. long.

(2) The annual life-cycle is begun upon Prunus by the stem-mother, which hatches from this winter egg. The stem-mother is stouter than the individuals of any of the other generations, with the legs, antennæ, and honey-tubes relatively shorter, while the cornicles between the antennæ are sub-obsolete. The colour is uniform pale green, with bright red eyes and faintly dusky tarsi.

(3) Three parthenogenetic generations are produced upon Prunus, the second at once distinguished by its more elongate form, much longer members, distinct cornicles and markings of darker green; while the third (or typical *malaheb* form) becomes winged, and instinctively abandons the plum and migrates to Humulus. The habit of moving from plant to plant after giving birth to an individual, and thus scattering the germs of infection on Humulus, is well marked in this winged generation.

(4) During the development of the three plum-feeding generations, the hop is always free, and subsequently, until the return migration, the plum becomes more or less fully free from infection by this species.

(5) A number of parthenogenetic wingless generations are produced on the hop (seven, or the tenth from the stem-mother on plum, having been traced up to August 5, and advices of the eleventh, up to August 19, have been received since my arrival in England); and, finally, there is a return migration of winged females to the plum in autumn. The wingless hop generations are not only incapable of migrating to the plum, but do not thrive upon it when artificially transferred thereto.

(6) Exact observations are not yet complete as to the full number of generations produced upon the hop before the winged return migrant appears, and another month's careful watching and experiment is needed to fill this hiatus in the annual cycle, as also to ascertain the exact number of generations produced in autumn on the plum. From knowledge extant and previous general observation, the facts will probably prove to be as follows:—

(7) The eleventh or twelfth generation will produce winged females (from the middle to the end of August), which will deposit their young upon the plum, and these will become the only sexed individuals of the year, the male winged and the female wingless, the latter after coition consigning a few impregnated or winter eggs to the twigs.

(8) Up to August 5 the first females on hop were still alive and breeding, having existed two months. There is, consequently, an increasing admixture of generations from the first

on hop until frost overtakes the species in all conditions and sweeps from the hop yards all individuals alike, perpetuating in the egg state those only which reached the sexual condition on the plum.

(9) Each parthenogenetic female is capable of producing on an average 100 young (the stem-mother probably being more prolific) at the rate of one to six, or an average of three per day, under favourable conditions. Each generation begins to breed about the eighth day after birth, so that the issue from a single individual easily runs up, in the course of the summer, to trillions. The number of leaves (700 hills, each with two poles and two vines) to an acre of hops, as grown in the United States, will not, on the average, exceed a million before the period of blooming or burring; so that the issue from a single stem-mother may under favouring circumstances blight hundreds of acres in the course of two or three months.

(10) While meteorological conditions may materially affect the increase and power for injury of the species, these are far more truly predetermined and influenced by its natural enemies, many of which have been studied and will be described.

(11) The slight colorational differences, as also the structural differences, including the variation in the cornicles on head and basal joints of antennæ, whether upon plum or hop, are peculiarities of brood, and have no specific importance whatever.

(12) The exact knowledge thus gained simplifies the protection of the hop plant from Phorodon attack. Preventive measures should consist in destroying the insect on plum in early spring where the cultivation of this fruit is desired, and the extermination of the wild trees in the woods wherever the hop interest is paramount; also in avoiding the introduction of the pest into new hop countries in the egg state upon plum cuttings or scions. Direct treatment is simplified by the fact that the careful grower is independent of slovenly neighbours, infection from one hop yard to another not taking place.

Experiments still under way have shown that there are many effective remedies, and that the ordinary kerosene emulsion diluted with twenty-five parts of water and sprayed with the cyclone nozzle, or a soap made by boiling one pound of pure potash in three pints of fish oil and three gallons of water, and this dissolved in eight gallons of water, and sprayed in the same way, are thoroughly effectual remedies, and leave the plant uninjured. The former costs 75 cents, the latter 30 cents, per acre, plus the time of two men for three hours, plus appliances. The object of further experimentation now being carried on is to simplify and reduce the cost of these last to a minimum. As they consist, however, essentially of a portable tank and a force-pump with hose and spraying attachment, which, together, need not involve a greater first outlay than 5 dollars to 10 dollars, and as every American hop grower can afford to expend the larger sum for the protection of a single acre, there is no longer any excuse for allowing this pest to get the better of us.

It is not my purpose, however, to enter into aphidicide details in this communication, which I will conclude by brief reference to the bearings of these discoveries in America on the problem in Great Britain. The most comprehensive and satisfactory review of the knowledge possessed on the subject in England that has come to my notice, is that by my esteemed friend and correspondent, Miss Eleanor A. Ormerod, Consulting Entomologist of your Royal Agricultural Society, in her "Report of Observations of Injurious Insects," &c., made in 1885. So far as her own careful observations are concerned, they fully accord with the facts here set forth; but on the authority of others, and especially on the evidence of Mr. C. Whitehead, who reported finding young lice and large viviparous females on hop shoots as early as March 29, and that of Mr. A. Ward, who experimented with surface dressings near Hereford, Miss Ormerod concludes that attack on the hop begins in spring from wingless females which come up from the hop hills, and, as a corollary, that dressings to prevent such ascent are strongly to be recommended. It is quite within the range of possibility, and what is known of aphid life, that where the winters are mild, with scarcely any frost, this Phorodon may continue on the hop from one year to another in the parthenogenetic condition. If such is ever the case in England, you have a somewhat different set of facts to deal with here from what we have in America. But for the reasons already stated in abstract, from many other detailed observations which it would be tedious to record here, as well as from the ease with which erroneous conclusions are arrived at in entomological matters of this kind, where not checked and proved by the most competent and careful study, I shall be inclined to believe that the facts in England are essentially the same as I have found them in America, until convincing and trustworthy evidence to the contrary be forthcoming. Mr. Whitehead may have had another species under observation, and Mr. Ward's surface dressings may have acted by repelling the winged female migrating from Prunus, in the same way that buckwheat sown among the hops is believed to do with us.

EDUCATION IN GERMANY.

THE following letter appeared in the *Times* on the 10th inst.:—

SIR,—I should be glad if you could find space in your columns for some remarks on the state of education in Germany. I have just completed a short tour in this country, mainly to inquire into its educational system, especially with reference to primary and technical schools. England has at last roused herself to the necessity for technical education.

The Bill, which was unhappily crowded out last session, will be reproduced next year, and, I trust, expanded to larger dimensions. It will contain, I hope, a clause for the establishment of evening continuation schools, for which object I gave notice of an amendment last session. My trip to Germany has been chiefly taken to learn what is doing here in this direction, and what is the drift of educated German opinion. With your permission I will briefly summarize my impressions. I premise by observing that each State of the German Empire manages its own education, and that the laws and regulations differ somewhat, so that general statements referring to all Germany cannot be made without qualifications. I will not weary your readers, however, by going into details respecting each State, but place broadly before them the general features of German education.

The salient fact which strikes all observers is the universality of good education in this country. There is no such thing as an uneducated class; there are no such things, speaking broadly, as neglected and uncared-for children. All classes of the community are better educated than the corresponding ones in our country; and this applies quite as much to primary as to secondary education. Nothing struck me more than the general intelligence of the humbler working classes. Waiters, porters, guides, &c., have a knowledge of history, geography, and other subjects far beyond that possessed by corresponding classes in England, and the reason is not far to seek. The whole population has long been passed through a thorough and comprehensive system of instruction obligatory by law and far more extended than is given in our elementary schools. I went through several of these schools and observed the method of teaching, which was simply admirable. The children are not crammed, but are taught to reason from the earliest stages. The first object of the teacher is to make his pupils comprehend the meaning of everything they learn, and to carry them from stage to stage so as to keep up an eager interest.

I saw no signs of weariness or apathy among either teachers or scholars. The teaching was all *vivâ voce*, the teacher always standing beside the blackboard and illustrating his subject by object-lessons. The instruction was through the eye and hand as well as the ear, and question and answer succeeded so sharply as to keep the whole class on the *qui vive*. The teachers are, as a body, much better trained than in England, and seem to be enthusiasts in their calling, and the school holds a far higher position in the social economy of the country than it does with us. What I am saying here applies equally to Switzerland as to Germany, and for educational purposes Zurich will compare with any part of this empire. The main advantage, however, that primary education has in Germany over England lies in the regularity of attendance and the longer period of school life. There is none of the difficulty of getting children to school that exists in England; the laws are very rigid and permit no frivolous excuses, and, what is even more important, the people entirely acquiesce in the laws, and are inclined rather to increase than relax their rigour. It is well known that in London and all our great cities a large part of the population seek to avoid school attendance by every means in their power, and consequently the attendance is most irregular. There is very little of this in Germany; at least I have not found it so. Then in our country a great portion of our children are withdrawn altogether from school, after passing the fourth or fifth standard, at the age of eleven or twelve, whereas in Germany almost everywhere attendance is compulsory until fourteen for boys, though in some places girls are allowed to leave at thirteen.

This last point is the one I wish to emphasize. The great defect—I might almost call it the fatal defect—of our system is that it stops just at a time when real education should begin. It allows a child to leave school at an age when its learning is soon forgotten and its discipline effaced. It is hardly too much to say that the two years' additional training the German child receives in the elementary school, doubles its chance in life as compared with the English child.

But this is not all. The Germans are rapidly developing a system of evening continuation classes which carry on education for two or three years longer. In Saxony the boys who leave the primary school, if they do not go to the higher schools must attend for three years longer—say, until they are seventeen—continuation classes for at least five hours per week. But teaching is provided for them, and they are encouraged to attend, twelve hours per week. So complete is this system that even the waiters at the hotels up to the age of seventeen attend afternoon classes, and are taught one or two foreign languages. I take Saxony as one of the most advanced States; but the law is much the same in Würtemburg and Baden, and the system is found to work so well that it is in contemplation to extend it to all the States in the German Empire, and Austria will probably follow suit. This is confidently expected to happen in the course of 1888. I may state as an undoubted fact that in Germany and Switzerland, and I believe in some other Continental countries, the opinion is ripening into a conviction that the education, even of the poorest class, should be continued in some form or another to the age of sixteen or seventeen. They find by experience that wherever this is adopted it gives an enormous advantage to the people in the competition of life, and, above all, trains them to habits of industry and mental application. I believe it is owing to this system of thorough education that Germany has almost extinguished the pauper and semi-pauper class, which is the bane and disgrace of our country.

Wherever I have gone I have inquired how they deal with the ragged and squalid class of children, and I have been told in every city I visited—in Zurich, Stuttgart, Nuremberg, Chemnitz, Dresden, and Berlin—that such a class practically does not exist. I do not mean that there is not poverty and plenty of it in Germany. Wages are much lower than in England, and many have a hard struggle to live. But there does not seem to exist to any extent that mass of sunken, degraded beings who with us cast their children upon the streets, or throw them on the rates, or leave them to charity. Some half-a-million of children in the United Kingdom are dependent, more or less, on the alms or the rates of the community, and probably another half-million are miserably under-fed and under-clad. Nothing to correspond with this exists in Germany. The poorest people here would be ashamed to treat their children as multitudes do with us. Indeed, I have not seen since I left home a single case of a ragged or begging child. I repeat that the great cause of this both in Germany and Switzerland is the far greater care they have taken of the education of the children for at least two or three generations, whereas we have only taken the matter up seriously since 1870, when Mr. Forster's great Act was passed.

Let us contrast the general condition of our London children, for instance, at the age of fifteen or sixteen, with that of the same class in Berlin or Dresden or Chemnitz. With us nine-tenths of the children have long since left school, and a too large proportion of them are receiving no training but the coarse and brutalizing education of the streets. Most of them retain little of what they have learnt at school, except the power to read the "penny dreadful," which stuffs their minds with everything a child should not know. They are to a very large extent adepts in profane and obscene language, and are frequenters of the public-house, the "penny gaff," and such like amusements; a great many of them are learning no useful trade or calling, but are drifting helplessly into the class of wretched, ill-paid, casual labourers. Very many of them marry before they are twenty, and are soon the parents of a numerous progeny, half-starved and stunted both in body and mind. Compare, or rather contrast, this with Germany. At fifteeen or sixteen a great part of the children are still under excellent instruction. Exceedingly few are to be found roaming about the streets. They are prohibited, at least in some parts of Germany, from entering the public-houses (except with their parents) until the age of seventeen, and I am told are everywhere prohibited from smoking until sixteen. In fact, there are, both by law and public sentiment, barriers placed against the corruption of the young which do not exist in England.

No country has ever suffered more from the abuse of the idea of individual liberty than England has done. Owing to this overstrained idea we did not get compulsory education until long after the advanced nations of the Continent, and still we are far behind them in the care we take of our children. It is intolerable that this state of things should continue longer. Democratic government everywhere insists upon good education, and expects each citizen to fulfil his duties to the State.

Public opinion in our country will certainly insist, and that before long, that we shall not be for ever disgraced with a residuum of the most drunken, demoralized, and utterly incapable population to be found in any modern State. It will insist that some time be spared for the solution of this vital question from the wrangles of party politics and the party recriminations of party leaders. When one sees what a poor country like Germany has done to raise its people in spite of the conscription and three years' compulsory military service, in spite of frequent and exhausting wars from which our island home has been free, one has grave doubts whether our system of party government is not a failure.

Certainly we waste on barren conflicts and wordy strife far more time than other nations do in the conduct of their affairs. They direct their energies with business-like precision to supply the exact needs of the people, we fritter away our enormous political energy in fruitless party contests which every year degrade Parliament lower and lower, and make it less and less fit for the practical work of governing the nation.

One thing seems certain. Unless we can give more attention to the vital questions which concern the welfare of the masses our country must go down in the scale of nations. No honest observer can doubt that in many respects the Germans are already ahead of us, and they are making far more rapid progress than we are. They are applying technical science to every department of industry in a way that Englishmen have little idea of. Their polytechnics and their practical technical schools are far ahead of anything we possess in England, the leaders of industry are far better trained, the workmen are better educated and far more temperate and thrifty than ours are. Wherever the Germans and English are coming into competition upon equal terms the Germans are beating us. This is not because the Germans have greater natural power. I believe the British race is the more vigorous naturally. But they are organized, disciplined, and trained far better than we are. They bring science to bear upon every department of the national life, whereas we, up till lately, resented all State interference, and so exaggerated the doctrines of freedom as almost to glory in our abuses.

There is much more that I might say if space permitted; but it will not do to trespass further on your indulgence. I will only add in conclusion that England must wake up, and that immediately, to the necessity of a far more thorough and practical system of education, else she will lose the great place she has hitherto held in the world's history.

I am, Sir, yours faithfully,

Berlin, October 4. SAMUEL SMITH.

THE BRITISH ASSOCIATION.

SECTION B—CHEMICAL SCIENCE.

The Atomic Weight of Gold, by J. W. Mallet, F.R.S.—Attention is called to the importance of correct determinations of atomic weights by different experimenters, and especially the elimination of "constant errors." Considering the desirability that all such values should be connected as directly as possible with hydrogen, a method is described by which this may be done in the case of gold. A known weight of zinc is dissolved in dilute sulphuric acid, and the hydrogen evolved is measured. A solution of bromide or chloride of gold is then treated with zinc more than sufficient to precipitate the whole of the gold, the residual zinc being determined by the hydrogen evolved on treatment with sulphuric acid. The difference in volume of hydrogen obtained gives a direct means of calculating the atomic weight of gold. The author described various experimental precautions that had been taken in measuring the gas.

The Atomic Weight of Zirconium, by Dr. G. H. Bailey.—The previous determinations of the atomic weight of this element were made by Berzelius (89·25), Hermann (88·8), Marignac (90·54). The earlier results were doubtless vitiated by the presence of iron and of the cerite earths, whilst Marignac's determination is open to objection from the character of the salt (potassium zirconium fluoride) which he used. In the present determination zirconia was prepared from North Carolina zircons by three independent methods. It was dissolved in sulphuric acid, and the sulphate was crystallized out. This salt becomes normal and constant in weight by heating some hours at 400°, the temperature at which it begins to decompose being 470°. The relation of zirconium sulphate to zirconia gives a ratio from which the atomic weight is calculated, and, though the work is not complete enough to state the result with accuracy, the value obtained agrees more nearly with that of Marignac. The author proposes to make further determinations, using the tetrabromide.

Torsion Balances, by Dr. A. Springer.—Light frames are made and stiffened by wires or flat bands being tensioned over them. The beam is then firmly clamped to the bands in such a manner that its centre of gravity is above its point of support; this tends to tip the beam, thus equilibrating the torsional resistance of the fulcrums. We thus have the torsional resistance exerted to keep the beam horizontal, and the high centre of gravity tends to tip it out of the horizontal. The adjustment of the position of the centre of gravity is most easily made by having an adjustable poise placed immediately above the centre of the torsional wire. In order to do away with the necessity of alignment of support, a secondary beam is attached to the first in such a manner that both beams tending to tip in the same direction remain stationary owing to their having opposite and equal moments. On this principle scales are constructed which can be used on rolling ships, or in buildings where there is considerable jarring. In all the "torsion balances" there is permanence of adjustment, consequently repeated weighings will give like results. Various "torsion balances" were shown illustrating the principle involved, as well as to show how equal sensitiveness can be obtained with any load.

Integral Weights in Chemistry, by T. Sterry Hunt.—The author maintains the necessity for taking hydrogen as standard of specific gravity, not only for gases, but also for liquids and solids, and thinks that such considerations lead to a comprehension of the physical properties and chemical constitution of chemical substances.

Action of Light on the Hydracids of the Halogens in presence of Oxygen, by Dr. A. Richardson.—HCl and O were mixed in different proportions in bulbs, and exposed forty to sixty days in sunlight, at the end of which time their contents were examined. Free Cl was only noticeable when large excess of O was present, and when the gases had not been dried. Similar experiments were made in the cases of HBr and HI.

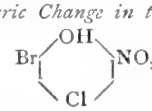
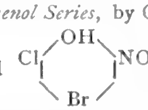
The Present Position of the Alkali Trade, by A. E. Fletcher.—The author, in presenting this report, remarked that no report on this subject had been presented to the Association since that of his predecessor in office, Dr. Angus Smith, in 1861. Tracing the general history of the alkali manufacture, he noticed the various improvements which have been carried out in the mechanical details and in the chemistry of the processes, with special reference to the recovery of by-products. The rapid growth of the alkali trade in Germany was shown by a reference to the exports and imports of the product. In Germany this advance is largely due to the adoption of the ammonia-soda process, a process which in this country has been taken up thus far by only three firms. It was pointed out, however, that the Leblanc process, with certain modifications, was not by any means a forlorn hope; and the improvements now being worked out by Messrs. Chance, Messrs. Parnell, and Messrs. Gaskell, Deacon, and Hurter were referred to.

On the Constituents of the Light Oils of Blast-Furnace Coal Tar from the Garthsherrie Works, by Watson Smith.—These tars are recovered from the condensation of the gases evolved from a blast-furnace, condensed by means of the process of Messrs. Alexander and McCosh. The various constituents are described, amongst others toluene and xylene, the latter consisting of about 70 per cent. metaxylene. Considerable interest was manifested in a specimen of flannel dyed by xylidine scarlet prepared from this metaxylene, being the first dye product ever prepared from the by-products of the blast-furnace.

A New Apparatus for condensing Gases by contact with Liquids, by Prof. Lunge.—The apparatus described is of a construction of the "plate column" style, and consists, as the name implies, of a series of perforated plates arranged in column, and made of stoneware. The gases as they rise are thus brought into immediate contact with an extensive plane surface of the absorbing liquid. The cooling is not so rapid as with coke towers and similar arrangements, but it is to be borne in mind that the gases pass more slowly owing to the completeness of the contact with liquid. Comparative results were given showing the working relation between this apparatus and others.

The Extent to which Calico Printing and the Tinctorial Arts have been affected by the Introduction of Modern Colours, by Charles O'Neill.—The author reviewed the state of things in 1856, at which time Perkin's aniline mauve was discovered, and showed the effect of the introduction of alizarine colours. It was shown that the demand had not kept pace with the production, and this had much to do with the unremunerative condition of calico printing.

The Chemistry of Cotton Fibre, by F. H. Bowman.—It is shown that cotton, in common with all vegetable substances, has for its base cellulose. The opinion is here expressed that, judging from the results of combustion, it consists of a series of bodies not agreeing in all respects with one another. The mineral matter of cotton amounts to about 1 per cent., and this is considered to form an essential constituent.

Isomeric Change in the Phenol Series, by O. R. Ling.—The isomers  and  were stated to have been prepared. The melting-point of the former was found to be 125°, and its calcium salt contained 2½ molecules of water, whereas the melting-point of the latter was 117°, and its calcium salt contained 7 molecules of water.

On Methylene Blue and Methylene Red, by Prof. Bernthsen.—In this paper it was shown how from thiodiphenylamine, by respective nitration, reduction, and oxidation, Lauth's violet (thionine) was obtained. Methylene blue was described as tetramethylthioninechloride.

On the Constitution of Azimido Compounds, by Drs. Noelting and Abt.—The formulæ given by Griess and by Kekulé for the azimido compounds were explained and discussed, and it was shown that the evidence was in favour of the formula of Kekulé.

Velocity of Formation of Acetic Ether by Prof. Menschutkin.—In these experiments acetic anhydride was used instead of the acid by which in his previous work the process of etherification had been effected. It was found that the velocity of the reaction was greater with the primary than with the secondary alcohols, and that it was much slower in the higher members of an homologous series than in the lower. A distinct and marked difference occurs between isomers as shown by the examination of butyl and isobutyl alcohols. By working in solution the reaction proceeds more regularly, and the results agree better amongst themselves. The author finds that the nature of the solvent affects the course of the reaction in such a manner as to throw some doubts on the hypothesis of Guldberg and Waage.

The Relation of Geometrical Structures to Chemical Properties, by Prof. Wislicenus.—This memoir—probably the most important contribution that has yet been made to the question of the constitution of unsaturated organic compounds—cannot readily be understood without the accompanying diagrams. The results have already been published in the Transactions of the Royal Saxon Academy.

Note on Valency, by Prof. Armstrong.—An attempt to explain the phenomena of valency according to the views expressed by Helmholtz as to the unit charge of the elements and its distribution.

The Solubility of Isomeric Organic Compounds, by Prof. Carnelley.

(1) For any series of isomeric organic compounds the order of solubility is the same as the order of fusibility, *i.e.* the most fusible compound is also the most soluble. This is shown to hold true in a very large number of cases, whilst there are very few exceptions, and those of a doubtful character.

(2) The order of solubility of two or more isomeric compounds is independent of the nature of the solvent. This has been experimentally proved, more particularly in the case of meta- and para-nitraniline, the solubility of each of which in thirteen different solvents has been determined. Also by a considerable number of cases taken from literature.

(3) The ratio of the solubilities of two isomers in a given solvent is constant, and is therefore independent of the nature of the solvent. So that—

$$\frac{\text{Solubility of A in any solvent}}{\text{Solubility of B in the same solvent}} = \text{constant.}$$

This has been proved for meta- and para-nitraniline in respect of thirteen different and very varied solvents.

Alcohol and Water Combinations, by Prof. Mendeléef.—The author looks upon solution as implying a definite chemical combination liable to alteration and to decomposition at ordinary temperatures. He has also examined the phenomena at low temperatures where cryohydrates are formed, and finds the same agreement with his adopted formula. At low temperatures he has obtained solid compounds of alcohol and water containing 17·56, 46·04, and 88·46 per cent. of alcohol respectively. The investigations have been extended to various mineral acids and salts without discovering any exception to the generalized expression given by him.

On the Constitution of Atropine, by Prof. Ladenburg.—This body is shown to belong to the class of bodies known as alkines. The reactions of tropine and its transformations show that it stands in a direct and simple relation to atropine. It has not yet been possible, however, to synthesize tropine.

The Reduction-products of the Nitro-paraffins and Alkyl Nitrites, by Prof. Dunstan and T. S. Dymond.—The authors have studied the reduction of ethyl nitrite, using ferrous hydroxide as the reducing agent. More than two-thirds of the nitrogen of the ethyl nitrite is liberated in the form of gas, either nitrous oxide or nitrogen; the remainder appears partly as ammonia and partly as ethylamine. If potassium hydroxide is mixed with the ferrous compound a considerable quantity of potassium hyponitrite is formed. It is also probable that ethyl hyponitrite, a compound that has not yet been prepared, may also be formed as an intermediate compound. The authors are further investigating the change with the object of isolating this compound and of discovering the mode of formation of the ethylamine. The reducing action of ferrous hydroxide on the nitro-paraffins was also partially investigated with interesting results.

On a Partial Separation of the Constituents of a Solution during Expansion by Rise of Temperature, by J. W. Mallet, F.R.S.—When solutions of colloid substances—some alcoholic, some aqueous—are exposed to cold, and afterwards to a gradually increasing temperature, the expansion in the liquid has been noticed to take place with a partial or entire separation of the substance dissolved in the upper layer, without any deposition of solid matter.

A New Method for Determining Micro-Organisms in Air, by Prof. Carnelley and Thos. Wilson, University College, Dundee.—This is a modification of Hesse's well-known process. It consists essentially in the substitution of a flat-bottomed conical flask for a Hesse's tube. Its chief advantages are: (1) much smaller cost of flask and fittings as compared with Hesse's tubes; (2) very much fewer breakages during sterilization; (3) great economy in jelly; (4) freedom from leakage during sterilization; (5) results not vitiated by aërial currents.

The Absorption-Spectra of Rare Earths, by Dr. G. H. Bailey.—This paper is an examination of the conditions of observation of absorption-spectra with a special view to the recent announcement of the twenty new elements of Krüss and Nilson. The author finds that the strengths of the absorption-bands do not diminish equally in all parts of the spectrum when the liquid is diluted. The presence of nitric acid also effects not only a displacement of the bands, but also an alteration in their relative intensity. It is further pointed out that a record of the strength of the bands in mixtures containing, in some cases, large quantities of samaria and erbia, and in others none, cannot be used as a means of comparison, and deductions drawn from variations of intensity are untrustworthy. Whilst acknowledging that, with due allowance for such factors, some assistance may be gained towards the course of fractionation, the author considers the announcement of new elements quite premature, and only calculated to throw further confusion into this already difficult field of work.

The Absorption-Spectra of the Haloid Salts of Didymium, by Dr. G. H. Bailey.—Bunsen has described certain variations that occur in the absorption-spectra given by crystals of the didymium salts. In this paper are detailed the variations produced in the absorption-spectra of crystalline salts of didymium when examined in polarized light. A comparison of the chloride, bromide, and iodide of didymium has also been made, from which it appears that in the bromide the bands are situated 5λ further towards the red end of the spectrum than in the chloride, whilst the displacement for the iodide is 14λ towards the violet. In the solution of the chloride (or nitrate) the bands have almost the same position as in the crystals of the iodide, whilst the addition of nitric acid causes a displacement of 12λ towards the red. It is proposed to determine how far this displacement of bands is due to the dispersion equivalent of the menstruum, and whether it gives evidence of dissociation in the liquid.

On Solution, by W. Durham.—The theory of solution adopted by the author of this paper was published by him before the Royal Society of Edinburgh in 1878. According to these views solution is due to the chemical affinity of the elements of the substance dissolved for the elements of the solvent.

Thermal Phenomena of Neutralization, and their bearing on the Nature of Solution, by W. W. J. Nicol.—A consideration of the general relationship existing between the heats of formation of various salts in dilute aqueous solutions.

Notes on some peculiar Voltaic Combinations, by C. R. A. Wright, F.R.S., and C. Thompson.—The combinations consist of a class of cells in which one or both of the "plates" is a film of gas condensed on the surface of an electrically conducting solid, which is itself not appreciably acted on. In Grove's gas battery both plates are gas films. Mercury and silver in dilute sulphuric acid opposed to aëration plates of platinum sponge form sulphates, whilst gold dissolves in potassium cyanide under the same conditions. If an acid and alkaline solution be united by means of a wick a considerable current is producible for a short time in the external circuit, but this falls off rapidly in consequence of the development of free hydrogen on the surface of the plate in the acid fluid and of oxygen on the other plate. No direct quantitative proof of this has, however, been given, and there are given in this paper a number of experiments showing that for every milligramme equivalent of silver deposited 11·2 cc. of hydrogen are evolved.

The Present Aspect of the Question of the Sources of Nitrogen in Vegetation, by Sir J. Lawes, F.R.S., and Dr. Gilbert, F.R.S.—An outline of the work of various experimenters in this line of research was given, indicating that, whereas in accordance with the original observations of Boussingault the authors had found that nitrogen was assimilated in part from the atmosphere directly, the results of Berthelot, Frank, and others derived all nitrogen from the soil. These results were criticised, and it was pointed out that the methods pursued failed in that they did not represent normal conditions of growth of vegetation.

Dispersion Equivalents and Constitutional Formulæ, by Dr. J. H. Gladstone.—The difference between the refraction equivalent, as observed for the line "A" and for the line "H" in the spectrum, is called the dispersion equivalent. The refraction equivalent of a compound is in general the sum of the refraction equivalents of the elements of which it is composed, but varies according to the constitution. The same holds for the dispersion equivalent, and it is shown that in some cases differences are observable in this respect where they are not indicated by constitutional formulæ, as, for instance, in the allyl compounds, and in naphthalene derivatives.

On Organic Vanadates, by J. A. Hall.—A series of organic vanadates are described, which can be distilled under reduced pressure without decomposition. They are obtained by the action of an alkyl bromide on silver orthovanadate.

On some New Cinnamic Acids, by Dr. Cohen and Prof Perkin.—A description of a method of obtaining certain substituted cinnamic acids from aromatic aldehydes in cases where, from the nature of the aldehyde, Perkin's reaction cannot be employed. The aldehydo-salicylic acids are converted into ethereal salts, and these are heated with sodium alcoholate, and finally with sodium acetate and acetic anhydride.

The Antiseptic Properties of Metallic Salts, by Prof. Carnelley. —This paper consisted of a description of experiments performed by the author illustrative of further relations between the chemical composition of a substance and its antiseptic properties.

Antiseptic Properties of some Fluorine Compounds, by W. Thomson.—The author describes in this paper the antiseptic properties of sodium fluosilicate. The body is not poisonous, and possesses no smell. Its superiority over chloride of mercury for surgical purposes is claimed.

On the Composition of Water by Volume, by A. Scott.—From various preliminary experiments the author was led to conclude that the relation of oxygen to hydrogen in water was not accurately represented by the numbers 1 : 2. Subsequent experiments gave the ratio 1 : 1·997 with small variation.

On some Vapour Densities at High Temperature, by A. Scott. —A number of vapour densities of inorganic salts of certain elements have been determined by means of the well-known method of Victor Meyer, at a temperature above the melting-point of cast iron. A breaking up of the molecule is shown to occur in the case of iodine, cadmium iodide, mercuric sulphide, mercurous chloride, and mercuric chloride.

On the Estimation of the Halogens and Sulphur in Organic Compounds, by R. T. Plimpton.—The method suggested as a substitute for those already in use is that of introducing the substance into the flame of a Bunsen burner, or into a jet of hydrogen, the products of the combustion being aspirated through an absorbent liquid. In this solution the amount of the halogens or of sulphur can afterwards be estimated.

On the Derivatives and Constitution of the Pyrocresols, by W. Bott and Prof. Schwarz.—The authors describe some derivatives of these bodies, and are led to conclude that the α-pyrocresol and its isomers are anhydrides similar in constitution to diphenyl ether.

Section C—Geology.

On the Mineralogical Constitution of Calcareous Organisms, by Vaughan Cornish and Percy F. Kendall.—In Dr. Sorby's presidential address to the Geological Society in 1879 it was stated that both calcite and aragonite occur in organic structures, and that aragonite fossils are less stable than those of calcite. It appeared probable that carbonic acid has been the agent which effected the removal of the aragonite, but there are no published experimental data to show that it would remove aragonite more readily than calcite. The authors gave an account of the experimental evidence obtained as to the cause of the inferior stability of aragonite fossils as compared with those formed of calcite, with observations on the geological conditions favourable to the removal of aragonite fossils. They then described the work done in following out these observations, and in the examination of certain organisms belonging to groups not yet classified according to their mineralogical constitution.

The Matrix of the Diamond, by Prof. H. Carvill Lewis.—A microscopical study of the remarkable porphyritic peridotite which contains the diamonds in South Africa demonstrates several interesting and peculiar features which are described in detail. It is one of the most basic rocks known, and has a composition which by calculation would belong to a rock composed of equal parts of olivine and serpentine, impregnated by calcite. It is a volcanic breccia, but not an ash or tuff, the peculiar structure being apparently due to successive paroxysmal eruptions. A similar structure is known in *meteorites*, with which bodies this rock has several analogies. The microscopical examination supports the geological data in testifying to the igneous and eruptive character of the peridotite, which lies in the neck or vent of an old volcano. While belonging to the family of peridotites, this rock is quite distinct in structure and composition from any member of that group heretofore named. It is more basic than the picrite porphyrites, and is not holocrystalline like dunite or saxonite. It is clearly a new rock-type, worthy of a distinctive name. The name *Kimberlite*, from the famous locality where it was first observed, is therefore proposed. Kimberlite probably occurs in several places in Europe, certain garnetiferous serpentines belonging here. It is already known at two places in the United States: at Elliott County, Kentucky, and at Syracuse, New York; at both of which places it is eruptive and post-Carboniferous, similar in structure and composition to the Kimberley rock. At the diamond localities in other parts of the world diamonds are found either in diluvial gravels or in conglomerates of secondary origin, and the original matrix is difficult to discover. Thus, in India and Brazil the diamonds lie in a conglomerate with other pebbles, and their matrix has not been discovered. Recent observations in Brazil have proved that it is a mistake to suppose that diamonds occur in itacolumite, specimens supposed to show this association being artificially manufactured. But at other diamond localities, where the geology of the region is better known than in India or Brazil, the matrix of the diamond may be inferred with some degree of certainty. Thus, in Borneo, diamonds and platinum occur only in those rivers which drain a serpentine district, and in Timor Laut they also lie in serpentine. In New South Wales, near each locality where diamonds occur, serpentine also occurs, and is sometimes in contact with carboniferous shales. Platinum, also derived from eruptive serpentine, occurs here with the diamonds. In the Urals, diamonds have been reported from four widely separated localities, and at each of these, as shown on Murchison's map, serpentine occurs. At one of the localities the serpentine has been shown to be an altered peridotite. A diamond has been found in Bohemia in a sand containing pyropes, and these pyropes are now known to have been derived from a serpentine altered from a peridotite. In North Carolina a number of diamonds and some platinum have been found in river sands, and that State is distinguished from all others in Eastern America by its great beds of peridotite and its abundant serpentine. Finally, in Northern California, where diamonds occur plentifully and are associated with platinum, there are great outbursts of post-Carboniferous eruptive serpentine, the serpentine being more abundant than elsewhere in North America. At all the localities mentioned chromic and titanic iron-ore occur in the diamond-bearing sand, and both of these minerals are characteristic constituents of serpentine. All the facts thus far collected indicate *serpentine*, in the form of a decomposed eruptive peridotite, as the original matrix of the diamond.

On the Discovery of Carboniferous Fossils in a Conglomerate at Moughton Fell, near Settle, Yorkshire, by Robert Law and James Horsfall.—After briefly noting the various exposures of the conglomerate, its unconformability with the Silurian rocks, its nature, probable age, and the circumstances which led to the discovery of fossils in it; the authors described the following section exhibited on the south-west side of the Moughton Fell.

	Feet.
a. Scar limestone, of light grey colour and well jointed; layers very distinct in lower parts and almost horizontal, the genus *Bellerophon* being the commonest fossil in the lowest bed of this rock	300 to 500
b. Conglomerate.—Of a bluish-grey colour when newly fractured, and becoming reddish on exposure to the air. The fragments consist of slate, grit, flagstone, and vein-quartz, all apparently derived from Silurian rocks. Fossil shells and corals are common throughout the bed. *Bellerophon*, *Euomphalus*, *Syringopora*, and *Lithostrotion* are the prevailing genera...	1 to 12
c. Lower Silurian slates, of great thickness, having a north-east strike, and a dip of about 65°. The dip and cleavage appear to be on the same plane in this locality.	

The nature and the origin of the stones in the conglomerate were next pointed out; also it was shown that the portion of the bed in which fossils had been found was not more than 200 yards in length, and that it was thickest in the middle, thinning out to the east and west, and at one point could be seen merging into the overlying limestone. Twenty species of fossils were collected from the conglomerate.

Places of Geological Interest on the Banks of the Saskatchewan by Prof. J. Hoyes Panton.—The writer, after referring to some of the marked geological features which characterize the three great prairie steppes of the North-west of Canada, proceeds to describe two localities more particularly, viz. the vicinity of Medicine Hat, situated on the banks of the Saskatchewan, 660 miles west of Winnipeg, and a locality near Irvine Station, 20 miles east of Medicine Hat.

The History and Cause of the Subsidences at Northwich and its Neighbourhood, in the Salt District of Cheshire, by Thos. Ward.—The frequent occurrence of subsidences in the neighbourhood of Northwich makes it desirable to learn their history and cause. Northwich overlies extensive beds of salt. These occupy about three square miles. The first, or "top" rock-salt, lies at a depth of about 50 yards from the surface, and is covered by Keuper marls, and these by the drift-sands and marls. Between the two beds of salt there are 30 feet of indurated Keuper marl. The second, or "bottom" rock-salt, is over 30 yards in thickness. The subsidences which are so destructive in the town of Northwich and the neighbourhood are entirely caused by the pumping of brine for the manufacture of white salt. It was only about 1770, or shortly afterwards, that the first sinking was noticed; since that date, subsidence has gone on very rapidly, and much destruction of property has resulted. Large lakes or "flashes," one of more than 100 acres in area, and of all depths up to 45 feet, have been and are being formed. This peculiar phenomenon of subsidence in the salt-districts is worthy of more consideration than it has hitherto received from scientific men.

The Sonora Earthquake of May 3, 1887, by Dr. T. Sterry Hunt, F.R.S., and James Douglas.—On the afternoon of May 3, 1887, at 2.12 Pacific time (= 120° W. of Greenwich), the first of a series of earthquake movements was felt in the State of Sonora and the adjacent parts of Mexico and the United States, over an area extending from El Paso in Texas on the east to the River Colorado and the Gulf of California on the west, and from the State of Sinoloa on the south as far north as Albuquerque in New Mexico; the extremes in both directions being over 500 miles. It was the fortune of the writers to be at the time at the great copper-mining camp of Bisbee in Arizona, in a narrow gorge of the Mule Pass Mountains, about 5300 feet above the sea, and near the border of Sonora. A violent tremor of the earth, including two sharp shocks, and lasting over ninety seconds, was succeeded at frequent intervals by many minor movements in the next three days, and, less frequently, at least up to May 29. In this part of Arizona solid house-walls, of *adobe* or unburned brick, were cracked or overturned, while huge rocks in the steep mountain gorges rolled down, causing much damage. Fires, perhaps kindled by these in their course, appeared immediately afterwards in various wooded regions in Sonora and Arizona, giving rise to many false rumours of volcanic eruptions. The movement here seemed from south to north. The small town of Bavispe in the Sierra Madre in Sonora was nearly destroyed, many people being wounded and forty-two killed. Opoto suffered in a similar way, and Fronteras to a less extent. The district chiefly affected by the earthquake is, however, for the most part a desert, with some cattle ranches and mining stations. Interesting studies were made by the authors in the valleys, or *mesas*, between the parallel mountain ridges in this region, both in the San Pedro and Sulphur Spring Valleys. The latter, to the east of Bisbee, and stretching north and south about 100 miles, is often 8 or 10 miles wide, and has its lower portion in Sonora. Though without a visible watercourse, water is there generally found at depths of from 10 to 40 feet in the numerous wells sunk at intervals to supply the needs of large herds of cattle. As described by many observers, the surface of this plain was visibly agitated by the first earthquake shock, so that persons were in some places thrown down by the heaving of the soil, which burst open with discharges of water, while the wells overflowed and were partially filled with sand. In the southern part of this valley, for about 7 miles south from the Mexican frontier, the authors found the results of the undulatory movement of the soil apparent in great numbers of cracks and dislocations. For distances of several hundred feet, along some lines with a generally north and south course, vertical downthrows on one side of from 1 foot to 2 feet and more were seen, the depressed portion rising either gradually or by a vertical step to the original level. Branching, and in some cases intersecting, cracks were observed. These depressions were evidently connected with outbursts of sand and water, which, along cracks, marked by depressions on both sides, sometimes covered areas of many hundred square feet with layers a foot or more in depth, marked here and there by craters 2 feet or more in diameter, through which water had risen during the outburst of these mud volcanoes. While the earthquake movements in the adjacent hills of Palæozoic strata had left no marks, the dislocations over many square miles in the valley would have sufficed to throw down buildings and to cause great loss of life in an inhabited region. There are believed to be no evidences of previous earthquake disturbances in this region since its discovery by the Spaniards centuries ago. From the published reports of commissioners named by the State of Sonora, it appears that the regions injured by the earthquake are in two nearly parallel north and south valleys in the district of Moctezuma, along the River Bavispe, a tributary of the Yaqui. In both regions are noticed the opening, in the arable lands, of numerous fissures, generally north or north-east in direction, from many of which water flowed abundantly. The river thus supplied in a time of excessive drought swelled to the volume usual in the rainy season of summer, a condition which lasted up to the time of the report of Señor Liborio Vasquez, dated at Bavispe, May 29, 1887. The fields had become green and the air moist with prevailing fogs. A report concerning the region of Opoto mentions not less than seven volcanoes in the vicinity, which were seen burning for two days, but without any flow of lava; while that for the Bavispe region declares that no volcano had there been discovered. The authors incline to the belief that, as was the case in the San José Mountains, and others examined by them along the borders of Arizona, the appearances of volcanoes near Opoto were due to forest fires.

The Disaster at Zug on July 5, 1887, by the Rev. E. Hill.—On July 5, 1887, at the town of Zug, in Switzerland, a portion of the shore gave way and sank into the lake. About three hours later another much larger adjacent area also suddenly subsided, so that in all an area considerably over two acres, with half of one of the principal streets, was submerged to a depth of about 20 feet. It can be seen that the subsoil consists of coarse gravel and sand, followed after a few feet by soft wet sand and fine mud. According to Prof. Heim, this fine mud or sludge reaches to a depth of nearly 200 feet, and the disaster is shown to be due to a flowing out into the lake of this mobile sludge from under the superincumbent weight of buildings and firmer ground. The buildings collapsed as they sank. The catastrophe must have been long impending; the exact cause which precipitated it is indeterminate, but a low level of the lake and tremors from pile-driving for new quays are suggested as contributories. On the English coast the incessant changes of pressure from tides probably render impossible such instability of equilibrium.

The Triassic Rocks of West Somerset, by W. A. E. Ussher.—This paper is the result of investigations made in the years 1878 and 1879. The constitution, extent, and general relations of the Lower, Middle, and Upper Triassic rocks of the area are briefly described, with the following general results:—The Lower Trias consists of breccia and breccio-conglomerate upon sands and brecciated sand and loam; it is faulted against Keuper basement beds, and is conformably overlapped by Middle Trias marls upon the margin of the older rocks. The Middle Trias consists of marls with sandstones in places at their base; it is faulted against the successive divisions of the Keuper on the east, and terminates northward in the angle made by converging faults at Bickноller. The Middle Trias marls rest on the older rocks near Vellow Wood Farm, and finally disappear near Orchard Wyndham, south of Williton, under Keuper breccias. The Keuper beds consist of marls, sandstones, and a locally varying series of conglomerates, gravels, and breccia in descending sequence. The sandstones are very calcareous south of Crowcombe; they form marginal deposits in places near Dunster. The coarser beds of the Keuper develop at the expense of the sandstones in the area west of Williton. It is very probable that the Keuper basement beds of the Porlock valley may be marginal deposits formed during a progressive subsidence, and therefore may belong to a higher horizon than the Lower Keuper beds south of Williton.

The Devonian Rocks of West Somerset on the Borders of the Trias, by W. A. E. Ussher.—The composition of the Quantocks is first briefly described, and the faulted relations of Middle Devonian grits, slates, and limestones of which they consist alluded to. From the constitution of the Palæozoic districts on the east and west of the Triassic rocks of Crowcombe and Stogumber, the author considered the beds eroded in the intervening valley would amply account for the variability of the Triassic strata derived from them. From Withycombe to Porlock the faulted relations of the Middle and Lower Devonian grits are then briefly described. The author considered that the elevation of the Quantocks, the Brendon, and the Dunkery ranges was pr

Triassic, accompanied by faulting on an extensive scale; that many lesser faults were produced in post-Triassic times, and that further movements took place along the old lines of fracture. He did not believe that the Devonian highlands were ever covered by Secondary sediments, but was of opinion that the Triassic rocks never extended far beyond their present boundaries, except in old valleys from which they had subsequently been almost entirely removed by denuding agencies.

Observations on the Rounding of Pebbles by Alpine Rivers, with a Note on their Bearing upon the Origin of Bunter Conglomerate, by Prof. T. G. Bonney, F.R.S.—The author describes the result of his observations of the rounding of pebbles in various torrents and rivers in the Tyrol and Dauphiné, and of the gravels of the Piedmontese and Lombard plains. These lead to the following conclusions, among others: (*a*) that pebbles are rounded with comparative rapidity when the descent of the stream is rapid, and they are dashed down rocky slopes by a roaring torrent capable of sweeping along blocks of much greater volume; (*b*) that pebbles are rounded with comparative slowness when the descent is gentle and the average pace of the river is about adequate to push them along its bed. The rocks observed were in some cases limestone and not very hard grits; in others various crystalline rocks, such as granite, gneiss, or mica-schist. Hence, as the majority of the pebbles in the Bunter are of harder material, and are generally better rounded than those which the author observed, he concludes that it is impossible to suppose them mainly derived from any tract of land which, in Triassic times, can have existed in either Central or Eastern England, for they must have been formed by rivers no less important, with courses either longer or steeper than those of Central Europe. Thus these observations are very favourable to the view which ascribes to them a Scotch origin, where alone rocks exactly like them are known to occur.

The Terminal Moraines of the Great Glaciers of England, by Prof. H. Carvill Lewis.—The investigation here recorded is based upon the important principle that *every glacier at the time of its greatest extension is bounded and limited by a terminal moraine*. The great ice-sheet which once covered Northern England was found to be composed of a number of glaciers, each of which was bounded by its own lateral and terminal moraines. These glaciers were studied in detail, beginning with the east of England; the North Sea glacier, the Wensleydale glacier, the Stainmoor glacier, the Aire glacier, the Irish Sea glacier, and the separate Welsh glaciers were each found to be distinguished by characteristic boulders, and to be defined by well-marked moraines. The terminal moraine of the North Sea glacier, filled with Norwegian boulders, may be seen in Holderness, extending from the mouth of the Humber to Flamborough Head, and consists of a series of conical hills inclosing meres. The Irish Sea glacier, the most important glacier of England, came down from Scotland, and, being reinforced by local ice-streams, and flowing southward until it abutted against the mountains of Wales, it was divided into two tongues, one of which flowed to Wellington and Shrewsbury, while the other went south-west across Anglesey into the Irish Sea. This great glacier and its branches are all outlined by terminal moraines, described in detail. A small tongue from it, the Aire glacier, was forced eastward at Skipton, and has its own distinctive moraine. In the neighbourhood of Manchester the great moraine of this Irish Sea glacier may be followed through Bacup, Hey, Staleybridge, Stockport, and Macclesfield, being as finely developed as the moraines of Switzerland and America. South of Manchester it contains flints and shell-fragments, brought by the glacier from the sea-bottom over which it passed. At Manchester the ice was at least 1400 feet thick, being as thick as the Rhone glacier. The great terminal moraine now described of the united glaciers of England, is a very sinuous line, 550 miles in length, extending from the mouth of the Humber to the farthest extremity of Carnarvonshire, and, except where it separates the Welsh glaciers from the North Sea glacier, it everywhere marks the extreme limit of glaciation in England, and is an important feature which might well hereafter be marked on the geological map of England.

In a separate paper, read at a subsequent meeting, the author described more in detail the moraine near Manchester.

On some Important Extra-Morainic Lakes in Central England, North America, and elsewhere, during the Period of Maximum Glaciation, and on the Origin of Extra-Morainic Boulder-Clay, by Prof. H. Carvill Lewis.—The lakes so characteristic of all glaciated regions are due to several causes. Some few are due to an actual glacier scooping-out of the rock-floor, many to an irregular deposition of the drift by which former watercourses are obstructed, and still others to the terminal moraine or to the glacier itself. These latter, known as *morainic lakes*, may be divided into *inter-morainic lakes*, *moraine meres*, and *extra-morainic lakes*, according to their position—back of, in, or outside of the moraine. Extra-morainic lakes, if dammed up by the ice front, are temporary in character, disappearing with the retreat of the glacier; but, as they may be of enormous extent if the glacier is large, they may produce deposits of much geological importance. Instances of such lakes occur in Switzerland, and ancient examples occur as well in Northern Germany, Asia, North America, and Central England. They are to be expected wherever a glacier advances against or across the drainage of a country. Mr. Belt supposed that Northern Asia was covered by a lake of this character, caused by the Polar glacier obstructing the rivers flowing north. In North America, where the terminal moraine has been accurately mapped for thousands of miles, deposits of boulder-clay and erratics occur outside of the moraine, and have been supposed to be due to an older glacier in the first Glacial epoch. But the entire absence of striæ or of glacial erosion or moraines in this district prove that a glacier was not the agent of deposition. Nor are there any traces of marine life in the deposits. This extra-morainic boulder-clay is narrow in Pennsylvania, where the author had called it "the Fringe," but west of the Missouri it is 70 miles wide; and in British America, between the great moraine called the "Missouri Coteau" and the Rocky Mountains, it is 450 miles wide and over 1000 miles long. It only occurs where rivers had flowed *towards* the glacier, and is explained as the deposit of great temporary fresh-water lakes dammed up by the ice front, the erratics having been dropped by icebergs. Similar deposits occur in England outside of the terminal moraine, and have been the subject of much discussion; being held by some to be a proof of marine submergence, by others to be the ground-moraine of a glacier. The "great chalky boulder-clay" is the best known of these deposits. There are serious objections to the two theories heretofore advanced to explain this, whilst the hypothesis of extra-morainic fresh-water lakes, dammed up by the glaciers, is sustained by all observed facts. The most important of these lakes was one caused by the obstruction of the mouth of the Humber by the North Sea glacier, whose terminal moraine crosses that river at its mouth. This large lake reached up to the 400-feet contour line, and extended southward nearly to London, and westward in finger-like projections into the many valleys of the Pennine Chain. It deposited the "great chalky boulder-clay," and erratics were floated in all directions by icebergs. The conclusion that the glacial phenomena of England are due neither to a universal ice-cap nor to a marine submergence, but to a number of glaciers bordered by temporary fresh-water lakes, is in accordance with all the observations of the author in England and elsewhere.

On the Extension of the Scandinavian Ice to Eastern England in the Glacial Period, by Prof. Otto Torell.—The author described the glacial deposits of Eastern England, particularly those of Holderness and Cromer, the latter having been examined by him on several occasions during the last twenty years. Applying his experience gained during winters spent within the Arctic Circle, the author showed that the boulder-clay of Holderness and Cromer is a true ground moraine formed near the southerly limit of the Scandinavian ice-sheet. The Cromer tills were formed by an extension of the Baltic ice; Swedish boulders brought by this ice can be traced across the German plain, and are found at Cromer. The till of Holderness was formed by an extension of the Skagerack, as is proved by characteristic Norwegian boulders. The Baltic ice retreated first, and the Skagerack ice, still moving onward, ploughed into and contorted the Cromer till. The distribution of the boulder is described in detail, as also is the succession of the beds as worked out by Mr. Clement Reid, whose facts and conclusions fit well with the opinions advanced by the author.

A Comparative Study of the Till or Lower Boulder-Clay in several of the Glaciated Countries of Europe—Britain, Scandinavia, Germany, Switzerland, and the Pyrenees, by Hugh Miller.—The sections of foreign till examined by the author occur chiefly in the neighbourhood of the Trondhjem Fjord in Norway, at Berlin and Leipzig in Germany, near the Lake of Geneva in Switzerland, and in the valleys of the Pyrenees directly south from Pau in Southern France. In these countries and in Britain the till bears an identical character. It is not more variable throughout Europe than the author has found it to be in Scotland and Northern England. On the basement-gneiss at Christiansund, in South-Western Norway, it is the same as on the basement-gneiss of Sutherlandshire; in the great limestone valley of Eaux Chauds, in the Pyrenees, it is scarcely to be distinguished from the till of the limestone valleys of Yorkshire. In all the places mentioned (more doubtfully at Berlin and Leipzig) it bears the unmistakable character of a ground-moraine accreted under the direct weight of glacier-ice; its essential character is that of a rude pavement of glaciated debris ground from the rocks over which the glaciers have passed, with its larger boulders firmly glaciated *in situ* on their upper sides in the direction of ice-movement, and with a tendency to the production of fluxion structure here and there in the matrix, due to the onward drag of the superincumbent ice. In mere indiscriminateness of composition (which is a character much emphasized by glacialists) the till is not to be distinguished from boulder-clays formed under berg- or raft-ice, such as the highest marine clays of the Norwegian coasts, which are stuck promiscuously through with boulders derived from the glaciers of the interior. The glaciation of boulders *in situ* the author finds to be a crucial distinction; he readily detected this "*striated-pavement*" character in the tills of all the districts above mentioned except Leipzig and Berlin, where the boulder clays more resemble the upper boulder-clay (Hessle clay) of the eastern seaboard of England and Scotland, and in the sections examined by him contained no large blocks.

Note on a few of the many Remarkable Boulder-stones to be found along the Eastern Margin of the Wicklow Mountains, by Prof. Edward Hull, F.R.S.—Amongst the evidences of the former existence of an extensive sheet of ice descending from the Wicklow Mountains towards the shores of the Irish Sea is the occurrence of boulder-stones, chiefly formed of granite or granitoid gneiss, derived from the mountainous range to the westward, of a size seldom equalled—probably not surpassed—amongst the British Isles. (1) The Mottha Stone is perched on the summit of Cronhane Hill, above Castle Howard, and is a conspicuous object for all directions. It consists of grey granite, and rests upon Lower Silurian slate; its weight would be about seventy tons. It lies at a level of 816 feet above the sea, and is about 10 or 12 miles from the flanks of Lugnaquilla, whence, as we may suppose, the granite block started on its journey. (2) In the valley between Castle Kevin and Moneystown, where large boulders are numerous, there lies a block of granite about 100 tons in weight. The birth-place of this boulder was probably the mountainous district about Mullaghcleevann, 2783 feet in height, lying at the head of the valley in which is situated the deep waters of Lough Dan, and it probably travelled a distance of 8 or 9 miles in an east-south-east direction. (3) The last boulder-stone is perhaps the largest in the British Islands. It stands behind a cottage by the roadside, near Roundwood Church, and is quite as large as the cottage itself, to which it forms a good protection from the storms descending from the mountains behind. This boulder consists of granitoid gneiss, resting on Lower Silurian slate and grit, and is about 240 tons in weight. The source of this block, which lies at an elevation of about 800 feet above the sea, was probably in the same locality with that of the Castle Kevin boulder, and the distance travelled was about 6 or 7 miles. The blocks above noticed, with many others of smaller size, do not belong to any of the local glaciers which once filled the valleys towards the close of the Glacial epoch, and which have left numerous well-formed moraines in nearly all the principal valleys descending from the Wicklow range. They are to be referred, in all probability, to the earlier stage of intense glaciation, in which the whole district was covered with perennial snows and ice, moving eastward into the hollow now occupied by the waters of the Irish Sea.

On New Facts relating to Eozoon Canadense, by Sir J. William Dawson, F.R.S.—For several years no new facts respecting the Canadian Eozoon have been published, though there has been some discussion on the subject abroad. In so far as the author is concerned, this has arisen from the circumstance that the late Dr. Carpenter had in preparation an exhaustive memoir, for which Canadian material was being supplied, but which was unfortunately left unfinished at his death. The material collected for this has now been placed at the disposal of Prof. T. Rupert Jones, F.R.S., and in the meantime the present note is intended merely to direct attention to some new facts recently obtained. The form of Eozoon has been definitely ascertained to be normally inverted conical or broadly turbinate, except where several specimens have become confluent, or when rounded masses have been based on some foreign body. The larger specimens are traversed by cylindrical or long conical vertical openings (pores or oscula), around which the laminæ, becoming confluent, form an imperfect wall. Some other points of detail were mentioned, and facts were referred to indicating the continuity and definitely stratified character of the beds in the Middle and Upper Laurentian of Canada. A variety of laminated rocks and minerals which had been mistaken for Eozoon were referred to. Their description in more detail will be found in forthcoming memoirs of the Peter Redpath Museum.

Elements of Primary Geology, by T. Sterry Hunt, F.R.S.—The author, after recalling his classification of original or non-clastic rocks into Indigenous, Endogenous, and Exotic masses, based on their geognostic relations, gives in a concise form his theory of the genesis of these various groups of rocks, as taught more at length in his recent volume entitled "Mineral Physiology and Physiography." The superficial portion of a cooling globe, consolidating from the centre from a condition of igneous fusion, he conceives to have been the protoplasmic mineral matter, which, as transformed by the agencies of air, water, and internal heat, presents a history of mineralogical evolution as regular, as constant, and as definite in its results as that seen in the organic kingdoms. This great transformation involves a series of processes, which include: (1) the removal from the protoplasmic mass, through permeating waters, heated from beneath, of the chief elements of the great groups of indigenous crystalline and colloidal rocks, by what he has called the crenitic process; (2) the modification of the residual portion by this lixiviation, which removes silica, alumina, and potash, and, by the intervention of saline waters, brings in additional portions of lime, magnesia, and soda; (3) the partial differentiation, by crystallization and eliquation, of portions of this more or less modified residue, giving rise to the various types of plutonic rocks. The direct and indirect results of subaërial decay through atmospheric agencies, and those of the products of organic life, are also considered. From the operation of all these processes result progresssive changes in the composition alike of plutonic and of indigenous rocks. The endogenous rocks or veinstones are, like the last, of crenitic origin, and may be granitic, quartzose, or calcareous in their characters.

The author next considers the conditions of softening and displacement of indigenous rocks which permit them to assume in many cases the relations of exotic rocks, and to become extruded after the manner of lavas, as seen in the case of trachytes and many granite-like rocks. Such masses he designates pseudoplutonic. With these are often confounded the endogenous granitic veinstones, which were formed under similar chemical conditions to the indigenous granites. Masses alike of indigenous, endogenous, and exotic rocks may also become displaced, not through softening, but by being forced while in a rigid state, through movements in the earth's crust, among masses softer and more yielding than they.

The author also considers the fluxional structure seen in plutonic and pseudoplutonic eruptive masses, which has led some theorists to regard these as of aqueous origin, and others to maintain that the crenitic stratiform masses themselves are of plutonic origin—two opposite errors which vitiate much of our geological literature. The crenitic process, by removing (from beneath what was the original surface of deposition) the vast amount of material which forms alike the indigenous, the endogenous, and the pseudoplutonic rocks, has effected a great diminution in volume in the protoplasmic mass, besides that due in later times to extrusion of plutonic matter itself. This decrease in bulk of the underlying stratum was a potent agent in producing the universal corrugation of the earlier crenitic rocks, and the frequent discordances observed among them.

The author considers further the gradual diminution of the crenitic process seen in the later periods of Archæan time, and its feebler manifestations in Palæozoic and more recent ages down to the present. He notes moreover, that, as the result of geographical changes, erosion and partial deposition alike disturbed the succession of the later groups of crenitic rocks, none of which can claim that universality and uniformity which belong to the oldest known group, the fundamental granitoid gneiss.

The author concludes with a brief sketch of the great divisions of the indigenous crenitic rocks recognized by him, from the most ancient granitic substratum to the Taconian series, which appears to be the last of the characteristically crystalline indigenous groups, it being, so far as known, succeeded directly by the uncrystalline Cambrian, or the equally uncrystalline Keweenian, which may not, improbably, be itself included in the lower part of the Cambrian series.

Gastaldi on Italian Geology and the Crystalline Rocks, by T. Sterry Hunt, F.R.S.—The author recalled the fact that, in discussing in 1883 the geological relations of serpentines, he had maintained that, although not confined to Archæan rocks (in which they are found at various horizons), those of Italy, believed by some geologists to be in part of Tertiary age, are, so far as his studies go, wholly Archæan, in accordance with the views set forth by Gastaldi. The serpentines and other rocks of the ophiolitic group existed in their present condition in the seas in which were deposited the Eocene strata, which latter, by subsequent terrestrial movements, had been disturbed, broken, and even inverted, so as to seem to pass beneath the ophiolites. The indigenous and neptunian character of serpentines is maintained, while the plutonic hypothesis of their origin has been so far modified by its modern Italian advocates that they now suppose the serpentines to be due to submarine eruptions of a hydrous magnesian mud, which subsequently consolidated into serpentine and even into chrysolite. It is difficult to admit any other than a neptunian origin for the stratiform ophicalcites into which the massive serpentines often graduate. In a letter written in July 1878 to the author, but until recently mislaid, Gastaldi showed that the ophiolites of the Ligurian Apennines and of those of Prato were, like those elsewhere examined by him, protruding portions of the ancient *pietre verdi* zone, identical with that of the Alps, from which the Apennines cannot be distinguished either geologically or geographically. The vast series designated by him as the *pietre verdi* zone, according to Gastaldi, overlies the ancient central or primary gneiss (generally granitoid, but including quartzites and crystalline limestones with graphite, &c.), and has a thickness of many thousand metres, embracing three subdivisions. The lowest of these, sometimes called by him the *pietre verdi proper*, includes serpentines, diorites, euphotides, chloritic schists, &c.; the second is designated by him recent gneiss and granite with mica-schists and hornblendic rocks; while the third consists in great part of soft argillaceous or lustrous schists, with included quartzites, marbles, statuary, and banded dolomites and occasionally also serpentines, the presence of which led Gastaldi to include it with the two preceding subdivisions in his great *pietri verdi* zone; a name which the present writer, with Neri and others would restrict to the lower subdivision, regarded by him as the equivalent of the Huronian of North America; the underlying or central gneiss being the Laurentian; the recent gneiss and mica-schist, the Montalban or White Mountain series, and the upper subdivision, the Taconian or Lower Taconic of North America; the wholly distinct Upper Taconic being an uncrystalline series of fossiliferous Cambrian strata. The writer in this connexion recalled the work of Neri, Gerlach, and others in the western Alps, and that of Von Hauer and his associates in the Lombardo-Venetian Alps, where the same distinction of the true *pietre verdi* zone between the ancient gneiss below and the recent gneiss above had, unknown to him, been pointed out by the Austrian Geological Survey two years before the present writer in 1870 defined and named the younger gneissic series in North America. The absence of the true *pietre verdi* series in some localities, alike in the Alps and in North America, between the older and younger gneisses was noted, as well as the existence of apparent discordances between each one of the four great divisions of Archæan or pre-Cambrian crystalline rocks above named.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. A. E. Shipley, of Christ's College, has been appointed Demonstrator of Comparative Anatomy; Mr. H. F. Newall, of Trinity College, Demonstrator of Experimental Physics; and Messrs. L. R. Wilberforce and H. L. Callendar, both of Trinity College, Assistant-Demonstrators of Experimental Physics.

Dr. Peile has been elected Master of Christ's College, an election which all classes of University men, and reformers in particular, rejoice in.

A novelty is to be introduced in the education lectures next term, Dr. Francis Warner, of the London Hospital, having consented to give a course of six lectures on the physical and physiological study of children.

The next examination for Entrance Scholarships and Exhibitions at Gonville and Caius College will begin on December 10. Candidates must be under nineteen on that day.

Two scholarships of £80, and four others of £60 to £40, will be offered, with exhibitions. Candidates for Natural Science Scholarships may be examined in any of the following subjects: physics, chemistry, biology, and animal physiology, and will be expected to show proficiency in two of them, of which chemistry must be one. The tutors will forward full particulars on application.

We cannot find space for the long list of lectures in natural science now being given at Cambridge. Prof. Liveing gives a general course of principles of chemistry, and Prof. Dewar lectures on dissociation and thermal chemistry. Prof. Thomson lectures on electricity and magnetism, and on the applications of dynamical principles to physical phenomena. One of the courses of demonstrations at the Cavendish Laboratory is an advanced one on the properties of matter, and on sound. Prof. Lewis lectures on mineralogy, Prof. Stuart on mechanism. Mr. Marr lectures on principles of geology, Mr. Roberts on the palæontology of Echinoidea.

Dr. Vines lectures on the physiology of plants (advanced), Mr. Potter on the geographical distribution of plants.

Prof. Newton lectures on evolution in the animal kingdom; Mr. Gadow on the morphology of Ichthyopsida (recent and extinct); the Curator in Zoology, Mr. Cooke, on the geographical distribution of recent Mollusca.

Prof. Foster's, Dr. Lea's, and Mr. Langley's physiological courses are as usual. Prof. Macalister gives demonstrations on topographical human anatomy, as well as an elementary course.

Prof. Cayley's lectures this term are on quaternions and other non-commutative algebras; Prof. Darwin's on the orbits and perturbations of planets. Mr. Glazebrook lectures on hydrodynamics (waves and sound); Mr. Hobson on spherical and cylindrical harmonics; Mr. Larmor on electrostatics; Mr. Forsyth on modern algebra (binary forms); Dr. Besant on theory of equations and differential and integral calculus; Mr. H. M. Taylor on higher plane curves; Mr. Pendlebury on theory of numbers; Dr. Glaisher on elliptic functions; and Mr. Stearn on elastic solids.

SCIENTIFIC SERIALS.

American Journal of Science, September.—Notes on the geology of Florida, by William H. Dall. In this paper the results are given of two excursions to Southern and Central Florida undertaken in 1885 and 1887 by instruction of the Director of the United States Geological Survey. Special attention is devoted to the process of contemporaneous rock-formation now going on along the Gulf shores of West Florida, and to the Tertiary rocks which prevail so largely throughout the Peninsula. No coral or coral-reef formation was anywhere observed, and it is evident from these further researches that the hypothesis of Agassiz regarding the geological origin of this region can no longer be maintained.—Notes on the deposition of scorodite from arsenical waters in the Yellowstone National Park, by Arnold Hague. The occurrence of this comparatively rare mineral as a deposition from thermal mineral springs is here noticed for the first time. It is found in several localities in the Yellowstone Park as an incrustation deposited from the waters of several hot springs and geysers, and is especially abundant at the Joseph's Coat Springs on Broad Creek, east of the Grand Cañon. The analysis—yielding Fe_2O_3 34·94, As_2O_5 48·79, and H_2O 16·27—shows this mineral to be true scorodite, a hydrous arsenate of iron, the layers varying from a mere coating to an eighth of an inch in thickness. Wherever observed it occurs as an amorphous deposit, and when pure, leek green in colour.—The effects of magnetization on the viscosity and the rigidity of iron and steel, by C. Barus. An attempt is made in this memoir to verify by a static method the results recently communicated by Mr. Herbert Tomlinson on the changes of viscosity and of elasticity produced by magnetizing iron. It is shown that the increment of rigidity due to magnetization increases at an accelerated rate as the soft, temporarily twisted wire becomes more nearly filamentary. A series of results is also given on the rigidity of magnetized steel temporarily strained and varying in temper from extreme hard to extreme soft. A main object of the paper is to show how the principles here established may be utilized for the construction of electric dynamometers.—Fauna of the "Upper Taconic" of Emmons, in Washington County, New York, by Charles D. Walcott. This paper deals specially with the fauna represented by *Atops trilineatus* and *Elliptocephala asaphoides* from the black Taconic slate near Bald Mountain, Washington County, as described by Dr. Emmons in his second memoir on the "Taconic System." The paper is accompanied by a plate illustrating nineteen specimens of this fauna.—On the amount of moisture remaining in a gas after drying by phosphorus pentoxide, by Edward W. Morley. This quantity is here determined by the method applied in the case of sulphuric acid, the process consisting in drying the gas with phosphorus pentoxide and then passing it through a weighed apparatus in which the gas is first slightly moistened, then much expanded, and lastly again dried by phosphorus pentoxide.—Is there a Huron group? by R. D. Irving. In this paper the author inquires whether there can be carved off from the upper part of the great complex of rocks ordinarily known as Archæan, a ***Huronian*** series, entitled to rank with such groups as the Cambrian, Siberian, &c. In this first part of the memoir it is shown that the series on the north shore of Lake Huron mapped by Logan on Plate iii. of the atlas to the geology of Canada (1863) is entitled to rank as a separate group by its intrinsic characters and its structural distinction from the older Archæan and younger Cambrian and pre-Cambrian rocks of that region.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 3.—M. Hervé Mangon in the chair.—On some properties relative to the action of crystalline plates on light, by M. Mascart. It is shown that a system of waves on the same plane traversing a crystalline plate with parallel faces is decomposed into two systems of polarized waves with unequal retardation which at the exit are reconstituted in a system of waves in a state of vibration different from the first. From this is deduced the theorem that the action on light of any group of crystalline plates, endowed or not with rotatory power, is equivalent to that of a single plate with axis parallel to the incident axis and perpendicular to the incident rays.—On an experiment with M. D. Colladon's artificial waterspout, by M. Mascart. The action of this ingenious apparatus, as well as that of M. Weyher, seems to show that there is undoubtedly an ascending movement in the central part of all cyclonic phenomena. With regard to the recent waterspout in Lake Geneva, it is pointed out that the ascending motion stated to have been witnessed by M. Dufour and other observers, could scarcely be an optical illusion, as maintained by M. Faye. Some of the water seen to ascend was afterwards precipitated as rain, drenching some men engaged on the railway.—Remarks on M. Colladon's recent experiment, by M. Faye. In reply to the foregoing, it is pointed out that in a series of remarkable experiments conducted under like conditions, M. von Bezold, Director of the Berlin Central Meteorological Observatory, has, on the contrary, produced a descending movement in the direction of the long axis. But M. Faye rejects both classes of experiments, holding that his theory is neither refuted by the first, nor confirmed by the second, as none of the apparatus in question really succeeds in reproducing a natural waterspout.—Experimental study of human locomotion, by MM. Marey and Demeny. In continuation of their previous communications on this subject, the authors here analyze, by means of the photochronographic process, the movements of the trunk in walking and running. The accompanying diagrams show the successive figures of a runner photographed from above at intervals of one-tenth of a second.—On the non-existence of spontaneous tetanus, by M. Verneuil. The existence is denied of spontaneous or medical as opposed to traumatical or surgical tetanus. It is shown, however, that besides the latter there also exists a variety of the disorder, for which the term tetanus by absorption is proposed.—Researches on the apparently spontaneous movements of contraction and relaxation which after death are continued in the muscles so long as the *rigor* lasts, by M. Brown-Séquard. The results are described of numerous experiments carried out on rabbits, dogs, and monkeys by means of the graphic process, showing that complex muscular action continues after death throughout the whole period of *rigor mortis;* that is, until putrefaction sets in, which may at times be deferred for several weeks. The action is mostly irregular, but occasionally almost rhythmical, and the more decided movements occur not in the early stages, but towards the end, sometimes in the second, third, and even fourth week. It is made clear that they cannot be attributed to changes of temperature, variations of humidity or ozone, barometric pressure, or other atmospheric influences, nor yet to magnetism or electricity, at least to any great extent. It will be shown in a future communication that they are due to the persistence of muscular irritability; that is, to the fundamental property of the living muscular tissue surviving till arrested by putrefaction.—General results of fresh studies on several series of fatty and aromatic monamines, by M. Malbot. These studies deal with the ethylamines, the propylamines, butylamines, amylamines, caprylamines, and aromatic amines. Their whole history is cleared up, and a general interpretation is arrived at of their formation. Whether occurring in the free state or in combination, they result from a conflict of energies between the rival affinities of ammonia and the amines for the ether and its acid. With regard to their formation, the author's experiments seem to favour the ethylene theory of Berthelot rather than that of ethyl advocated by Hofmann.—A memoir on the syphon barometer was presented by M. Govi, who credits Torricelli with the first idea, and Pascal with the practical execution and first employment of this instrument, the invention of which has been successively attributed to Robert Hooke, Robert Boyle, and Borelli. He shows that the principle was known to Torricelli in 1644, when he used it to explain to Ricci the theory of the cistern barometer; also that Pascal was acquainted with it in 1653, while Hooke mentions it for the first time in 1665, Boyle in 1666, and Borelli in 1667.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Student's Hand-book to the Microscope: A Quekett Club Man (Roper and Drowley).—Weather: Hon. Ralph Abercromby (Kegan Paul).—Our New Zealand Cousins: Jas. Inglis (Low).—Our Fancy Pigeons: George Ure (Mathew, Dundee).—The Solomon Islands and their Natives: H. B. Guppy (Sonnenschein).—The Solomon Islands, their Geology, &c.: H. B. Guppy (Sonnenschein).—A Sketch of Geological History: Prof. E. Hull (Deacon).—Factors in Life: Prof. H. G. Seeley (S.P.C.K.).—Pictorial Geography of the British Isles: M. E. Palgrave (S.P.C.K.).—Sixth Annual Report of the U.S. Geological Survey: J. W. Powell (Washington).—Journal of the Royal Statistical Society, September (Stanford).—Mind, October (Williams and Norgate).

CONTENTS.

THURSDAY, OCTOBER 20, 1887.

THE ENCYCLOPÆDIA BRITANNICA.

The Encyclopædia Britannica. Vol. XXII. Sib—Szo. (Edinburgh: A. and C. Black, 1887.)

THE leading mechanical articles in this volume are full of interest to the general reader, and contain all recent information sought after by the specialist. Captain C. A. G. Bridges, R.N., contributes a short and concise article on naval signalling. The article is well written, and contains a large amount of historical information condensed into a small compass. The history of signalling is traced from a very early period. In fact, we are told that Polybius described two methods—one proposed by Æneas Tacitus, 300 B.C., and one introduced by himself, by which any word could be spelled, thus embodying the underlying principle of all recent methods. After an account of signalling by combinations of flags, perfected by different people at different periods, we come to Captain Philip Colomb's flashing system of signalling adopted in the navy in 1867. By means of the Morse alphabet, using long and short flashes of light by night, and blasts of a horn or steam-whistle during a fog, or by waving a flag in day-time, any communication can be made by this system. For short distances the semaphore is now greatly used. This consists of a vertical post with two arms movable in a vertical plane, the changing positions of the arms indicating different letters.

The article on sounding, by Mr. W. E. Hoyle, is a very short *résumé* of the subject. The operations in sounding are described, and the gradual improvement in the apparatus used is traced to the present date; beyond this there is nothing particular to note, and the article might have been extended with advantage.

The most exhaustive article in the volume is by Prof. J. A. Ewing, on the steam-engine. Prof. Ewing has skilfully condensed into twenty-six pages a large amount of useful information. To commence with, we have a good historical account of the early inventions. Hero, Savery, Papin, and Newcomen have each their place, and receive careful attention at the hands of the author. Watt's inventions are described, and the several forms of his engines are clearly illustrated.

The compound engine, the engine of to-day, dates from 1781, when Jonathan Hornblower patented an arrangement of two cylinders of different sizes, the steam being first admitted into the smaller cylinder, then passing into the larger, doing work in each cylinder. Woolf revived this class of engine in 1804, and in 1814 introduced it as a pumping-engine in Cornwall.

Richard Trevithick shortly afterwards introduced, in Cornwall, a single-cylinder high-pressure engine, which very soon was generally adopted, and became known as the "Cornish" pumping-engine. This engine worked with a comparatively high steam-pressure, and with considerable expansion. The cylinder was placed under one end of an over-head beam, and the pump rods were connected to the other end. The steam was admitted above the piston for a short portion of the stroke and then cut off, expanding the remaining portion, and doing work by lifting the pump rods and their attachments. The space above and below the piston was then put into connexion through the equilibrium valve, and the piston ascended by reason of the weight of the pump rods and gear, and did work in the pumps. The frequency of the stroke was regulated by means of a device called a cataract. This class of engine was used for many years and reached a high state of efficiency; we are told that the Fowey Consols engine has a consumption of coal of only 1¾ lbs. per horse-power per hour, a result considered exceptionally good even to-day.

Prof. Ewing treats the steam-engine as a heat-engine in a masterly manner. Nothing of importance has been omitted; the arrangement of facts being in a carefully condensed form and easily followed. The actual behaviour of steam in the cylinder is demonstrated, and the effects of "wire drawing" and clearance in the cylinder and steam ways are duly explained. He carefully treats the testing of steam-engines; the best modes of taking indicator-diagrams are given, and points liable to be forgotten are well looked after. The actual testing is explained by means of a numerical example, viz. the trial of a compound beam engine, steam-jacketed, with an intermediate receiver between the cylinders. On compound expansion Prof. Ewing has a great deal to say. The indicator-diagrams illustrating the letterpress are very clear and to the point.

Next we find a good general description of steam-engines and boilers for stationary, marine, and locomotive purposes. These are well described, and few, if any, important points omitted. Under the head of locomotives we have an account of Mr. F. W. Webb's compound locomotive. This is a three-cylinder engine, having two equal high-pressure cylinders fixed outside the frame, and driving the trailing-wheels by means of two crank-pins at right angles; a single low-pressure cylinder of suitable dimensions is set under the smoke-box, and is coupled to the driving-wheels by means of a single-throw cranked axle. The two high-pressure cylinders exhaust into the low-pressure valve-chest, and this in its turn exhausts into the atmosphere through the blast-pipe. These engines are doing good service on the London and North-Western Railway, and a considerable saving of fuel is claimed for them over the quantity used by ordinary locomotives working the same traffic.

Prof. Ewing has, however, omitted to mention Mr. T. W. Worsdell's successful compound locomotive, first tried on the Great Eastern Railway, and now being used on the North-Eastern for passenger and goods traffic. This engine is very little different from the ordinary locomotive, the only difference being that the two cylinders used are of unequal diameters to suit compound working. This is probably a special gain because no radical change is made in the general arrangement, and the different parts of the valve motion, &c., come in equally well for the compound as for the ordinary engine. The main feature of the Worsdell engine lies in the starting arrangements. A locomotive must be able to start in any position with considerable power to fully answer its purpose, and the ordinary engine, after an eighth of a revolution of the driving-wheels, has both its cylinders thoroughly effective. In the two-cylinder compound the low-pressure cylinder does not get an efficient steam-pressure at once, because the steam must first work in, and be exhausted from, the high-pressure cylinder before reaching the low-pressure cylinder. Thus, it is evident that the high-pressure piston has to move the engine through at least half a revolution of the driving-wheels before the low-pressure piston is able to do any work; and further, if the high-pressure crank happens to be at or near the dead centre, it is impossible for the engine to move one way or the other, unless some means are adopted to make the low-pressure piston effective to move the high-pressure piston past the dead point. Mr. Worsdell's starting-gear entirely gets over this difficulty, and makes the two-cylinder compound locomotive a handy engine and a good starter.

By means of this arrangement the engine-driver is able to close the pipe connecting the high- and low-pressure valve-chests by a valve opening towards the low-pressure valve-chest. At the same time, steam direct from the boiler is admitted into the low-pressure cylinder, the intercepting valve preventing it blowing back into the high-pressure cylinder. On the engine moving for half a revolution of the driving-wheels, the high-pressure cylinder exhausts into the low-pressure valve-chest, blowing the intercepting valve open on its way, and compound working commences. As soon as the engine moves, the supply of steam from the boiler is of course discontinued in the low-pressure cylinder. In actual practice this gear is more or less self-acting, and is set in motion by moving one small lever.

At the end of the article a useful index is given, and throughout copious references are made to many authorities, which will be of service to anyone searching for information in any particular branch.

Prof. Ewing also contributes an interesting article on the strength of materials, and has treated the subject in a very satisfactory manner. After a clear introduction to the subject, we find a really practical description of tensile testing of materials. This is very well done, including as it does all recent information and experiments. After the general adoption of steel in the construction of engineering structures, whether bridges or ships, tensile testing of the material came into every-day use, and a testing machine is now to be found in every steel-works worthy of the name. Engineers were slow in adopting steel, and rightly so, considering the many unaccountable failures of that material which took place a year or two ago. Even now, when its manufacture is much better understood, stringent tests are specified by our leading engineers before the material is allowed to be used; and, as an example, all steel plates used in the bridge work for certain railways are tested as follows. *Every plate rolled* has a side and end shearing tested by tensile tests, besides the usual quenched and cold bends, in order to guard against the possible use of brittle or dangerous steel. It is not to be wondered at, therefore, that the tensile testing machine has reached a high state of perfection, the latest improvement being the addition of an autographic recorder by which the results of each test piece broken are recorded by the machine itself. This article contains all the information likely to be required, and has been put together in a concise form, Prof. Ewing making the most of the limited space placed at his disposal. Both articles are quite models of encyclopædic articles.

The principal articles relating to natural history are the following:—On the "Siluridæ," by Dr. Günther. Some of the more remarkable points in the structure and life-history of the members of this group having been already noticed under the article "Ichthyology," in the present article we find only a notice of the chief sections into which the Siluroids have been divided; but though thus short, it gives a most useful *résumé* of what is known on the subject. A long and important article on "Snakes" is also from the pen of the same author. The article commences with an account of the classification and distribution of the members of this order, the number of known species being reckoned at from 1500 to 1800; next follow some details as to their general anatomy, especially as to their poison-fangs. A list of the sub-orders and families is given, and there are some excellent illustrations of some of the more important species. The various notices on birds, the snipe, stork, swan, &c., have been contributed by Prof. Alfred Newton, and, it is needless to say, embody all the most recent information possible in the small space allowed. The article on "Swine" is by Prof. Flower. A most excellent article on the "Skeleton" is by Mr. St. George Mivart. While we have nothing but praise for his treatment of the subject of the general skeletal conditions of the Vertebrata, we may venture to doubt whether the author could justify the assertion which occurs in the second paragraph of the article, to the effect that among plants the Desmidiæ clothe themselves with a horny coat. Making all due allowance for the rather loose way in which this word "horny" is used by some biologists, we think the botanists have not yet agreed to use it for any of the varieties of cellulose, and the Desmids are not even very exceptional in their clothing. The most important article from a zoological point of view in the volume is that on "Sponges," by Prof. Sollas. He takes notice of the great advance that has been made in our knowledge of this group during the last fifteen years, and gives one of the best accounts of the group in our language. Beginning with a sketch of the general structure and form of Sponges, he plunges into a disquisition on the characteristic spicules met with; and, though within very brief limits, ventures on a classification, with a detailed nomenclature of these protean forms. Whether the mass of the new names will meet with acceptance or not, time must decide; but there can be no doubt that the author deserves every commendation for his brilliant effort to reduce the various forms into an orderly sequence. Within the last few years the improvements in the methods of technique have opened new vistas into the histology of Sponges. The classification of the group is given in detail as far as the families: the Phylum being regarded as derived from the choanoflagellate Infusoria, but pursuing for a certain distance a course of development parallel with that of the Metazoa. The paragraph on the sponges of commerce is very interesting: we do not know of an equally accurate account in English, and yet no toilet article is in more universal use or more talked about. The little that is known as to the extent of the large trade in sponges may be judged of from the fact that the latest information the author was able to get on the subject dates from 1871, and that it only gives an account of the sponges sold in Trieste for that year. This article may be described as an excellent introduction to the history of the Sponges.

Although the "Encyclopædia Britannica" has reached its twenty-second volume, the editors are to be congratulated on the continued excellence of the articles generally, and it is evidently their intention to complete the series without lowering the quality of the work in the slightest degree. When completed, this edition will be a great addition to our literature, of which we may without hesitation be thoroughly proud.

THE MOTIVE POWERS OF THE MIND.

Psychology: The Motive Powers; Emotions, Conscience, Will. By James McCosh, D.D., LL D., Litt.D., &c. (London: Macmillan and Co., 1887.)

THE first part of this volume, constituting more than two-thirds of the work, is a somewhat condensed and slightly modified reprint of the author's volume on "The Emotions," issued some seven years ago by the same publishers. It would, we think, have been well to have stated this fact. As it stands, there is nothing to show that this volume is not wholly new. It does not appear that the author's views, or the manner of their presentation, have undergone any material change.

Emotion is considered under four aspects, determined by four elements. First, there is the disposition, inclination, or "appetence," an innate or acquired principle of the mind; secondly, there is the idea or "phantasm" of an object or occurrence fitted to gratify or disappoint an appetence; thirdly, there is the conscious feeling or excitement, with attachment or repugnance, called forth by the phantasm; fourthly, there is an organic affection or bodily expression of the emotion.

A good deal of space is devoted to the appetences. Such an appetence is described as "simply a tendency in the mind to crave for an object for its own sake." It is difficult to see wherein it differs from a latent emotion. The possession of an appetence implies the possession of an emotional nature capable of responding in certain ways at the bidding of the appropriate idea or phantasm. But it is questionable whether it is very satisfactory to classify apart from the actual emotions these inherent possibilities of emotion. The appetences are divided into primary and secondary, or derivative. The account of the former begins with the love of pleasure and the aversion to pain. These, says the author, "do not need to be defined, for all sensitive beings know what they are. I rather think," he naïvely adds, "that all pain originates in a derangement of our organism. But it is not felt as pain till perceived by the conscious soul." The other primary appetences include, in the order named, the sympathetic instincts, attachment to relatives, native tastes and talents, the appetites, love of society, love of esteem and commendation, love of power, love of property, the æsthetic sentiment, and the moral sentiment. "The derivative appetences," we are told, "may and do assume an immense variety of forms, which run into and are mixed up with each other," and are "woven together in all sorts of ways, so that it is difficult to unravel the web." It is noteworthy that such unravelling as is to be found comes under the head of the classification of the emotions themselves.

Writing of "conflicting appetences," the author would almost seem for a moment to have fallen into the slough of determinism. "Passions," he says, "may contend in two ways. First, there may be the operation at one and the same time of two inconsistent propensities (*e.g.* duty and pleasure). Were the two equally balanced they might counteract each other, and inaction be the statical result. . . . But more frequently both passions act." (This we presume is a loose way of saying that they are not so absolutely and equally antagonistic as to produce inaction.) In this case "on the principle of the parallelogram of forces the man follows an intermediate course." Could any determinist have expressed his heresy more clearly? In the third part, however, dealing with the will or "optative power," we find a chapter with the orthodox heading, "The Will has Freedom." And though there is scarce any word therein with which the determinist will feel disposed to quarrel, we are led to suppose that Dr. McCosh would maintain that in the action chosen under volition the result is not always determined by the several "appetences" called into play by the "phantasm." Into this question, however, he does not enter: he merely contends for freedom in the popular sense, which no one denies.

Under the heading "The Organic Affection," and in the classification of the emotions, the modes of physical expression are set forth with quotations from Darwin, Bell, and Cogan. A short account of the anatomy of expression, by Prof. Osborn, has been added in this edition.

Regarded as a whole, Dr. McCosh's volume, notwithstanding a certain sketchiness and superficiality, shows not a little insight into the workings of the human mind. It is essentially descriptive. The author does not profess to dig down into origins. "I wish it to be distinctly understood," he says, "that in this treatise I undertake not to determine the origin of motives in the ages past and among the lower animals; I am satisfied if I can give an approximately correct account of them as they now act in the human mind." The promise to enter little into controversy is fairly redeemed. Even the few controversial passages there are had been better omitted, since they deal for the most part with questions of origin, that of the conscience for example, into which as we have just seen, Dr. McCosh wisely does not undertake to enter.

C. LL. M.

OUR BOOK SHELF.

Our New Zealand Cousins. By the Hon. James Inglis. (London: Sampson Low, 1887.)

THIS is an interesting little book, and at the present time, when so much is said about the relations between the colonies and the mother country, it ought to appeal to a wide circle of readers. Mr. Inglis had a pleasant tour in New Zealand in 1885, and as he had been there twenty years before, he was able to note the progress that had been made in the political and social development of the colony. The results of his observations are presented in a fresh, clear, and lively style, and he will no doubt communicate to a good many of his readers a little of his own enthusiasm about the condition and prospects of "the new Great Britain of the Southern Seas."

Pictorial Geography of the British Isles. By Mary E. Palgrave. (London: Society for Promoting Christian Knowledge, 1887.)

THIS volume, although it could not be used as a text-book, might be of considerable service to young students of geography. The pictures would probably excite their interest, and would certainly tend to give definiteness to some of their conceptions. The letterpress is, upon the whole, very good. Beginning with a chapter on how our scenery was made, the author gives what she calls "a summary of British scenery," and then proceeds to describe the coasts, the mountains and hills, the plains and rivers, and the lakes and islands of the British Isles. There are also chapters on historical scenery and industrial geography.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The *Spectator* and Science.

IN a recent number the *Spectator* discussed a rumour that an American inventor had discovered a compound which possessed the peculiar property of exploding "forward only." The matter was discussed quite seriously, and it was pointed out that if the report were correct the defence of the northern frontier of India would be facilitated, as it would be possible to substitute parchment for metal in the construction of guns. So enamoured was the writer with this idea that it was again referred to in a subsequent article on "The New Air-Cannon."

Upon this I ventured to address to the editor a short note, in which I pointed out that as it is improbable that the most ingenious inventor will now upset the law that "action and reaction are equal and opposite," the rumour might be safely discredited.

More than one number has since appeared, but no steps have been taken to remove the misconception which the serious discussion of an absurdity must have produced on the minds of many non-scientific readers.

It may be, of course, that the *Spectator* would consider it a useful exercise to discuss what would follow if perpetual motion were realized or the circle squared. If so, there is nothing more to be said, except that the grave application of such speculations to questions like the defence of India is apt to mislead. It may perhaps be added that such a habit is not likely to increase the respect with which the opinions of the paper are received when it plunges hotly into a controversy of practical importance on scientific methods, such as that on the utility and morality of vivisection.

It is, I believe, a subject of regret to others besides myself that a journal, the attitude of which on other matters we admire, should betray such obvious ignorance on matters scientific. Before the *Spectator* discusses yachts' bottoms, new air-cannons, and compounds which explode forward only, it would be well for the management to obtain the advice of someone who has a competent knowledge of the scientific problems involved.

October 10. ARTHUR W. RÜCKER.

"Toeing" and "Heeling" at Golf.

I WAS much interested in the "Unwritten Chapter on Golf" (NATURE, Sept. 22, p. 502), signed with the well-known initials of "P. G. T." The mechanical explanation of "toeing and heeling," is however incomplete, as it does not take into account the torsion of the head and shaft caused by the impact of the ball on one side or other of the centre of percussion. If the ball be "heeled" (that is, goes off any point of the club-face nearer to the heel than the centre of percussion), the impact on the club-head causes it and the shaft to twist horizontally from right to left, a movement that is plainly felt in the hands as a disagreeable jar. Even should the club-face approach the ball in a line perpendicular to the direction of the intended drive, it no longer remains so on meeting the ball.

In the best driving the club follows the ball nearly to the extent of the swing, so that before they part company, the elasticity of the shaft twists the club-face back to or beyond its normal position, which should be perpendicular to the line of drive. If the ball happens to be "toed," the reverse movement takes place. A curve to the right in the course of the ball so invariably follows "heeling," even with the best drivers, and a curve to the left (but not so frequently) "toeing," that they have become recognized by golfers as cause and effect. I have always looked upon the torsional movement described as the main cause of the horizontal rotation given to the ball, and still think that any explanation which leaves this unconsidered is incomplete.

T. MELLARD READE.

Park Corner, Blundellsands, September 24.

[THE cause spoken of by Mr. Reade has occasionally some little effect, and I was fully aware of this long before I wrote my article. But, as most golfers know to their disgust, a ball can be badly "heeled" or "toed" when driven by a club or a cleek with the most untwistable of shafts. I should thus have confused instead of enlightening the ordinary reader, had I entered upon such a subsidiary question as *this* effect of torsion. For my own part, I believe that the most serious effects of torsion are produced *before the club reaches the ball.* This is not alluded to by Mr. Reade.

Mr. Reade uses the word "heeling" in the literal sense of "striking with the heel of the club," and has thus been led to state the opposite of the facts. If he will think over the result of the impulsive rotation of the club-head, which is due to smallness of torsional rigidity in the shaft, he will see that (supposing the club-face at impact to be exactly perpendicular to the course desired) hitting off the heel tends to produce what is commonly called "toeing":—*i.e.* skewing to the *left!* Similarly, hitting off the toe will produce what is commonly called "heeling":—*i.e.* skewing to the *right!* Thus the torsion of the shaft tends to mitigate ordinary "heeling" if the heel of the club be used, and to intensify it if the toe be used. Surely this would not have been easily understood by the ordinary reader of newspapers, for whom my article was written.—P. G. T.]

The Fertilization of the Coffee Plant.

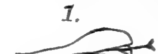
I SEND you the following notes on the fertilization of the coffee plant (*C. arabica*) which I made some time ago, and which may be interesting to those who study the subject.

Your readers are doubtless aware that coffee was cultivated some twelve years ago to a very large extent in Ceylon and South India, but owing to the attacks of leaf disease, the area has been rapidly reduced, except, I believe, in some parts of Coorg and Mysore, where the climate is drier, and the leaves suffer less from the fungus. It has now been largely replaced by tea.

The jasmine-like flowers of the coffee are borne in clusters in the axils of the leaves, and appear simultaneously all over the estates. After a prolonged drought of one or two months, or even more, at the beginning of the year, there is generally a heavy fall of rain, sometimes lasting only an hour or two, sometimes continuing for two or three days: the amount that falls must be enough to saturate the ground, and should not be less than one inch.

In from six to eight days from the time of the first shower, the flowers burst into full blossom, last for a day, and then drop off. On the evening before the blossom is fully out, if the flowers are examined it will be found that they are partially open, the stigma being protruded and receptive. During the

1.



night the hum of insects can be distinctly heard, and I am of opinion that the flowers are largely fertilized by night-flying insects which carry pollen from those flowers which happen to be open rather before the others, as some are delayed. On the following morning all the flowers will be found open, and the field of coffee presents a sheet of white. These flowers are frequented by immense numbers of bees, of two kinds, one about three-quarters of an inch long and black, the other smaller and with white bands round its abdomen. The stigmas now are covered with pollen, and the anthers bursting, and the larger of these bees may be seen buzzing from flower to flower sweeping up the grains of pollen between its front legs, and rolling them into balls. Long before evening all the anthers are exhausted of pollen, and the insects have departed. Besides bees some butterflies visit coffee, such as *Hypolimnas bolina*, *Papilio Polymnestis*, and two or three *Danaidæ*.

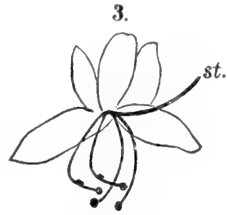
The coffee plant by being proterogynous is intended by Nature to be cross-fertilized, but owing to all the plants in one clearing being usually grown from seed of a single estate, there must be a great deal of interbreeding, more especially as all the coffee of Ceylon and most of South India is supposed to be descended from a single plant introduced into Batavia about two centuries ago. This may have something to do with the manifest deterioration in stamina of the younger coffee.

While on this subject I may mention the curious alteration in the position of the organs of *Clerodendron infortunatum* when flowering. This plant is proterandrous: at first the style hangs

2.

First position.

3.



Second position.

down, while the stamens are erect: as soon as the pollen is shed the stamens drop, while the style rises, and the stigma becomes receptive. The chief carriers of pollen in this plant are small ants.

T. F. BOURDILLON.

Quilon, S. Travancore, India, September 13.

Pearls of *Jasminum Sambac*.

DR. RIEDEL tells us in NATURE of September 15 (p. 461), that he possesses in his collection two melati pearls of *Jasminum Sambac*. I beg to say that, as in the case of tabasheer (see NATURE, vol. xxxvi. p. 30), and in that of cocoa-nut pearls (*ibid.* p. 158), Rumphius, in the almost inexhaustible treasure of his "Herbarium Amboinense," has already mentioned the pearls found also *in the flowers* of *Jasminum Sambac*. He gives in his fifth volume, in the 30th table, a good picture of that plant, and says in the description that a "dendrites" found in its flower in 1672 was sent to him two years after. It had the shape of a bud of the same vegetable, and was white-coloured and hard like silica or alabaster; moreover, it must have been without doubt a carbonate of calcium or some other alkaline earth, for Rumphius remarks that when the pearl was imprudently moistened with citric acid part of it was consumed by the acid. He also tells us that the common name given to all stone-concretions in fruits, wood, and animals by the Malayan people is "mestica," which corresponds well with Dr. Riedel's name of "mustica." ["In Celebe, ac præsertim in Macassara in *cunctis* sæpe fructibus dendrites quædam reperiuntur, ubi inter alia in hoc quoque frutice (Jasminum Sambac) talis detecta fuit, quæ loco floris inventa fuit anno 1672 in horto quodam Germani ibi habitantis, quæque mihi biennium post transmissa fuit. Formam habebat capituli, seu instar veri floris Bonga Manoor, nondum aperti, eratque alba et dura instar silicis seu alabastri; inventa autem fuit in tubo veri floris atque petiolum habebat ex ligno et lapide sensim compositum; quique hanc invenerat, imprudenter in mensa deposuerat, limonum succo commaculata, qui subito eius portionem consumserat."]

Frankfurt a. Oder. E. HUTH.

Action of River Ice.

IN the year 1854 the Yellow River burst through its left embankment near Kaifung-fu, and took a new course to the sea through the province of Shantung, occupying in its lower course the bed of the Tatsing-ho, which it scoured out and widened. Prior to the change the Tatsing-ho had been crossed at Tsiho-hien, about seventeen miles above Tsinan-fu, the provincial capital, by a stone bridge, seven arches of which remained standing in 1868 when Mr. Ney Elias visited the river (see Journal Roy. Geog. Soc. vol. xl. p. 6). Owing to the increased width of the channel, this bridge only reached about three-quarters of the distance across the river, and formed a serious impediment to the navigation.

Crossing the river myself at this site in April last, I made inquiries regarding the old bridge, but, as customary in China, could elicit nothing definite; the bridge had gone, and no visible obstruction existed in the channel.

When I arrived in Tientsin in July, the Yellow River was a frequent subject of conversation, and an old friend and well-known resident, Mr. J. G. Dunn, gave me the following account of a curious phenomenon witnessed by him when crossing the river in January 1883, on his way overland to Shanghai. The winter was a severe one, and the ice on the Yellow River at this spot was about three feet in thickness. Most of the ordinary traffic of the district was carried across the ice in carts and wheelbarrows; a space was, however, kept open for the ferry, by which usually the entire traffic of the high-road from the capital crosses the river, the ice being broken up every morning so as to leave a clear passage. Mr. Dunn preferred crossing the river by the ferry, as seeming to him more convenient and safer. From the boat he witnessed the extraordinary sight of a stone bridge floating on the upper surface of the ice; the piers had apparently been lifted bodily up, some of the arches were standing, still supported at one end by an abutment, but some had fallen, and were resting as they fell in order on the surface of the ice. The bridge had apparently floated some distance down; Mr. Dunn thought, from the confused answers of the people, a considerable distance, but from a comparison of the site it could scarcely have been more than a hundred yards or so. Strong westerly winds had been blowing for some time, and probably had, combined with other causes, induced a slight rise in the level of the water sufficient to break the connexion of the ice-sheet with the banks; the space kept open for the ferry had enabled it to move downwards by degrees under the influence of wind and current, and as the piers of bridges in China are usually built without cement they offered little obstruction to the movement.

From my own experience of the people in the district I can understand Mr. Dunn's mistake as to the distance the bridge was carried, and there can be no doubt that the bridge seen was the original one described by Mr. Elias.

The fact of a bridge lifted bodily off its piers by the floating power of river ice is probably unique, but in any case is sufficiently interesting to be worthy of record. I may add that the latitude of Tsiho is approximately 36° 40′ N., and the width of the river about 2000 feet.

THOS. W. KINGSMILL.

Shanghai, August 26.

Unusual Rainbow.

A RAINBOW after sunset is probably a somewhat unusual occurrence, but on the evening of September 11 I witnessed a very beautiful one from the band-stand in the Alfred Park, which is about the highest ground in Allahabad. Just before sunset the sky was more or less covered with high cirro-stratus, and promised one of the very highly-coloured sunsets common in the rainy season, while at the same time a slight storm, heralded by distant thunder, was coming up from the east. After spending a few minutes in the Public Library near the band-stand, I came out, and found the sun had set behind a bank of what Abercromby calls "rocky cumulus," or some other lumpy form of cloud, and was sending long shafts of alternate light and shadow across the southern half of the sky, while towards the north and overhead the clouds were lighted up with the most gorgeous colours. On turning to the east to see whether the flutings of the cloud-shadows appeared to meet in that quarter, as they usually do, I saw on the approaching shower, which was towards east-south-east, a beautiful double rainbow, both arcs being some 20° long, but stopping short of the horizon by $1\frac{1}{2}$° or 2°, to which height the earth-shadow already extended. Both bows seemed to the eye to be somewhat narrower than usual, and between and beyond them the fluted cloud-shadows appeared, by the illusion of perspective, to converge towards the anti-solar point. The bow must therefore have been produced by the light from a portion only of the sun's disk, shining through a hollow on the top of the western bank of cloud, and doubtless the same portion which illuminated the clouds directly overhead at the time of observation. The rainbow suffered no diminution of brightness where it was apparently crossed by the fluted shadows, the latter being far away in comparison with the bow-producing raindrops, which, of course, were in sunshine.

I regret that I am unable to send a photograph or sketch of the phenomenon, which was a most beautiful one, and must be of rare occurrence. I have never before seen anything similar, nor have I read anywhere a description of a rainbow after sunset.

Allahabad, India, September 18. S. A. HILL.

Occurrence of *Sterna anglica* in Belfast Lough.

IT may possibly interest some of your ornithological readers to know that towards the end of September a specimen of the gull-billed tern (*Sterna anglica*) was shot in Belfast Lough. The bird was placed in the hands of Mr. Darragh, of the Museum of that town, and brought by him to me for determination. On consulting the last edition of "Yarrell," I find that it does not appear to have been previously recorded from Ireland.

ROBERT O. CUNNINGHAM.

Queen's College, Belfast, October 8.

MODERN VIEWS OF ELECTRICITY.[1]

PART II.

III.

WE have now glanced through electro-static phenomena, and seen that they could be all comprehended and partially explained by supposing electricity to be a fluid of perfect incompressibility—in other words, a liquid—permeating everywhere and everything; and by further supposing that in conducting matter this liquid was capable of free locomotion, but that in insulators and general space it was as it were entangled in some elastic medium or jelly, to strains in which electrostatic actions are due. This medium might be burst, in a disruptive discharge, but easy flow could go on only through channels or holes in it, which therefore were taken to represent conductors; and it was obvious that all flow must take place in closed circuits.

To day I want to consider the circumstances of this flow more particularly: to study, in fact, the second division of our subject (see classification on page 532), viz. *Electricity in locomotion.*

I use the term "locomotion" in order to eliminate rotation and vibration: it is translation only with which we intend now to concern ourselves.

Consider the modes in which *water* may be made to move from place to place; there are only two: it may be pumped along pipes, or it may be carried about in jugs. In other words, it may travel *through* matter, or it may travel *with* matter. Just so it is with *heat* also: heat can travel in two ways: it can flow *through* matter, by what is called "conduction," and it can travel *with* matter, by what is called "convection." There is no other mode of conveyance of heat. You frequently find it stated that there is a third method, viz. "radiation"; but this is not truly a conveyance of *heat* at all. Heat generates radiation at one place, and radiation reproduces heat at another; but it is radiation which travels, and not heat. Heat only naturally flows from hot bodies to cold, just as water only naturally flows down hill; but radiation spreads in all directions, without the least attention to where it is going. Heat can only flow one way at any given point, but radiation travels all ways at once. If water were dissociated on one planet into its constituent gases, and if these recombined on another planet, it would not be water which travelled from one to the other, neither would the substance obey the laws of motion of water—water would be destroyed in one place, and reproduced in another; just so is it with the relation between radiation and heat.

Heat, then, like water, has but two direct modes of conveyance from place to place. For *electricity* the same is true. Electricity can travel with matter, or it can travel through matter; by convection or by conduction, but in no other known way.

Conduction in Metals.

Consider, first, conduction. Connect the poles of a voltaic battery to the two ends of a copper wire, and think of what we call "the current." It is a true flow of electricity among the molecules of the wire. If electricity were a fluid, then it would be a transport of that fluid; if electricity is nothing material, then a current is no material transfer; but it is certainly a transfer of electricity, whatever electricity may be. Permitting ourselves again the analogy of a liquid, we can picture it flowing through, or among, the molecules of the metal. Does it flow through or between them? Or does it get handed on from one to the next continually? We do not quite know; but the last supposition is often believed to most nearly represent the probable truth. The flow may be thought of as a perpetual attempt to set up a strain like that in a dielectric, combined with an equally perpetual breaking down of every trace of that strain. If the atoms be conceived as little conductors vibrating about and knocking each other, so as to be easily and completely able to pass on any electric charge they may possess, then, through a medium so constituted, electric conduction could go on much as it does go on in a metal. Each atom would receive a charge from those behind it, and hand it on to those in front of it, and thus may electricity get conveyed along the wire. Do not, however, accept this picture as anything better than a *possible* mode of reducing conduction to a kind of electrostatics—an interchange of electric charges among a series of conductors. If such a series of vibrating and colliding particles existed, then certainly a charge given to any point would rapidly distribute itself over the whole, and the potential would quickly become uniform; but it by no means follows that the actual process of conduction is anything like this. Certainly it is not the simplest mode of picturing it for ordinary purposes. The easiest and crudest idea is to liken a wire conveying electricity to a pipe full of marbles or sand conveying water; and for many purposes, though not for all, this crude idea suffices.

Leaving the actual mode of conveyance as unknown, let us review how much is certainly known of the process called conduction.

This much is certainly known:—

(1) That the wire gets heated by the passage of a current.

(2) That no trace of a tendency to reverse discharge or spring back exists.

(3) That the electricity meets with a certain amount of resistance or friction-like obstruction.

(4) That this force of obstruction is accurately proportional to the speed with which the electricity travels through the metal—that is, to the intensity of the current per unit area.

About this last fact a word or two must be said. The amount of electricity conveyed per second across a unit area is called the intensity of current; and experiment proves, what Ohm originally guessed as probable from the analogy of heat conduction, that this intensity is accurately proportional to the slope of potential which causes the flow; or, in other words (since action and reaction are equal and opposite), that a current in a conductor meets with an obstructive electromotive force exactly proportional to itself. The particular ratio between the two depends upon the particular material of which the conductor is composed, and is one of the constants of the material, to be determined by direct

[1] Continued from p. 5'1.

measurement. It is called its "specific conductivity" or its "specific resistance" according to the way it is regarded. The law here stated is called Ohm's law, and and is one of the most accurately-known laws there are. Nevertheless it is an empirical relation; in other words, it has not yet been accounted for—it must be accepted as an experimental fact. Undoubtedly, it is one of vast and far-reaching importance: it asserts a connexion between electricity and ordinary matter of a definite and simple kind.

Now if we think of this opposing electromotive force as analogous to friction, it is very easy to think of heat being generated by the passage of a current, and to suppose that the rate of heat-production will be directly proportional to the opposing force and to the current driven against it—as in fact Joule experimentally proved it to be.

But if we are not satisfied with this vague analogy, and wish to penetrate into the ultimate nature of heat and the mode in which it can be generated, then we can return to the consideration of a multitude of oscillating and colliding particles moving with a certain average energy which determines what we call the temperature of the body. If now one or more of these bodies receives a knock, the energy of the blow is speedily shared among all the others, and they all begin to move rather more energetically than before: the body which the assemblage of particles constitutes is said to have risen in temperature. This illustrates the production of heat by a blow or other mechanical means. But now, instead of *striking* one of the balls, give it an electric charge; or, better still, put within its reach a constant reservoir of electricity from which it can receive a charge every time it strikes it, and at the same time put within the reach of some other of the assemblage of particles another reservoir of infinite capacity which shall be able to drain away all the electricity it may receive. In practice there is no need of infinite reservoirs: all that is wanted is to connect two finite reservoirs, or "electrodes," as one might now call them, with some constant means of propelling electricity from one to the other, *i.e.* with the poles of a voltaic battery or a Holtz machine.

What will be the result of thus passing a series of electric charges through the assemblage of particles? Plainly the act of receiving a charge and passing it on will tend to increase the original motion of each particle; it will tend to raise the temperature of the body. In this way, therefore, it is possible to picture the mode in which an electric current generates heat.

But although this process may be used as a possible analogy, it cannot be a true and complete statement of what occurs; for it is essentially the mode of propagation of *sound*. Sound travels at a definite and known velocity, being a mechanical disturbance handed on from particle to particle in the manner described. But heat, being some mode of motion, must also be handed on after some analogous fashion, so that when heat is supplied to one point of a mass it spreads or diffuses through it. It is difficult to suppose the conduction of heat to be other than the handing on of molecular quiverings from one to another, and yet it takes place according to laws altogether different from those of the propagation of the gross disturbance called sound. The exact mode of conduction of heat is unknown, but, whatever it is, it can hardly be doubted that the conduction of electricity through metals is not very unlike it, for the two processes obey the same laws of propagation: they are both of the nature of a diffusion, they both obey Ohm's law, and a metal which conducts heat well conducts electricity well also.

Conduction in Liquids.

Leaving the obscure subject of conduction in metals for the present, let us pass to the consideration of the way in which electricity flows through liquids. By "liquids," in the present connexion, one more particularly means definite chemical compounds, such as acids, alkalies, salt and water, and saline solutions generally. Some liquids there are, like alcohol, turpentine, bisulphide of carbon, and possibly water, which, when quite pure either wholly or very nearly decline to conduct electricity at all. Such liquids as these may be classed along with air and gases as more or less perfect dielectrics. Other liquids there are, like mercury and molten metals generally, which conduct after precisely the same fashion as they do when solid. These therefore are properly classed among metallic conductors.

But most chemical compounds, when liquefied either by heat or by solution, conduct in a way peculiarly their own; and these are called "electrolytes."

The present state of our knowledge enables us to make the following assertions with considerable confidence of their truth:—

(1) Electrolytic conduction is invariably accompanied by chemical decomposition, and in fact only occurs by means of it.

(2) The electricity does not flow *through*, but *with*, the atoms of matter, which travel along and convey their charges something after the manner of pith balls.

(3) The electric charge belonging to each atom of matter is a simple multiple of a definite quantity of electricity, which quantity is an absolute constant quite independent of the nature of the particular substance to which the atoms belong.

(4) Positive electricity is conveyed through a liquid by something equivalent to a procession of the electro-positive atoms of the compound in the direction called the direction of the current; and at the same time negative electricity is conveyed in the opposite direction by a similar procession of the electro-negative atoms.

(5) On any atom reaching an electrode it may be forced to get rid of its electric charge, and, combining with others of the same kind, escape in the free state: in which case visible decomposition results. Or it may find something else handy with which to combine—say on the electrode or in the solution; and in that case the decomposition, though real, is masked, and not apparent.

(6) But, on the other hand, the atom may cling to its electric charge with such tenacity as to stop the current: the opposition force exerted by these atoms upon the current being called polarization.

(7) No such opposition force, or tending to spring back, is experienced in the interior of a mass of fluid: it occurs only at the electrodes.

It would take too long to go into the evidence for these statements and to adduce examples: I will try and make the process of electrolytic conduction clearer by reverting to our mechanical analogies and models.

Looking back to Figs. 5 and 6 (p. 559), we see illustrations of metallic conduction and of dielectric induction. In each case an applied electromotive force causes some movement of electricity; but, whereas in the first case it is a continuous almost unresisted movement or steady flow through or among the atoms of matter, in the second case it is a momentary shift or displacement only, carrying the atoms of matter with it, and highly resisted in consequence:—resisted, not with a mere frictional rub which retards but does not check the motion, but by an active spring back force, which immediately checks all further current, produces what we call "insulation," and ultimately, when the propelling force is removed, causes a quick reverse motion or discharge. But the model is plainly an incomplete one: for what is it that the atoms are clinging to? What is it ought to take the place of the *beam* in the crude mechanical contrivance? Obviously another set of atoms, which are either kept still or urged in the opposite direction by a simultaneous opposite displacement of negative electricity. We are to picture two or any number of rows of beads, each row threaded on its appropriate cord; the cords alternately representing positive and negative electricity respectively, and being simultaneously displaced in opposite directions by any applied E.M.F. The beads threaded on any one cord have, in a dielectric, elastic attachments to those on some opposite cord, and thus continuous motion of the cords in opposite directions is prevented: only a slight displacement is permitted, followed by a spring back and oscillation after the fashion already described.

Very well; now picture the elastic connexions between the beads all dissolved, and once more apply a force to each cord, moving half of them one way and the alternate half the other way, and you have a model illustrating an electrolyte and electrolytic conduction. The atoms are no longer attached to each other, but they are attached to the cord. In the first respect, an electrolyte differs from a dielectric; in the second, it differs from a metal.

Moreover, electrolytic conduction is perceived to be scarcely of the nature of true conduction: the electricity does not slip through or among the molecules, it goes with them. The constituents of each molecule are free of each other, and while one set of atoms conveys positive electricity, the other set carries negative electricity in the opposite direction; and so it is by a procession of free atoms that the current is transmitted. The process is of the nature of convection: the atoms act as carriers. Free locomotion of charged atoms is essential to electrolysis.

In order to compare with Figs. 5 and 6, so as to bring out the points of difference, Fig. 13 is drawn. The beads representing one set of atoms of matter are tightly attached to the cord, no trace of slip between them being permitted, but otherwise they are free, and so are represented as supported merely by rings sliding freely on glass rods. The only resistance to the motion, beside the slight friction, is offered at the electrode, which is typified by the spring-backed knife-edge, Z. This is supposed to be able to release the beads from the cord when they are pressed against it with sufficient force. The cling between the bead and cord (*i.e.* between each atom and its charge) is great enough to cause a perceptible compression of the springs, and accordingly to bring out a recoil force in imitation of polarization.

The piece of cord accompanying each bead on its journey (*i.e.* the length between it and the next bead) represents the atomic charge, and is a perfectly constant quantity: the only variation permissible in it is that some kinds of atoms have twice as much, or are twice as far apart on their cord, and these are called by chemists dyad atoms; another kind has three times as much, another four, and so on; these being called triad, tetrad, &c.

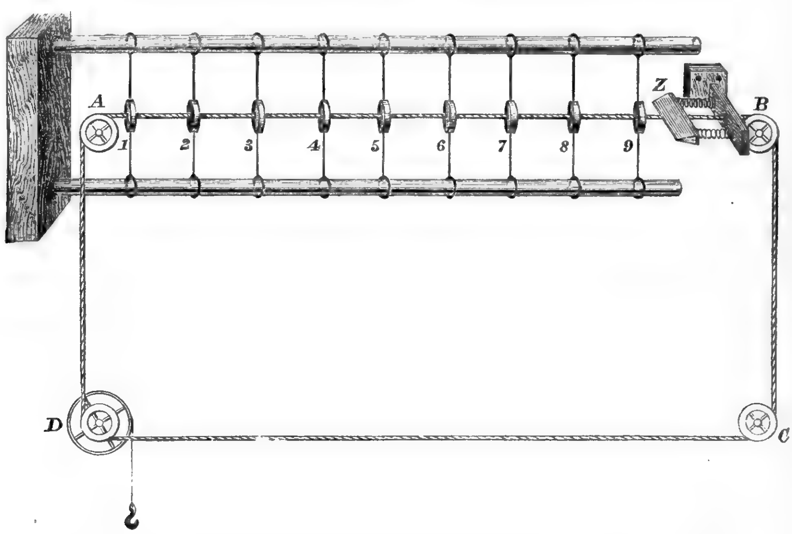


FIG. 13.—Crude mechanical analogy, illustrating a few points in a circuit partly *electrolytic*.

If the cord be taken to represent positive electricity, the beads on it may represent atoms of hydrogen, or other monad *cation*, travelling down stream to the cathode. Another cord representing negative electricity may be ranged alongside it, with its beads twice as far apart, to represent the atoms of a dyad *anion*, like oxygen. If the cords are so mechanically connected that they must move with equal pace in opposite directions, we have a model illustrating several important facts. The number of oxygen atoms liberated in a given time will be obviously half the number of hydrogen atoms set free in the same time, and will therefore in the gaseous state occupy but half the volume. For any element whatever, the number of atoms liberated in any time is equal to the number of atoms of hydrogen liberated in the same time, divided by the "valency" of the element as compared with hydrogen. This law was discovered by Faraday, and appears to be precisely true; and inasmuch as the relative weight of every element is known with fair accuracy, it is easy to calculate what weight of substance any given current will deposit or set free in an hour, if we once determine it experimentally for any one substance.

We may summarize thus:—

If we apply E.M.F. to a metal we get a continuous flow, and the result is heat.

If we apply it to a dielectric we get a momentary flow or displacement, and the result is the potential energy of "charge."

If we apply it to an electrolyte we again get a continuous flow, and the result is chemical decomposition.

There are a large number of important points to which I might direct your attention in the mode by which an electric current is conveyed through liquids, but I will specially select one, viz. that it is effected by a procession of positively charged atoms travelling one way, and a corresponding procession of negatively charged atoms the other way.

Whatever we understand by a positive charge and a negative charge, it is certain that the atoms of, say a water molecule, are charged, the hydrogen positively, the oxygen negatively; and it is almost certain that they hang together by reason of the attraction between their opposite charges. It is also certain that when an electromotive force—*i.e.* any force capable of propelling electricity—is brought to bear on the liquid, the hydrogen atoms travel on the whole in one direction, viz. down hill, and the oxygen atoms travel in the other direction, viz. up hill; using the idea of level as our analogue for electric potential in this case. The atoms may be said to be driven along by their electric charges just as charged pith balls would be driven along; and they thus act as conveyers of electricity, which otherwise would be unable to move through the liquid.

Each of this pair of opposite processions goes on until it meets with some discontinuity—either some change of liquid, or some solid conductor. At a change of liquid another set of atoms continues the convection, and nothing very particular need be noticed at the junction; but at a solid conductor the stream of atoms must stop: you cannot have locomotion of the atoms of a solid. The obstruction so produced may stop the procession, and therefore the current, altogether; or on the other hand the force driving the charges forward may be so great as to wrench them free, to give the charges up to the electrode which conveys it away by common conduction, and to crowd the atoms together in such a way that they are glad to combine with each other and escape.

Now notice the fact of the two opposite processions. One cannot have a procession of positive atoms through a liquid without a corresponding procession of negative ones. In other words, an electric current in a liquid necessarily consists of a flow of positive electricity in one direction, combined with a flow of negative electricity in the opposite direction. And if this is thus proved to occur in a liquid, why should it not occur everywhere? It is at least well to bear the possibility in mind.

Another case is known where an electric current certainly consists of two opposite streams of electricity, viz. the case of the Holtz machine. While the machine is being turned, with its terminals somehow connected, the glass plate acts as a carrier conveying a charge from one collecting comb to the other at every half revolution; but, whereas it carries positive electricity for one half a rotation, it carries negative for the other half. The top of the Holtz disk is always, say, positively charged, and is travelling forward, while the bottom half, which is travelling backward at an equal rate, is negatively charged.

In the Holtz case the speeds are necessarily equal, but the charges are not. In the electrolytic case the charges are necessarily equal, but the speeds are not. Each atom has its own rate of motion in a given liquid, independently of what it may happen to have been combined with. This is a law discovered by Kohlrausch. Hydrogen travels faster than any other kind of atom; and on the sum of the speeds of the two opposite atoms in a compound the conductivity of the liquid depends. Acids therefore in general conduct better than their salts.

OLIVER J. LODGE.

(*To be continued.*)

JOSEPH BAXENDELL, F.R.S.

WE have already announced the death of Joseph Baxendell, an event which took place on Friday, the 7th inst., at the Observatory, Southport.

Born at Manchester in 1815, he had not the advantage of a thorough scientific training, such an education being much less frequent at that early period than it is at the present day. On the contrary, he had to make his way in the world, and went to sea when quite a youth. We are all of us moulded by circumstances, and while Baxendell no doubt inherited an aptitude for science, yet the particular bent which this took was unquestionably determined by the circumstances of his profession. An intelligent seaman cannot fail to be impressed with the importance of astronomy and meteorology, and it was in these two sciences that Baxendell especially distinguished himself in after life.

Meanwhile, notwithstanding the engrossing duties of a sailor, his energy and perseverance in the pursuit of science were such that he was enabled to supplement the deficiencies of his limited education, acquiring a knowledge of mathematics which was of great service to him in his investigations. A training of this kind is well qualified to produce a mature and thoughtful student of Nature, and it had this effect upon Baxendell. Owing to a retiring disposition, he was not much seen in general scientific society, but was, on the other hand, very highly esteemed by students like himself. A gathering of such students usually takes place once a fortnight during the winter months at the rooms of the Manchester Literary and Philosophical Society. At these meetings Baxendell was a most regular attendant, and he ultimately became Secretary of the Society as well as editor of its publications. It is in the Memoirs and Proceedings of this Society that most of his scientific contributions will be found, and in astronomy it is only necessary to notice his catalogue of variable stars, which is very highly esteemed by all observers.

Baxendell's contributions to meteorology are very important, and in one branch of this science he may claim to be the pioneer. In 1871, from an analysis of eleven years' observations of the Radcliffe Observatory, Oxford, he came to the conclusion that the forces which produce the movements of the atmosphere are more energetic in years of maximum than in years of minimum sun-spot activity. This conclusion has now been confirmed in various directions by other observers. We have heard it objected that Baxendell generalized from a comparatively small number of observations, but in a question like this such a procedure is essential to the pioneer. His task is to deduce with a mixture of boldness and prudence something of human interest out of the mass of observations already accumulated, and thus to stimulate meteorologists not only to go on with their labour, but to cover more ground in the future than they have covered in the past. Baxendell's procedure in this respect has been abundantly justified by the fact that many other men of science are now following in his footsteps.

It is believed that he was the first to propose the use of storm-signals which are now universally adopted by all maritime nations. He likewise foretold the long drought of 1868, and enabled Manchester to take precautionary measures which had the effect of rendering the inconvenience less severe. As an astronomer and meteorologist Baxendell was naturally interested in a study of the sun, and was an independent discoverer of the fact that the faculæ which accompany sun-spots are for the most part thrown behind them—the word *behind* having reference to the direction of rotation of our luminary. It was, we believe, his opinion that the behaviour of sun-spots is intimately connected with that of meteoric matter around the sun. Without asserting the exact nature of the bond between these two phenomena, we think that various students of the sun's surface are now inclined to be of this opinion.

He was a Fellow of the Royal Society and of the Royal Astronomical Society. He was likewise a corresponding member of the Royal Society of Königsberg, of the Scientific and Literary Academy of Palermo, and of the National Observatories of France, Germany, and Italy. For some years he enjoyed the use of his friend Mr. Robert Worthington's observatory at Crumpsall. On the death of the Rev. H. H. Jones, in 1859, he was appointed Astronomer to the Manchester Corporation. Latterly his health forced him to reside at Southport, in the neighbourhood of which he continued his observations until his death.

BALFOUR STEWART.

NOTES.

ONE of the most illustrious men of science of the present century, Prof. Gustav Kirchhoff, died at Berlin on Monday. He was sixty-three years of age. Next week we shall have something to say about his services to science.

THE *Gartenflora* for October announces the death of Dr. Robert Caspary, for many years Professor of Botany in the University of Königsberg. He was a native of Königsberg, where he was born in 1818, and the immediate cause of his death was a fall down stairs. The deceased was not a prolific writer, yet he was well known to botanists as a critical authority on the Nymphæaceæ. Local botany and the investigation of abnormal growths occupied much of his leisure time.

MR. ROBERT HUNT, F.R.S., died on Monday, at his residence in London. Mr. Hunt was born in 1807 at Devonport, was the Keeper of Mining Records at the Museum of Practical Geology, and was the first-appointed Professor of Mechanical Science to the Government School of Mines.

PROF. JOHANN KONRAD ULLHERR, a well-known mathematician, died at Kaufbeuren on September 28, aged sixty-seven.

THE last number of the Journal of the China Branch of the Royal Asiatic Society (vol. xxi. new series, Nos. 5 and 6) contains an obituary notice of the eminent Chinese scholar, Alexander Wylie, who died early in the present year. He was the author or translator of a considerable number of works of elementary science into Chinese. Amongst them were treatises on mechanics and arithmetic, translations of De Morgan's "Algebra," Loomis's "Geometry," Herschel's "Astronomy," "Euclid," and Main on the steam-engine. He also compiled a list of stars and astronomical terms in Chinese and English, and a paper on the Mongolian astronomical instruments in Pekin. It may be interesting also to notice that the same number of the Journal contains a sketch of the late Dr. Hance, of Canton and Whampoa, who was well known in Europe by his botanical writings, and whose death was noticed not long since in NATURE. The writer appends a complete list of Dr. Hance's papers on botanical subjects, beginning with the year 1848. There are in all 119 of these relating to Chinese botany.

ON the 9th inst. an interesting ceremony took place in the town of Le Mans (Sarthe). It was the unveiling of a statue erected to the memory of Pierre Belon, the celebrated zoologist and traveller of the sixteenth century. Pierre Belon was born in 1518. He was one of the first who established the homologies between the skeletons of different vertebrates. Over a century before the creation of the Jardin des Plantes in Paris, he had formed two botanical gardens. It was he who brought to France the first cedar planted there. It is a common tradition that the first specimen of this tree was brought by de Jussieu, but Belon had anticipated him by a century. The monument is very handsome. Belon is represented seated and holding a book. The expenses were covered by a public subscription.

THE Council of the Senate of the University of Cambridge report, respecting the site for the Geological Museum, that, as the price required by Downing College is £5390, the University do not proceed further with the proposal. They recommend that a syndicate be appointed to consider the plans, and, if necessary, to procure fresh plans for the erection of the Sedgwick Memorial Museum on the site to the east of the new chemical laboratory, and to present plans to the Senate for their approval before the division of the Lent Term, 1888. The plans are to be so arranged that a part of the building sufficient for the purposes of teaching and study might, with the consent of the Sedgwick Memorial Committee, be erected with the money now in their hands.

THE thirty-fourth annual meeting of the German Geological Society was held at Bonn on September 26, under the Presidency of Prof. Römer (Breslau). The following were among the papers read: on the dolerites of Londorf, near Giessen, by Prof. Streng (Giessen); on the basaltic rocks of the Vogelsberg, by the same; on the chalk of Umtamfuna, in Natal, the Upper Silurian Eurypterus dolomite of Gaaden, near Kiel, and on the mollusk fauna (fifty-four species) in the Central Oligocene of Itzehoe, by Dr. Gottsche (Hamburg); and on fossil footprints in the New Red Sandstone in Thuringia, by Dr. J. G. Bornemann (Eisenach).

A CORRESPONDENT in Trinidad writes:—"We have a Committee appointed here for the purpose of endeavouring to determine the influence the moon has upon vegetation. It arose in this way: I found that in cutting timber, bamboo, pruning cacao, sowing seeds, planting provisions, the phase of the moon was always considered, and in consequence much time was lost. In England, years ago, I heard the same idea and disproved it by experiment. Here it has such a hold upon the Spanish section of the community that it is no use endeavouring to combat it by speaking or writing, and therefore experiment has been authorized. The superstition is so infectious that the Hon. Director of Public Works, after many years' experience, was inclined to believe in it. But after perusing the works of Arago which I lent him, and Lardner's 'Astronomy,' he has refrained from asserting anything, but has devoted himself to the experiments to prove the truth or falsity of the theory. When you aver that no notice is taken of the moon in agricultural operations in England, you are met by the reply, 'Oh, the moon has more influence in the equatorial districts,' &c., &c."

AN Imperial Decree has been issued by the Mikado of Japan sanctioning regulations for the establishment of meteorological observatories, at the public expense, in the country. The regulations provide that the Central Observatory shall be situated in Tokio, and local observatories at such convenient places as may be designated by the Home Minister, without whose consent the local authorities may not establish observatories. The Central Observatory is to be under the Home Minister, while the local observatories are to be under the respective local Governments. The cost of maintaining local observatories is to be defrayed out of local taxation; they are to communicate and correspond with the Central Observatory according to departmental regulations which shall be made by the Home Minister.

THE Aristotelian Society has decided to print, at the close of each session, an abstract of its proceedings. The first number, edited by Prof. Dunstan, has just appeared. It represents the work done during the eighth session, which terminated last June, and contains lengthy abstracts of many papers of interest, among them being: the ultimate questions of philosophy, by Prof. Bain; the re-organisation of philosophy, by Mr. Shadworth Hodgson; Neo-Kantism in its relation to science, by Mr. Romanes and Mr. Bernard Bosanquet; recent psycho-physical researches, by Dr. Cattell. The ninth session will open on November 7 with an address by the President. Among the papers to be read is one by Mr. Romanes, on Darwinism in relation to design; and one by Prof. Bain, on the demarcations and definitions of the subject sciences.

A BOOK on tattooing, by Wilhelm Joest, will shortly be published by Messrs. A. Asher and Co., at Berlin. In this elaborate work, which will be fully illustrated, the author will present much information which he has collected during his extensive travels. He will also thoroughly discuss the question as to the motives which have led to the practice of tattooing.

A NEW periodical is being issued by Julius Springer, of Berlin, who has sent us a copy of the first number. The periodical is called *Zeitschrift für den Physikalischen und Chemischen Unterricht*, and is edited by Dr. Fritz Poske, with the aid of Dr. E. Mach and Dr. B. Schwalbe. The editor's principal object will be to provide an adequate exposition of the best ideas of the age as to the methods of instruction in chemistry and physics.

We have received the first number of the *Journal of Morphology*, edited by Mr. C. O. Whitman, with the co-operation of Mr. E. P. Allis, Junr., and published by Messrs. Ginn and Co., Boston, U.S.A. The journal is to be devoted principally to embryological, anatomical, and histological subjects. It will be published at irregular intervals, new numbers appearing "as often as the requisite material is furnished." The second number will be issued in November, and will complete the first volume.

Messrs. Macmillan have just issued a second edition of "The Growth of the Recruit and Young Soldier," by Sir W. Aitken. The work was originally published twenty-five years ago, and the writer's main object was to suggest a judicious selection of "growing lads" for the army and a regulated system of training recruits. In the present edition the subject-matter has been recast to meet the requirements of the time. Sir W. Aitken holds that the circumstances which justified the first publication are far more pronounced now than they were twenty-five years ago.

A seventh edition of Prof. H. Alleyne Nicholson's well-known "Manual of Zoology" (Blackwood), has just been issued. While the general plan of the original book has been retained, the work has been recast. A considerable number of fresh illustrations have also been added.

The Madras Literary Society has issued its Journal of Literature and Science for the session 1886-87. It contains, besides other good papers, notes on the cyclone of November 9, 1886, by C. Michie Smith; on a new method of finding the factors of any given number: a contribution to the theory of numbers, by J. K. Winter; on the reputed suicide of scorpions, by A. G. Bourne; the cosmogony of the Vedas, by the Rev. Maurice Phillips; and the pearl oyster of the Gulf of Manaar, by H. S. Thomas.

The second Annual Report of the City of London College Science Society has been sent to us. The Society has steadily grown during the past year; and it is claimed that in general interest and thoroughness of treatment the papers read at the evening meetings were fully up to the standard previously set.

The General Electric Apparatus Company have issued a third edition of their Illustrated Catalogue of Electric Lighting Plant and Material, and a sixth edition of their Illustrated Catalogue of Electric Bells. These catalogues present much interesting evidence as to the growth of a new and important industry.

A new tetrahydric alcohol, $C_{10}H_{20}O_4$, belonging to the series CnH_2nO_4, of which it is as yet the only known member, has been synthetically prepared in the laboratory of M. Friedel, by M. Combes (*Ann. de Chim. et Phys.*, October 1887). It is of special interest to organic chemists, as being the first tetrahydric alcohol which has been prepared by direct synthesis, and the discovery is but one of many exceptionally rich ones which have followed the application, by M. Combes, of the well-known aluminium-chloride reaction of MM. Friedel and Crafts, to the fatty series. While studying the action of chloride of aluminium upon acetyl chloride, it was found that a remarkable organo-metallic compound, $C_{12}H_{14}O_6Al_2Cl_8$, was formed, consisting of crystalline lamellæ showing strong colours in polarized light. These lamellæ dissolved in water with great violence, evolving carbonic and hydrochloric acid gases, and extraction with chloroform and subsequent distillation showed that the decomposition by water had resulted in the formation of a new ketone of the composition $C_5H_8O_2$ and constitution $CH_3—CO—CH_2—CO—CH_3$. To this ketone M. Combes gave the name acetyl-acetone, and it was by the hydrogenation of this substance that the new tetrahydric alcohol was obtained. The reduction was effected by means of hydrochloric acid and sodium amalgam, and, when the reaction was completed, a second extraction with chloroform and subsequent evaporation yielded a syrup consisting of a mixture of two compounds: one, boiling at 177°, being another new glycol of the composition $CH_3—CHOH—CH_2—CHOH—CH_3$; and the other, passing over at 270°, consisting of the anhydride of the tetrahydric alcohol, which appears to lose the elements of water very readily. The constitution of this alcohol is pretty conclusively shown to be

$$\begin{array}{l} CH_3—COH—CH_2—COH—CH_3. \\ \quad\quad\;\; | \quad\quad\quad\quad\quad\; | \\ CH_3—COH—CH_2—COH—CH_3 \end{array}$$

It should be stated that the above are all general reactions, and open a wide field for further research; indeed, there can be little doubt that the richness of the results obtained through their first application by M. Combes will only prove an earnest of greater success in the future.

A correspondent of the *Nation* for October 6, "R. T. H.," writing from Arkadelphia, Ark., directs attention to what he calls a "scientific revival" in the Southern States of America. "In several of the States," he says, "the question of elementary physiology and hygiene in the public schools has come before the Legislature this year, and, though generally decided adversely, opinion in its favour is growing, and it is a most active leaven. In the Colleges there is a great advancement, and technical studies and natural history may be said to be enjoying a 'boom.' The University of North Carolina has a small but modern Natural History Department. The University of Tennessee is also waking up in this respect. The Mississippi State Agricultural College is exceptionally modern, and the Arkansas State Industrial University has recently added a competent naturalist to its faculty. Tulane University of New Orleans is also paying attention to biologic studies. The most interesting struggle, however, is in Texas, where, owing to an inexplicable tangle, we have the spectacle of the most progressive University in the South handicapped by the most unreasonable embarrassments—the rivalry of another State institution and many sectarian Colleges. But there is no room to doubt that in a few years the struggle will end in the University being unfettered, and becoming a centre from which will radiate much intelligent thought." The writer says that one great obstacle to biologic teaching in the South—opposition to the importation of teachers from the North—is being in part obviated by the fact that young Southerners are beginning to be found who have been abroad or North. For all those who are fitted, good places are made.

An earthquake is reported from Constantinople. It occurred at 10 a.m. on September 30, and lasted for seventeen seconds. Violent shocks were noticed on October 4 and 5 on the Greek mainland, the Ionian Islands, the Cyclades, and the Peloponnese.

Several earthquakes are reported from the south-east of Hungary, many having occurred during the last weeks of September. The most severe one was noticed at St. Peter's (Temesvar), one shock lasting for three seconds, and many houses being greatly damaged. The direction of the shock was from south-west to north-east.

A steamer which arrived lately at New York brought information from Navassa, an island lying between Hayti and Jamaica, that on September 23 an earthquake occurred there, which seemed to send a tremor through the whole island. No damage was done.

Prof. Hudson is giving a course of lectures in the Michaelmas Term of 1887 at King's College once a week, on Wednesdays, at 7 p.m., on "Elementary Applications of the Differential and Integral Calculus," beginning with applications to dynamics.

The additions to the Zoological Society's Gardens during the past week include a Crested Lark (*Alauda cristata* ♂) from India, presented by Colonel Verner; two Proteus (*Proteus *anguinus*) from the Caves of Adelsburg, presented respectively by Prof. W. H. Corfield, F.Z.S. and Dr. E. Rickards; a Spotted Salamander (*Salamandra maculosa*), European, presented by Mr. Alban Doran; a Gorilla (*Anthropopithecus gorilla* ♂), three Pluto Monkeys (*Cercopithecus pluto*), an Erxleben's Monkey (*Cercopithecus erxlebeni* ♀) from West Africa, deposited; two Coypus (*Myopotamus coypus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

New Minor Planet.—A new minor planet, No. 270, was discovered by Dr. Peters on October 13.

Olbers's Comet.—The following corrected elements and ephemeris for this object are by Herr O. Tetens (*Astr. Nachr.* No. 2806):—

$$T = 1887 \text{ October } 8{\cdot}4938 \text{ Berlin M.T.}$$

$$\left.\begin{aligned}\pi &= 149^\circ\ 48'\ 7''\\ \Omega &= 84\ \ 27\ \ 40\\ i &= 44\ \ 32\ \ 53\end{aligned}\right\}\text{Mean Eq. } 1887{\cdot}0$$

$$\phi = 68\ \ 35\ \ 36$$

$$\log q = 0{\cdot}078899$$

$$x = [9{\cdot}854835]\, r \sin (v + 237^\circ\ 35'\ 31'')$$
$$y = [9{\cdot}972351]\, r \sin (v + 168\ \ 39\ \ 22)$$
$$z = [9{\cdot}891623]\, r \sin (v + 95\ \ 54\ \ 3)$$

Ephemeris for Berlin Midnight.

1887	R.A. h. m. s.	Decl. ° ′	log *r*.	log Δ.
Oct. 22	13 6 38	21 47·3 N.	0·0856	0·2795
24	13 15 8	20 43·7		
26	13 23 28	19 59·3	0·0898	0·2831
28	13 31 39	19 14·5		
30	13 39 39	18 29·4	0·0950	0·2875
Nov. 1	13 47 30	17 44·1		
3	13 55 10	16 58·8	0·1010	0·2924
5	14 2 41	16 13·5		
7	14 10 2	15 28·3	0·1077	0·2979
9	14 17 13	14 43·5		
11	14 24 15	13 59·0 N.	0·1152	0·3037

The brightness on October 26 will be 1·48, and on November 11, 1·20; that on August 27 being taken as unity.

Southern Double Stars.—A welcome addition to the still somewhat scanty supply of observations of southern double stars is contained in the *Monthly Notices* for June 1887, which furnishes a series of measures of stars in relative motion recently made at the Sydney Observatory, special attention having been paid to the binaries α Centauri and γ Coronæ Australis. The mean of eighteen measures of position-angle and distance of the components of α Centauri gives for the epoch 1886·47: angle = 202°·3, distance = 15″·10; whilst the mean of four measures of difference of R.A. and of declination of the components gives for 1886·55: angle = 201°·0, distance = 14″·87. Referring to *Monthly Notices*, vol. xlvi. p. 340, we find that for the former epoch the computed places are as follows:—

Downing-Elkin orbit-angle = 202°·6, distance = 15″·11.
Powell orbit-angle = 201·8, distance = 15·26.

These orbits give for the periodic time of α Centauri the values 76 years and 87 years respectively; it appears, however, that several more years' observation will be necessary to decide which of these is the more accurate. Of γ Coronæ Australis eight measures were made at Sydney in 1886. The most satisfactory orbit of this binary hitherto published is that computed by Mr. Gore (*Monthly Notices*, vol. xlvi. p. 104), and the errors of the computed quantities as compared with the observations which have been published since the computations were made are:—

Epoch.	Observed angle.	Error.	Observed distance.	Error.
	°	°	″	″
1881·72	225·5	+ 1·8	1·38	− 0·02
1883·62	217·8	− 1·3	1·62	− 0·33
1886·615	200·6	− 4·6	1·45	− 0·33

The first two of these observations were made at Cincinnati, and published in the *Observatory*, vol. ix. p. 234, the last at Sydney. Mr. Gore's orbit gives 1886·53 as the time of periastron passage; it is very desirable, therefore, that this pair should be repeatedly measured during the next few years in order that the small corrections to the elements which appear to be required may be accurately determined.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 OCTOBER 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 October 23

Sun rises, 6h. 39m.; souths, 11h. 44m. 26·2s.; sets, 16h. 50m.; decl. on meridian, 11° 24′ S.: Sidereal Time at Sunset, 18h. 57m.

Moon (at First Quarter October 23, 18h.) rises, 13h. 37m.; souths, 18h. 1m.; sets, 22h. 28m.; decl. on meridian, 18° 47′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	9 7	13 14	17 21	21 14 S.
Venus	3 15	9 21	15 27	0 30 N.
Mars	1 25	8 25	15 25	10 57 N.
Jupiter	7 50	12 36	17 22	14 49 S.
Saturn	22 40*	6 28	14 16	19 6 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	σ Capricorni	5½	20 18	21 29	113 327
24	θ Capricorni	4	16 32	17 52	78 277
26	70 Aquarii	6	19 50	21 7	88 323
28	B.A.C. 81	6½	23 32	0 21†	85 12
29	26 Ceti	6½	19 22	20 37	87 274
29	29 Ceti	6½	22 40	23 57	141 294

† Occurs on the following morning.

Oct.	h.	
27	3	Mercury at greatest elongation from the Sun, 24° east.
28	—	Venus at period of greatest morning brilliancy.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·3	81 16 N.	Oct. 23,	3 51 *m*
			,, 28,	3 30 *m*
Algol	3 0·8	40 31 N.	,, 24,	2 32 *m*
			,, 26,	23 21 *m*
ζ Geminorum	6 57·4	20 44 N.	,, 25,	0 0 *M*
S Cancri	8 37·5	19 26 N.	,, 26,	2 10 *m*
S Ursæ Majoris	12 39·0	61 43 N.	,, 25,	*M*
U Coronæ	15 13·6	32 4 N.	,, 24,	1 58 *m*
T Ophiuchi	16 27·3	15 53 S.	,, 25,	*M*
U Ophiuchi	17 10·8	1 20 N.	,, 23,	0 51 *m*
		and at intervals of		20 8
η Aquilæ	19 46·7	0 43 N.	Oct. 24,	20 0 *m*
S Aquilæ	20 6·4	15 17 N.	,, 23,	*m*
δ Cephei	22 25·0	57 50 N.	,, 25,	1 0 *M*
			,, 28,	19 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near β Tauri	78°	30° N.	Swift.
From Canis Minor	105	12 N.	Swift; streaks.
,, Cancer	135	20 N.	Very swift.

GEOGRAPHICAL NOTES.

MM. Bonvalot, Capus, and Pepin, who have just returned to France from an extensive journey in Central Asia, are credited with having been the first to cross the Pamir. They may certainly have been the first to take the particular north to south route traversed by them, but the Pamir has been crossed and very thoroughly explored in recent years by several Russian travellers, while Mr. Ney Elias has done much to make known its peculiar features. The three French travellers seem to have suffered much during their journey across this mountain mass, especially from the extreme cold and the rarefaction of the air. They had frequently to throw themselves down upon the snow from exhaustion. These enforced halts were taken advantage of by M. Capus to register the pulse-beats of himself and his companions; he found the mean number per minute rise to 170.

THE Danish Expedition to the coast of Northern Greenland has just returned to Copenhagen. It has been absent since the spring of 1886, and was directed by Herr C. Ryder. During the two summers it was enabled to proceed from lat. 72° to lat. 74½°. It investigated the Upernivik glacier during the winter. Many meteorological, magnetic, and astronomical observations were made, many anthropological measurements were taken, and botanical and zoological collections have been brought back. The investigations of the western coast of Greenland are not likely to be continued for the present.

THE Dutch Geographical Society has abandoned its plan of sending a scientific expedition to the Dutch part of New Guinea, but intends sending one to the Key Islands instead. The researches will not only be ethnographical and anthropological, but especially botanical.

AT the recent meeting of the French Association M. Schrader described the results of his ten years' study of the Pyrenees, which has led him seriously to modify previously accepted ideas upon the contour and structure of that range. According to the old descriptions the mass of the Pyrenean Chain was comparable to a fern-leaf with its transverse nerves, or to the backbone of a fish. In reality the Pyrenees consist of a long series of lines of elevations oblique to the imaginary axis of the chain, with which they often form an acute angle. It is impossible to look at the network exhibited in the map by the valleys and the ridges without being struck with the extreme precision of the meshes. These meshes are broken up in all directions, the slopes, however, presenting very different aspects. On the French side the crests are blunted. The incessant humidity of the atmosphere has used them up; mountains, ravines, crests, all are effaced to assume the form of juxtaposed cones or pyramids. On the Spanish side, again, the fractures have remained much fresher, the angles sharper, the forms rougher, due no doubt to the much drier climate of the south side. The slope on the Spanish side is very gradual, while on the French side the mountains rise like a wall.

To the *Zeitschrift* of the Berlin Geographical Society (Nos. 129–30), Herr Erich von Drygalski contributes an elaborate paper of over 100 pages on the deformations of the earth's form during the Glacial epoch. Dr. Oppel brings together much useful information on the religious conditions of Africa, his map showing very strikingly the distribution of the various forms. The whole of North Africa is covered with the Mohammedan tint (with the exception of Abyssinia and part of Algeria), coming down on the east to beyond the equator. Different shades of the tint show the oldest Mohammedan region as a narrow fringe along the Mediterranean. A lighter tint indicates the spread of Islam from the eleventh to the seventeenth centuries, and the lightest the broad belt in the south, which has been included during the present century.

THE HARVEIAN ORATION.

AT the Royal College of Physicians, on Tuesday afternoon, the Harveian Oration was delivered by Dr. William H. Stone. In the course of his remarks Dr. Stone sketched the lineaments of Harvey, self-revealed, as a scholar, a lecturer, a physicist, and as a man of genial, not to say humorous, disposition, and said:—"Perhaps the most important part of my prescribed task is to draw a practical conclusion from the essentially physical and mechanical character of Harvey's great discovery. That Harvey himself fully knew this, has been shown in his own words; it is also by his division of anatomy into three parts—*philosophica*, *medica*, and *mechanica*. Now, at the present time, investigation and research is carried on in the pathological, physiological, and therapeutical aspects of medicine, but the physical or mechanical side is somewhat neglected. For hundreds of ardent questioners of Nature who are labouring with the microscope in the biological and bacteriological laboratories, those who attack medicine from its physical side may be counted on the fingers of one hand. Nor, indeed, are the written treatises on this subject abundant, in this country at least. The 'Animal Mechanics' of the Rev. Dr. Haughton, of Trinity College, Dublin, is an exceptional work of great value, which has hardly received the attention it deserves from the medical profession, but it stands almost alone as the representative of its class. On the Continent, however, and in America the case is very different. The admirable 'Medical Physics' of Prof. Wundt, of Heidelberg, has been translated from the German into French, with valuable additions, by Dr. Ferdinand Monayer, who regularly lectures on medical physics at the Lyons Faculty of Medicine, and affords a storehouse of information of the highest value to the medical practitioner. Dr. John C. Draper, Professor of Chemistry and Physics in the Medical Department of the University of New York, has also made a valuable contribution to the literature of this subject in his text-book of 'Medical Physics,' published the year before last. There is, indeed, a small but scanty manual by Dr. Macgregor Robertson, the Muirhead Demonstrator of Physiology in the University of Glasgow, published in Cassell's Student's Series, but it is entirely unfit to compete with the two exhaustive treatises named before. As with the bibliography, so with the teaching. With the exception of a course of lectures which the present speaker has delivered since 1871 in St. Thomas's Hospital, I am not aware of any systematic attempt in London to teach the medical student the vast mass of physical facts which underlie the daily practice of medicine. This College, however, forms an honourable exception, for it has on two occasions kindly given me the opportunity to bring before my brother physicians some few of what our Harvey terms *nova vel noviter inventa*, respecting the physical basis of auscultation in the Croomian, and the electrical conditions of the human body in the Lumleian, lectures of a few years back. It is true that the University of London in its preliminary scientific examination for the degree of Bachelor of Medicine requires students to satisfy their examiners in physics by means of a written paper. But this paper is the same as that set to Bachelors of Science not medical. It is a terrible stumbling-block to the rising medical generation; it bristles with what the late genial Prof. De Morgan, himself a mathematician of the highest order, delighted to call mathematical conundrums. It is set by pure physicists, who know nothing and probably care little for the problems which interest us as medical men. It contributes a large percentage to the slaughter of innocent aspirants to the higher degrees in medicine, on which one of their most distinguished graduates, now Censor of this College, has feelingly and righteously commented. In the sixteen years during which I have carefully read the papers there set I have never once seen a question directly or indirectly bearing on the physics of medicine. The fact is that the large, difficult, and somewhat heterogeneous branch of knowledge connoted by the word physics is rapidly splitting into several independent portions. There are now distinctly molecular, mathematical, industrial, and physiological physics. It is the last of these with which we are concerned. The third or industrial branch has been enormously developed of late by the technical colleges at Bristol, Manchester, the City Guilds' at Kensington, and elsewhere. The mathematical branch is well cared for by the two old Universities of Oxford and Cambridge, but the physiological section has been hitherto hardly enough recognized by our teaching bodies. Surely an earnest student should be able somewhere to obtain information as to the natural laws on which the stethoscope, the microscope, the ophthalmoscope, and the sphygmograph are founded without having to wade through interminable problems on the C.G.S. system of units, or vortex theories of matter, or, chimera of chimeras, the possibility and advantages of four-dimensional space. It is to the promotion of this particular branch of study by means of experiment that it is this day my duty to exhort the College. An admirable opportunity exists, for in April of the present year the Committee of Delegates appointed by this College and the Royal College of Surgeons of England reported: (1) That it is desirable to utilize tha vacant ground adjoining Examination Hall for scientific purposes, under the control and management of the two Colleges. (2) That the 'scientific purposes' be, in the first place, the investigation and exposition of such branches of science connected with medicine and surgery as the two Colleges may from time to time determine. The College has subsequently adopted the report. Now I submit with the utmost respect, but with the greatest earnestness, to those here assembled, that a course of physiological physics to be delivered in the new College of Science would be a real boon to all students of medicine, whether they had succeeded in obtaining their diploma or not. The human body is a mass or congeries of separate machines, susceptible of mechanical explanation; but, setting aside the heart and lungs, already named, how many students have their attention specially drawn to Donders's and Landolt's optical researches on the eye and eyesight, or to Helmholtz's account of the mechanism of the ear? Such a course, moreover, would in no way clash with other courses given elsewhere on different branches of the same subject, and it would eminently fulfil the exact purpose even to the very words of the great man whom we are to-day met to commemorate."

THE BRITISH ASSOCIATION.

Section C—Geology.

Preliminary Note on Traverses of the Western and of the Eastern Alps made during the summer of 1887, by Prof. T. G. Bonney, F.R.S.—The first traverse was made along the line of the Romanche from near Grenoble to the Col du Lautaret, and thence by Briançon over the Mont Genèvre and the Col de Sestrières to Pinerolo, at the edge of the Italian plain. The second went from Lienz, across the central range of the Tyrol to Kitzbühel, and the rocks of this range were also investigated at other places. During both traverses the author had the advantage of the assistance of the Rev. E. Hill, who had accompanied him on a similar journey in 1885. The results of their examination fully confirm the views already expressed by the author as to the nature and succession of the crystalline rocks of the Alps.

(1) The lowest group consists partly of modified igneous rocks (which indeed occur at all horizons), partly of gneisses of a very ancient (Laurentian) aspect.

(2) The next group, up to which there seems a gradual passage, consists mainly of more friable gneisses and moderately coarse mica-schists (Lepontine type). This group is commonly less fully developed in the above districts than in the Central Alps, having probably been removed by very ancient denudation.

(3) The third group has an enormous development. It forms a large part of the Cottian and Graian Alps, and it flanks the central axis of the Eastern Alps on both sides, often passing beneath the ranges of Secondary strata which here form the northern and southern ranges. It has been traced almost without interruption from east to west for more than 50 miles on the southern and 80 on the northern side of the central range. It has a very close resemblance in all respects to the uppermost group of schists in the Central Alps, found to some extent in the Lepontine and yet more largely in the Pennine Alps, and the author fully agrees with the Swiss and Austrian geologists in regarding it as in the main a prolongation of the same series. It is characterized especially by rather dark-coloured mica-schists, often calcareous, sometimes passing into fine-grained crystalline limestones, with occasional intercalated chloritic schists, especially in the lowest part and with (rarely) quartz schists.

(4) The Carboniferous and Secondary strata infolded or overlying in the Western Alps section, and the Palæozoic (? Silurian) and Secondary strata succeeding the metamorphic rocks in the Eastern Alps, are comparatively little altered and are each readily to be distinguished from the above.

(5) The succession of strata in the third group is inexplicable unless it be due to stratification; in the second this explanation appears highly probable; and in the first not more difficult than any other.

(6) As groups of rocks with marked lithological characters occur in like succession over a mountain chain measuring above 400 miles along the curve, and sometimes at distances of 40 miles across it; and as these groups correspond with rocks recognized as Archæan elsewhere, which exhibit like characters and sometimes a like order of succession, the author thinks a classification of the Archæan rocks by their lithological characters (using the phrase in a wide sense) may ultimately prove to be possible.

(7) The views already expressed by the author as to the distinctness of cleavage-foliation and stratification-foliation have been fully confirmed by the examination of the above districts. He believes that the failure to recognize this distinction is the cause of the contradictory statements with regard to the relation of foliation and bedding which have been made by so many excellent observers, and lies at the root of much of the confusion which exists on the subject of the so-called metamorphic rocks.

The Origin of Banded Gneisses, by J. J. H. Teall.—The author first discussed the meaning of the term gneiss. This term was generally understood to denote a more or less foliated rock of granitic composition. Dr. Lehmann had proposed, however, that it should be used in a structural sense only, as meaning a more or less foliated plutonic rock. He would thus speak of granite-gneiss, diorite-gneiss, and gabbro-gneiss. The author called attention to specimens illustrating gneissic structures in acid and basic plutonic rocks. When various examples of gneissic rocks—that is, rocks of the composition of plutonic igneous rocks but possessing parallel structures—were compared, two types of parallel structure might be recognized; the one characterized by a parallel arrangement of the constituents, the other by an arrangement of the constituents in bands of varying mineralogical composition; thus, bands having the mineralogical composition of a diorite frequently alternated with others having the composition of granite. He proposed to discuss a possible mode of origin for the banded gneisses of the latter type. It was now admitted that those of the former type were largely due to the plastic deformation of masses of plutonic rock either during or subsequent to the final stages of consolidation.

Many observers were, however, still inclined to believe that those of the latter type conld only be accounted for by supposing that the original materials had accumulated by some process akin to sedimentary deposition. Now a possible mode of origin for these could be found if we could show: (1) that plutonic masses are liable to vary in composition, and (2) that such masses are occasionally deformed either during or subsequent to their consolidation. Scrope long ago proved that the laminated structure of certain volcanic rocks (liparites) is due to the plastic deformation of heterogeneous masses of acid lava. Any heterogeneous lump if deformed into a flat sheet will show laminated or banded structures, because each individual portion must of necessity take the form of the entire mass. Scrope not only proved this, but called attention to the similiarity between the structures of acid lavas and those of gneisses and schists. ("Geology of Ponza Isles.")

The author then proceeded to refer to illustrations of the fact that plutonic masses do vary in composition. He referred to the so-called contemporaneous veins, which are often more acid, and to the concretionary (?) patches which are often more basic, in composition than the main mass of the rock with which they are associated. He also referred to cases in which granite and diorite may be seen to vein each other in the most intricate manner, and especially drew attention to the photographs taken at the Lizard last year illustrating this feature. If complex masses of the kind referred to were deformed after the fashion of the acid lavas described by Scrope, then banded and puckered gneissic rocks would necessarily result. He then showed that in the Lizard district the banded rocks of Prof. Bonney's "granulitic series" were continuous with masses in which granitic and dioritic rocks could be seen to vein each other in the most intricate manner, and that the constituent bands of the granulitic series were composed of rocks petrologically identical with those of the igneous complex. He did not mean to imply that the deformation was connected with the intrusion of the plutonic masses. He was rather inclined to regard it as due in the majority of cases to mechanical forces acting posterior to consolidation. The uncertainty which might exist as to the precise conditions under which the deformation was affected did not invalidate the main conclusion, which was that a banded structure in rocks having the composition of plutonic igneous rocks was no proof that the latter were not of igneous origin.

On the Occurrence of Porphyritic Structures in some Rocks of the Lizard District, by Howard Fox and Alex. Somervail.—Prof. Bonney has described a porphyritic diabase which is seen on the shore at Polpeor; it cuts, in an intricate manner, through micaceous and hornblendic schists. The authors have traced this rock further, and have recognized a porphyritic structure not only in many dykes and intrusions along the coast which cut through the serpentine, but also in the darker bands of Prof. Bonney's "granulitic group." Descriptions of these various localities are given, and illustrative specimens are exhibited. The crystals of feldspar are found to be most numerous in those rocks which lie in the closest proximity to the gabbros and serpentine. They have their long axes at various angles, and are mostly small except at Parn Voose, Cavouga, and Green Saddle. The feldspathic and hornblendic lines often circle round the crystals. Without discussing any theory as to the true nature and origin of the whole of the schists, the authors think that the porphyritic structure so prevalent in the dark bands of the "granulitic group," in many of the micaceous and other rocks, as also in the later intrusions cutting the serpentine, indicates an igneous origin for many rocks hitherto regarded as schists.

Preliminary Observations on the Geology of Wicklow and Wexford, by Prof. Sollas.—Of rocks older than the Cambrian, examples probably occur in the Carnsore district, but most of the presumed Archæan rocks are to be explained as crushed igneous dykes and flows. The Cambrian are certainly unconformably succeeded by the Ordovician. The main granite of the district is a truly intrusive rock, but at its junction with the Ordovician, which it penetrates, it possesses the characters of a true gneiss, the schistosity of which corresponds in direction with that of the adjoining schists, having resulted from earth-movements which took place after the Ordovician and before the Lower Carboniferous period.

Some Effects of Pressure on the Sedimentary Rocks of North Devon, by J. E. Marr.—The structures described in this paper are mainly seen in the Ilfracombe division of the Devonian system, as exposed near the bathing place at Ilfracombe. The rocks there consist of argillaceous beds, with thin bands of grit and crinoidal limestone; these harder beds are folded into a series of small sigmoidal folds, which form portions of similar larger folds. When the middle limb is replaced by a fault, the cores of the folds remain as "eyes" of limestone or grit, and these "eyes" have undergone further modification, having been pulled out into thin lenticular masses. In this way we have all the mechanical structures of a true schist produced (including the apparent false-bedding), the rock now consisting of clay-slate with alternating folia of grit or limestone, or both. Quartz veins are folded in a similar way to that described above, and the final result of this folding appears to be the production of a rock consisting of alternating clay-slate, limestone, and quartz-folia. Every stage of the process is seen in the case of the limestone "eyes." The cores of limestone, when not dragged out, have their compound crinoid stems pressed into polygons, which have been formed in the way described elsewhere by Dr. Sorby. When the limestone is pulled out the stems are separated, as in the case of the Belemnites figured by Heim, and the intervening portion is filled with calcite. In this neighbourhood, then, we find sedimentary rocks presenting all the mechanical peculiarities of normal schists, without any great amount of chemical change.

On the Organic Origin of the Chert in the Carboniferous Limestone Series of Ireland, and its Similarity to that in the Corresponding Strata in North Wales and Yorkshire, by Dr. George Jennings Hinde.—The author showed that this rock, which has hitherto been usually regarded as an inorganic deposit of silica direct from the sea-water, is in reality made up of the microscopic detached spicules of siliceous sponges. These sponges lived in successive generations over certain areas, and, after the death of the sponges themselves, the minute spicules forming their skeletons fell apart and were strewn over the bottom of the Carboniferous seas in countless numbers, so that by their accumulation beds of solid rock with a total thickness of from 150 to 350 feet have been formed. Sponges were thus more important as rock-formers in the Carboniferous than at any subsequent geological epoch.

On the Affinities of the so-called Torpedo (Cyclobatis, *Egerton*) *from the Cretaceous of Mount Lebanon*, by A. Smith Woodward.—Following Egerton's original determination, the fish seems to have been universally regarded up to the present time as referable to the Torpedinidæ, partly on account of its rounded shape, and partly on account of the supposed absence of dermal defences. The fine series of specimens now in the British Museum, however, appears to demonstrate conclusively that these generally-accepted views as to the affinities of *Cyclobatis* have no sure foundation in fact, but that the genus is truly referable to the Trygonidæ. There is thus no evidence, as yet, of the existence of "electric rays" of an earlier date than those made known by Volta and Baron de Zigno from the Eocene of Monte Bolca, near Verona, in Northern Italy.

The Pliocene Beds of St. Erth, Cornwall, by Robert George Bell.—The opinion expressed in the earlier reports upon this deposit, as to the southern facies of its fauna, has been amply justified by fresh researches. Had there been any connexion with northern seas or colder waters, it would be difficult to understand the entire absence of those forms of Pleurotoma (Bela) so abundant in the Boreal seas of the Crag period and the present age, as well as the equally characteristic bivalves, Astarte and Cyprina. Some conflict of opinion exists upon the depth of water in which the St. Erth clays were deposited. In a letter to NATURE, vol. xxxiv. p. 341, a very competent authority on Pliocene phenomena, Mr. Clement Reid, gave it as at least 40 or 50 fathoms, founding his view on the evident fact of its deposition in still water, which he maintains could not be found in a district exposed to Atlantic swells at less depth. To this the writer must take serious exception. Undoubtedly the clays exhibit an entire absence of such a disturbing cause as the influence of great wave action, but it remains to be proved that such a great depression as Mr. Reid describes did occur at the western end of Cornwall, and as far as the author's observations go there is little indication of such a fact. Some depression, of course, must have happened, sufficient to submerge the low-lying land near St. Erth, causing a strait or gulf, dividing the Land's End from the main eastern portion of the county. In this shallow strait the clays and sands were deposited, and just such an assemblage of Mollusca is found as will bear out this view. Scarcely any of the shells which are of living species are known to inhabit such deep water as Mr. Reid indicates, while the majority show the presence of a laminarian zone, extending to not more than 15 fathoms.

On a Starfish from the Yorkshire Lias, by Prof. J. F. Blake.—The specimen described was an external cast of the under side of a *Solaster*, which was sufficiently well preserved to afford both generic and specific characters. The only known species with which it is comparable is *Luidia murchisoni*. If this is truly described, and is in fact a *Luidia*, then the present specimen, which is certainly a *Solaster*, must belong to a different species. It was found at the base of the cliff at Huntcliff by the Rev. G. Crewdson, of Kendal.

The Classification of the Dinosauria, by Prof. Seeley, F.R.S.—The author discussed the structure of the animals named Dinosauria, and concluded that the group had no existence, the constituent animals belonging to two orders which have no near affinity; they are named Omosauria and Cetiosauria, the former with a sub-avian pubis and ischium, the latter with those bones sub-lacertilian. The *Omosauria* is defined as having the ventral border of the pubic bone notched out, so that one limb is directed backward parallel to the ischium, while the other is directed forward. The ilium has a slender prolongation in front of the acetabulum. The *Cetiosauria* is defined by having the pubes directed forward with a median symphysis, but with no posterior limb to the bone. The anterior prolongation of the ilium has a vertical expansion.

On the Reputed Clavicles and Interclavicles of Iguanodon, by Prof. H. G. Seeley, F.R.S.—The author showed, by superimposing a figure of the reputed clavicle upon the bone figured by Mr. Hulke as clavicle and interclavicle of Iguanodon (*Quart. Journ. Geol. Soc.*, vol. xli. plate xiv.), that the supposed sutures are fractures, and that the supposed interclavicle has no existence, except as an ossification posterior to the reputed clavicles. Then it was urged that these bones are unparalleled by any vertebrate clavicles, while the reputed pubes of crocodiles and prepubes of other animals offer a more probable analogy. The ossification in front of the pubis in Ornithosaurus is of similar form in several genera. And in crocodiles the ossification of the fibrous extension which connects the reputed pubes with the sternal ribs would produce a bone like the supposed interclavicle of Iguanodon. Hence it was urged that these bones in Iguanodon are pre-pelvic, and the author identified them with the pre pubic bones.

On the Permian Fauna of Bohemia, by Prof. Anton Fritsch (of Prague).—After having mentioned the seventy-three species of Labyrinthodonts of which he has given figures in his work ("Fauna der Gaskohle"), and of which he exhibited the electrotypes and restored models in the galleries of the Owens College, the author mentioned the discovery of a very peculiar genus *Naosaurus* (Cope). Then he explained some unpublished plates of Ctenodus, Orthacanthus, Ctenacanthus, and a new Ganoid Fish (Trissolepis), with three kinds of scales. Then he proved Acanthodes to be very near to the Selachians, and drew attention to the gigantic fish (Amblypterus), 113 cm. long, exhibited in the galleries.

Section D—Biology.

Proposed Contributions to the Theory of Variation, by Patrick Geddes.—The author argued that the variations which furnish the distinguishing characters of orders, genera, or species alike are seen to be not merely "spontaneous" or "indefinite," but parallel, or rather convergent; *i.e.* directed through the checking of vegetation by reproduction along a definite groove of progressive change. Passing from the study of the flower to the larger question of the classification of plants, an antagonism between nutrition and reproduction is seen to be general and constitutional, affording a constant factor in variability. In every natural alliance of flowering-plants, be it species or genus, order or class, we can distinguish the appearance of a predominantly floral, and of a predominantly leafy, or weedy, type; *i.e.* of a reproductive and vegetative one. What we figuratively call higher or lower species are thus essentially the leaders or the laggards along one or other of these two main lines of evolution—the representatives on one side or other of this or that stage in the rhythm between vegetative and reproductive changes which we know as the essential functions of organic life.

On the Structure of Haplodiscus piger, by W. F. R. Weldon.—This remarkable organism consists of an outer cuticle within which is a protoplasmic layer with interspersed nuclei; this protoplasmic layer is continuous with a network which ramifies through the body; in the middle of the body and towards the under surface the network is condensed into a solid mass which sends out pseudopodia-like processes and is evidently an organ of nutrition; the interstices of the network are occupied by the reproductive organs; there is a vesicula seminalis communicating with the exterior by an ejaculatory duct. The author compared this organism to a Foraminifer, such as *Haliphysema*, which had developed sexual organs.

On the Degeneration of the Olfactory Organ of certain Fishes, by Prof. Wiedersheim.—The author described a remarkable series of intermediate forms between a species of *Tetrodon* in which the olfactory organ was represented by a bifid tentacular outgrowth on each side, and another form in which these organs had degenerated into a simple flat plate.

On the Torpid State of Protopterus, by Prof. Wiedersheim.—In this paper the structure of the "cocoon" was described, and the author announced the startling discovery of a peculiar respiratory organ in the tail of the fish; the relations of this organ to the lungs during the torpid condition is not known.

The Larynx and Stomach of Cetacean Embryos, by Prof. D'Arcy Thompson.—The author pointed out that the divisions of the stomach in the Cetacea, do not really correspond to those of the Ruminants, with which they have been erroneously compared.

The Blood Corpuscles of the Cyclostomata, by Prof. D'Arcy Thompson.—The red blood corpuscles of *Myxine* are oval, as are also those of the larval lamprey. The adult lamprey has round corpuscles.

On the Luminous, Larviform Females of the Phengodini, by Prof. C. V. Riley.—Certain interesting phosphorescent Coleopterous larvæ, reaching from 2½ to 3 inches in length, have been well known to occur in America since 1862. Prof. Riley gave a minute description of these larvæ, calling attention to certain structural features of the head, and to other points.

The great interest attaching to these larvæ is not so much in their luminosity as in the fact that a portion of them are now known to be true and perfect females of Phengodini, which have until recently been represented in Coleopterological collections in the male sex only.

Prof. Riley has critically examined in all some thirty different sets of specimens. These all belong to *Phengodes* and *Zarhipis*, with the exception perhaps of one, which may be *Spathizus*. The differences between the larvæ proper and the adult female are so slight that it was difficult to separate them without some absolute index. Prof. Riley had obtained undoubted females, coupled with their males, of *Phengodes laticollis* and *Zarhipis riversii*, and in both genera there were absolutely no other structural differences than the somewhat shorter mandibles and tarsal claws in the adult. In reference to life-history, the food of Zarhipis is known to be Myriapods. The eggs in both genera are spherical, translucent, and laid in masses in the ground, the newly-hatched larvæ in both are structurally identical with the parent; and the female larva goes through a pseudo-pupal state prior to the final moult. Nothing is yet known of the male larva and pupa, and the author only conjectures that certain darker, more slender larvæ, structurally identical, belong to this sex. The author discussed the bearing of the facts on the theory of evolution. In these larviform females we get a glimpse, so to speak, into the remote past, from which has been handed down to us, with but little alteration, an archetypal form which prevailed before complete metamorphosis had originated; while on the other hand her male companion, during the same period, had developed wing-power and the most elaborate and complex sensorial organs, the eyes and antennæ in these beetles being among the most complex of their order. Whatever we believe of the origin of the female Phengodes, one thing is self-evident; viz. that there is direct relation between the phosphorescence and the remarkable differentiation of the sexes, and, further, that such relationship is explicable and full of meaning on evolutionary grounds.

A discussion upon *The Present Aspect of the Cell Question* was opened by Prof. Schäfer. After a brief historical *résumé* of the different conceptions of the cell, the speaker brought forward facts in support of his view that the essential part of the cell is not the reticular substance, but the interstitial substance. It was pointed out in the first place that the various materials produced in the cell by the activity of its protoplasm, *e.g.* fat, appeared in the interstitial substance; and that, in the second place, the *Amœba* presented no reticular substance whatever. The structure of the white blood-corpuscle was also quoted as an additional argument. These corpuscles have a reticulum like that of other cells, but the pseudopodia are prolongations of the interstitial matter; hence the activity of the cell for movement is lodged in this substance, and not in the reticulum.

Prof. Weismann contributed to the discussion an account of his views upon the nature and meaning of polar bodies, announcing at the same time the discovery of a single polar body in the parthenogenetic eggs of certain animals.

Prof. Lankester drew attention to a statement made by the President of the Association in his opening address. Sir H. Roscoe had stated that protoplasm was not a chemical compound, but a structure built up of compounds. This statement was indorsed as in harmony with the views of at any rate many biologists. The term *protoplasm* was originally applied by von Mohl to the whole of the slimy matter within the vegetable cell-wall. But nowadays biologists were more and more limiting the term protoplasm, and applying the term true protoplasm to the chemical substance of highest elaboration, which is the important living part of von Mohl's "protoplasm." Prof. Lankester suggested that the term "plasmogen" should be used for this substance. With regard to the structure of protoplasm, it was considered to be vesicular, the reticulum or walls of the vesicules being that part of the protoplasm in which the plasmogen resides which is not contained in the vesicular spaces. The idioplasm and germ-plasm of Prof. Weismann were probably varieties of plasmogen.

The discussion was continued by Profs. Krause, Carnoy, Marshall Ward, and Hartog, Mr. Gardiner, and Mr. Sedgwick.

Prof. Riley read a paper upon *Icerya purchasi, an Insect injurious to Fruit-Trees.*—It was stated that this species is the most polyphagous of Coccids, living on a great variety of fruit-trees. As it is capable of moving about at all stages of development after leaving the egg, and can survive for a long period without food, it is one of the most injurious of parasites. It is believed to have originated in Australia and to have been introduced into other parts of the world upon living plants. It is very hard to destroy the eggs by any insecticides because of the fluted waxy ovisac. In California these difficulties have, however, been largely overcome by the use of kerosene emulsions and of resin soaps, as well as by inclosing the tree in a portable tent, which is then filled with certain gases.

The Hessian Fly, by Prof. Fream.—The Hessian fly was discovered in Britain in barley-fields near Hertford in July 1886, previous to which date there is no record of its occurrence in this country.

During the present summer it has been traced over the greater part of England and Scotland, and the author found it on July 14 in fields of wheat and barley on the borders of South Wilts and South Hants. The theory that the fly was introduced into the United States by Hessian troops during the War of Independence is regarded as untenable. Packard, discussing Wagner's results, concludes that the Hessian fly had appeared in the Eastern States before the Revolutionary War, that it has never been known to inhabit England or Northern Europe, that it was not known even in Germany before 1857, that it has "from time immemorial" been an inhabitant of wheat-fields on the Mediterranean coasts, that it most likely originated in this region, or farther east (in the probable original habitat of wheat and other cereals), and that it was introduced thence into the United States before the war. How it reached Britain is not known, but it probably came as "flaxseeds" in straw used for packing or for litter. Wheat, barley, and rye are the cereals attacked; oats appear to escape. The "flaxseeds" or puparia have also been found upon timothy grass (*Phleum pratense*, L.), but there is no evidence of any other grass being attacked. American observations indicate that the fly flourishes best in warm, moist seasons, so that the hot, droughty character of the recent summer can hardly have specially favoured it; in fact, it seems to have made headway under rather adverse conditions, and with one of our usual moist summers the attack would probably have been more severe. Many precautions have been suggested for the use of agriculturists with the object of minimizing the attacks in future years. Several species of Hymenoptera are parasitic upon the Hessian fly. Specially useful in this way are *Semiotellus destructor*, Say, one of the Chalcididæ, which deposits its eggs in the pupa of the Hessian fly, and *Platygaster error*, Fitch, which places its eggs within those of the fly. These minute parasites have done splendid service in the American wheat and barley fields, where they are as active friends to the corn grower as are the aphis eating lady-birds in this country to the hop grower. It has been suggested that if the parasites have not accompanied the fly to Britain they should be colonized here. On August 11, however, from a "flaxseed" in the possession of the author there emerged a chalcis fly, and other observers have confirmed the presence in this country of insect parasites of this much-dreaded crop scourge.

Recent Researches on Earthworms.—Mr. W. B. Benham gave a general account of his own researches into the structure of this group, as well as those of Beddard, Horst, Perrier, and others. One object of the paper was to compare the facts already known about this group and to deduce therefrom the mutual affinities of the different genera. *Perichæte* was regarded as being an ancient form, while *Criodrilus* was referred to as a degenerate form.

A Luminous Oligochæte.—Prof. Harker described a species of *Enchytræus* which he had noticed gave off a brilliant phosphorescent light.

Mr. F. E. Beddard communicated a paper *On the Structure of Fratercula arctica.* The point of the paper was to record the fact that the "oblique septa" of this bird, like those of the duck, were covered with a layer of muscular fibres; in this respect these two birds agree more closely than any other birds with the crocodile, in which animal, according to Prof. Huxley, the homologues of the oblique septa are largely muscular.

On Cramer's Gemma borne by Trichomanes alata.—Prof. Bower described peculiar developments on a plant of *Trichomanes alata* from the Royal Botanic Garden in Edinburgh. From the tips of the pinnæ are produced flattened outgrowths of an apparently prothalloid character; these produce spindle-formed gemmæ, which are recognized as corresponding to those previously described by Cramer. These are thus shown to be genetically connected with a plant of Trichomanes, and the opinion of Cramer is thus now confirmed; but, further, if the flattened outgrowth on which they are produced be truly prothalloid in its character, there is here a further example of that direct transition from the fern plant to the prothallus which has been described under the name of apospory.

On Bennettites, the Type of a New Group between Angiosperms and Gymnosperms.—Count Solms-Laubach described a genus of fossil plants, *Bennettites*, the type of a new group between Angiosperms and Gymnosperms. The plants in question accord with the Cycadeæ in their vegetative structure, but possess fruits which exhibit the true structure of the Gymnosperms.

The Secretion of Pure Aqueous Formic Acid by Lepidopterous Larvæ for the Purposes of Defence, by E. B. Poulton.—It has long been known that the larvæ of the genus Cerura (Dicranura) have the power of ejecting a colourless fluid from the mouth of a gland which opens on the prothoracic segment. The latter segment is dilated when the larva is irritated, so that the fluid is thrown in a forward direction, and for a distance of several inches. When the larva is touched, the head and anterior part are immediately turned towards the source of irritation, and the fluid is thrown in this direction. In 1885 I found that the secretion was strongly acid to test-paper, and that it caused very strong effervescence when placed upon sodium bicarbonate; while a little later I showed the fluid to Prof. Wyndham Dunstan, who told me that the characteristic smell of formic acid could be plainly detected. This opinion was further confirmed when it was found that silver nitrate was readily reduced by the secretion (Trans. Ent. Soc. Lond. 1886, part ii., June, pp. 156–57). In 1886 I obtained a larger number of larvæ, and with the kind help of Mr. J. P. Laws, I was enabled to show that the secretion contains about 33 per cent. of anhydrous acid. All the well-known qualitative tests were applied to the secretion and to the alkaline salts obtained by neutralizing with standard alkali. Among other tests, the secretion was found to dissolve the oxide of lead, a white crystalline salt being deposited. Although only a very minute weight of this was obtained, Prof. Meldola kindly offered to estimate the amount of lead present in the salt. The weight was found to correspond to one of the basic formates of this metal formed by the action of the normal formate upon the excess of oxide. During the past summer I have had a very large number of these larvæ, and the investigation has been continued with larger amounts of secretion. The pipette has been applied for the removal of secretion between 500 and 600 times, and between twenty and thirty volumetric determinations have been made. A mature larva which has not been previously irritated will eject ·050 gramme of secretion containing about 40 per cent. of anhydrous acid. Half-grown larvæ eject nearly as much, but the fluid is weaker, containing about 33-35 per cent. of acid. The rate of secretion is comparatively slow—*e.g.* two days and a half after ejection, two large larvæ only yielded together ·025 gramme of secretion. Two captured larvæ, to which the eggs of parasitic Ichneumonidæ had been affixed, only ejected ·035 and ·045 gramme of secretion; having incompletely made up the amount lost during the attack of the Hymenopterous insect. Starvation lessens the amount of secretion, and also decreases the proportion of acid; but probably both these effects are due to general health, and do not imply the direct formation of the acid from the food. The different food-plants—poplar and willow—do not make any difference in the amount or strength of the secretion. About half the total quantity of secretion obtained was made use of in preparing a relatively large amount of the normal formate, which is now in Prof. Meldola's possession. The weights of the constituent elements will be determined by combustion. The rest of the secretion has been used for other exact methods of estimation and analysis under the kind direction of Mr. A. G. Vernon Harcourt, the work having been conducted in his laboratory at Christ Church. Mr. Harcourt suggested that it was most important to prove that the amount of acid shown to be present by volumetric analysis is formic acid, and nothing else. This proof was obtained in two ways: (1) a certain weight of the secretion was divided into two parts; the amount of acid in one of these was determined by the volumetric method, while the other was decomposed by strong sulphuric acid, and the carbon monoxide which was evolved was exactly measured in the apparatus for gas-analysis, and the amount of formic acid present was calculated from the data thus obtained. The two percentages nearly corresponded, and, as the latter was the higher, it was obvious that no other acid could be present. (2) A certain weight (·186 gramme) of secretion was heated in a tube over a water bath, and, after drying at 100° C., only ·0004 gramme of solid residue remained, and this was probably accidental. The rest of the fluid was distilled into a tube containing carbonate of lead, and this was afterwards heated to 100° C., and the water collected in drying-tubes. As a result, the increase in weight of the latter, and the tube containing lead carbonate, the weight of formate of lead obtained from the latter, and of sulphate of lead obtained from the formate, all corresponded almost exactly to the weights which would have been given by pure aqueous formic acid having this composition: water, 62·5 per cent.; formic acid, 37·5 per cent.

Since writing the above I have received the results of Prof. Meldola's analysis, from which he concludes that the secretion consists of aqueous formic acid almost in a state of purity.

Further Experiments upon the Colour-Relation between Phytophagous Larvæ and their surroundings, by E. B. Poulton.—From the instance of the larval *Smerinthus ocellatus*, I have shown that certain Lepidopterous larvæ are susceptible to the influence of surrounding colours, so that the larvæ themselves gain a corresponding appearance.[1] This larva varies from bright yellowish-green to a dull whitish or bluish-green, and either variety can be produced by the use of a food-plant, with the appropriate colour on the under side of the leaves. Although the difference between the two varieties is very great when they are placed together, so great in fact that I can readily distinguish three intermediate stages of variation between the extremes—yet it is not nearly so well marked as in the case of the green and brown varieties of many dimorphic larvæ. I was therefore anxious to test one of these latter, and to ascertain whether either variety can be produced at will by surrounding the larva with the appropriate colour. Lord Walsingham had previously called my attention to the variable larvæ of *Rumia cratægata*, some of which are brown, some green, while many are intermediate. The larva exactly resembles the twigs of its food-plant, and always rests upon the branches in a twig-like attitude, and this habit rendered the species very favourable for the purpose of this inquiry, which was conducted as follows:—A glass cylinder was provided with a black paper roof, and a similar floor, and a small quantity of the food-plant—hawthorn—the rest of the space being entirely filled with dark twigs. Owing to their habit, the larvæ always rested upon these latter, and after reaching maturity in two such cylinders, forty dark varieties were produced. Three other cylinders were roofed and floored with green paper, and green shoots bearing leaves were introduced as food, nothing brown being allowed inside the cylinder. In these cylinders twenty-eight green varieties were produced. The young larvæ were obtained from the eggs of three captured females. After hatching, the larvæ were thoroughly mixed and introduced into the cylinders when quite small. Some of the dark varieties were greenish, and some of the green larvæ brownish, but the greenest in the dark cylinders was browner than the brownest in the green cylinders. The larvæ were compared by placing the sets side by side upon white paper, and the contrast between the larvæ brought up in different surroundings was very marked. In this case the larvæ ate precisely the same kind of leaf, so that it is clear that the effects follow from the surrounding colours, and not from the action of food. The instance recorded above is the best among the many cases of adjustable colour-relation which are now known in Lepidopterous larvæ. It is now extremely probable that all dimorphic species will show more or less of this susceptibility to the colours of their environment.

Further Experiments upon the Protective Value of Colour and Markings in Insects, by E. B. Poulton.—The experiments undertaken in 1886, of which a short account was given in a paper read before Section D, at Birmingham, led to such interesting results that I determined to renew the investigation during the present year.[2] At the same time the range of the inquiry has been widened, and for the first time a mammal has been included in the list of insect-eating vertebrates used in the experiments. For this purpose a marmoset has been employed, and this animal appears to be highly insectivorous. With the kind help of Mr. A. G. Butler I have been able to add largely to the number of experiments made with birds, and these results have been especially needed. In addition to the species of lizards and frogs made use of last year, I have also experimented with a chamæleon and a salamander. The comparison of the results obtained from these different groups of insect-eaters is extremely interesting. In nearly all cases there is complete concurrence in their treatment of highly-coloured nauseous insects. But there are great differences in the relative ease with which the different groups can be induced by hunger to eat distasteful insect food.

The frogs and the birds appear to be the least scrupulous in this respect. It seems probable that the superficial skin of the frog is more delicate than the lining of the oral cavity. Thus the Hymenopterous larvæ, *Crœsus septentrionalis* and *C. varus*, were eaten in considerable numbers, but the face was carefully wiped by the paw after being touched by the everted glands of the larvæ. I am inclined to think that lizards are less unscrupulous in this respect than the most completely insect-eating birds. Mammalia (*i.e.* the marmoset), appear to be far more difficult to please than any of the other groups. The above arrangement accords well with what is known on other grounds of the development of the sense of taste in the vertebrate classes.

I will now bring forward a few of the instances which support the above-mentioned conclusions. The marmoset would never touch a hairy or spinous larva of any kind; this was because of the presence of the structures themselves, for the same species was always eaten in the pupal stage. All the other vertebrates will sometimes eat hairy larvæ. Birds have a special advantage in their power of getting rid of unpleasant appendages—such as hairs or wings. Large lizards will eat unpalatable insects which are often refused by smaller ones, probably because the former can swallow their prey without so much biting, and thus without tasting it so much. Lady-birds were eaten by the nightingale, and by frogs when very hungry; hitherto they have been invariably refused by the other vertebrates. The green larva of *Pieris rapæ* was eaten, but disliked by the marmoset, relished by the lizards. The hairy larva of *Orgyia antiqua* was eaten by birds but refused by lizards, except on one occasion, when two lizards fought over the larva, and in the struggle tore out the hairs incidentally. An experiment with this latter larva gave a very probable interpretation of the meaning of the hairy tufts on many Bombyx larvæ. A lizard seized the larva by one of these tufts, which immediately came out, leaving the lizard with a mouthful of hairs. After this it did not again touch the larva. These tufts are placed on the back, in the part where larvæ are nearly always seized; being formed of very closely approximated fine hairs of the same height, the whole tuft suggests a solid part of the animal rather than a mass of loosely fixed hairs.

The following conspicuous nauseous forms have been eaten, when the vertebrates have been hungry:—

Euchelia jacobææ, larva, by lizard.
Pygæra bucephala, larva, by lizard.
Porthesia auriflua, larva, by lizard.
Zygæna filipendulæ, imago, by lizard.
Zygæna trifolii, imago, by frog.
Diloba cæruleocephala, larva, by lizard.
Liparis salicis, larva, by lizard.
Liparis salicis, imago, by lizard and marmoset.
Abraxas grossulariata, imago, by lizard.

L. salicis (imago) is evidently very distasteful, but the very similar (although smaller) *P. auriflua* (imago) is palatable, and the latter probably benefits by the reputation of the former. Thus the marmoset when very hungry ate the former, although it was much disliked; immediately afterwards the mammal refused the latter, although on other occasions he ate as many as four of these moths with evident relish. Highly-gilded pupæ of *Vanessa urticæ* were eaten with relish by birds and the marmoset, and it is clear that the appearance does not in this case indicate an unpleasant taste, as has been suggested. The spider-like larva of *Stauropus fagi*, in its terrifying attitude, somewhat impressed a lizard and the marmoset, but not to such an extent as to prevent the larva from being eaten. This was to be expected, for both animals will eagerly devour spiders. Such effect as was produced was due to the suggestion of no ordinary English spider, but one of much greater size, and with the terrific aspect highly idealized. The terrifying larva of *Cerura vinula* certainly frightened the marmoset, and either its appearance or the secretion of formic acid greatly affected the lizards. The terrifying but quite harmless larva of *Chærocampa elpenor*, which is known to frighten all but the boldest of birds (as Weismann has shown) was offered to a large lizard. The latter examined the larva most cautiously and many times before touching it; then it bit the larva gently, and retired to watch the effect, repeating this process several times. Finally, finding that nothing happened, it seized the larva, and soon swallowed it. The effect produced by this serpent-like larva was not due to its size, for the equally large larvæ of *Smerinthus ocellatus* were seized at once. The imagines of *Sesia bembeciformis* and *S. apiformis*, resembling hornets, were offered to lizards. On the first occasion the moth was approached with the greatest caution, examined carefully, and seized by the head and thorax,

[1] An account of these experiments will be found in Proc. Roy. Soc., No. 237, 1885, p. 269, and No. 243, 1886, p. 135.

[2] For the complete account of the experiments in 1886 see a paper by the author in Proc. Zool. Soc., Lond., March 1, 1887, pp 191–274.

just as though it possessed a sting. At the same time the lizard evidently doubted whether it was a really dangerous insect at first sight. When, a few days later, a second moth was offered to the same lizard, it was immediately seized without any caution or hesitation. The lizard had learnt its lesson. Instances of this kind support the belief that insect-eating animals have no instinctive knowledge of the palatable, or unpalatable, or dangerous character of their prey, but that they learn by experience. Thus the chamæleon was offered a bee, which was caught at once with the tongue; as the organ was withdrawn, the chamæleon was stung, and shook the bee off; after this it would never touch a bee again. Similarly with many conspicuous nauseous insects, they were generally caught once, but rarely a second time. Now if such instinctive knowledge existed, the chamæleon above all might be expected to possess it, because of the manner in which it catches insects. Shooting its prey from a considerable distance, it can rarely gain any knowledge of a new insect without, so to speak, committing itself, whereas other lizards can make use of the tactile sense in their tongues, while their sense of smell must be more delicate because of their greater proximity before capture. It appears, however, that the chamæleon brought among the insects of a new country relies solely upon a good memory and powerful sight, and these are so efficient that a single instance of each species of insect is sufficient for a thorough education. If the chamæleon possessed an instinctive knowledge of the dangerous or unpalatable insects by which it is normally surrounded, it is most probable that it would also shun the insects of other countries which are protected by similar "warning" colours.

All the species of the genus Zygæna hitherto tested are nauseous, and all are conspicuous and strikingly similar, so that it is probable that we have here an instance of divergence checked by the advantages which follow from simplifying the education of enemies, by setting them one pattern to learn instead of several. Instances of this are well known in other countries, but this is the first example in our own fauna. Among all the experiments previously recorded there occurred no instance of an unpalatable imago which had been palatable in earlier stages. I have paid especial attention to working through many histories in this way, and as a result I have found one such instance. The larva of *Arctia caja* is unpalatable because of the presence of hairs, but apparently not otherwise; the pupa is palatable, while the imago is highly conspicuous and extremely nauseous.

Section E—Geography.

Dr. Ludwig Wolf, who accompanied Wissmann in his exploration of the southern tributaries of the Congo, gave some account of his journeys on the Upper Kasai and the Sankuru, the leading results of which have already appeared in our "Geographical Notes." The point of his discovery was that the Sankuru, which hitherto had been supposed to fall into the Congo, joined first with the Kasai. He described the nature of the country and the habits of the people, giving an account of many personal adventures. The people are superstitious and offer human sacrifices. He did not think Central Africa would ever become a country for European emigration. At the same time Europeans of good constitution could not only live there, but do several hours' manual labour every day. He wished the Congo Free State all success in its efforts to civilize the natives.

A paper by Capt. Coquilhat on the Bangala, a tribe of the Upper Congo, was read. The Bangala are in some cases given to cannibalism, suicide is not unusual, and certain games of chance are popular. European spirits are unknown, and the most popular drink is a kind of beer made from the sugar-cane. The tribe is intelligent and ambitious.

A sociological study on the tribes of the Lower Congo was contributed by Mr. R. C. Phillips, for many years a trader there. His opinion was that these natives had degenerated from a higher standard. They believed in witchcraft, charms, and fetishes. They recognized a something in the sky as a god, but no form of worship followed upon this recognition; it was simply a matter of knowledge. The family relations were fairly developed, and there were fair principles of public justice in operation. The rise of legitimate trade on the Congo had to some extent deprived the chiefs of their wealth, and in this way the lower classes had been benefited.

A discussion followed on the climate of West Africa and its adaptability to European colonization, the general opinion being that European families could not be reared, at least on the coast regions of tropical Africa. Mr. R. C. Phillips, in answer to a question as to the fitness of the Congo districts for emigration, said that during the sixteen years he had known the river (he spoke of it from the mouth up to Vivi) he had seen only three or four white ladies there, and they had either died or been invalided home. At St. Paul de Loanda and other places on the coast the white ladies looked very sallow, and their children did not seem healthy. The population had to be kept up by importation from Europe. His opinion was that the Congo in general was no place for Europeans.

Account of a recent Visit to the ancient Porphyry Quarries of Egypt, by W. Brindley.—Egyptian porphyry has been sought after from the earliest times, as one of the most precious building stones. Ancient writers differed as to the whereabouts of the quarries from which that stone was obtained, and in modern times they were literally rediscovered by Burton and Wilkinson in 1823, and subsequently visited by Lepsius in 1845. The information published by these visitors proving of no immediately practical value, the author determined to follow in the footsteps of Wilkinson, and, accompanied by his wife, he came to Cairo in February last. Having examined the ancient granite quarries at the first cataract, which supplied deep red, rose, and dark grey stone, which was quarried by metal wedges, and not wood (as is generally supposed), the author started from Keneh with a small caravan and supplies calculated to last three weeks. Passing the remains of several Roman stations, the author, on the fifth day, reached an excellent well in the charming Wadi Kitar, hemmed in on three sides by precipitous mountains. Soon after leaving this valley he crossed the watershed (2400 feet above the Nile), and then travelled along the flank of the immense porphyry mountain of Gebel Dukhan as far as the old Roman station with an old fort. The morning after his arrival the author ascended to the top of a pass (3100 feet), without having found even a fragment of porphyry; but espying by the aid of a good field-glass porphyry colouring on the opposite mountain he resolved to go there, and his delight knew no bounds when he found the ground there strewn with pieces of the most sumptuous porphyry, and discovered a pitched way or slide, 16 feet wide, down which the blocks were lowered. Further examination led him to the locality where the Romans had extracted their grandest masses, and he found that these quarries had yielded not only the usual spotted variety but also the brecciated sorts and green-greys The great quarry was at an altitude of 3650 feet above the sea, and a road led down to it to an ancient town with workshops. A path led hence to the old town in the valley, further up which are the ruins of a Roman temple. The blocks were formerly carried to the Nile, a distance of 96 miles, but they will in future be conveyed by a gentle incline to the Red Sea, which is about 25 miles distant. On his return to Cairo the author secured a concession to rework the quarries, the terms of which have since been ratified.

Matabeleland and the Country between the Zambesi and the Limpopo, by Capt. C. E. Haynes, R.E.—The Matabele are the near kinsmen of the Zulus, and have nearly identical customs. They were driven out of Zululand about the beginning of the century, and under their chief Umselikazi, they became a terror to all the Bechuana tribes living north of the Vaal River. Attacked by the Voortrek Boers, and by the Zulus under Panda, they were forced to retire north of the Limpopo, and finally settled down in the midst of the Makalaka and Mashona tribes. About the same period the Gaza kingdom was established by Manikuza, one of Chaka's generals, to the east of the Sabi River. The invasion of the Matabele has caused the annihilation or disruption of the tribes with whom they came into conflict. There are only fragments of the aboriginal people now, who still carry on in a furtive manner some of their old industries, such as gold-digging, iron-working, and weaving. The climate of Matabeleland resembles that of the Transvaal, and the high veldt which ranges from the Nati River to the vicinity of the Zambesi near Tete, is well fitted for European settlers, and promises to become a prosperous agricultural region, with numerous local markets at hand in the mining districts. Care should be taken to protect the forests there. Their wholesale destruction has already begun. The Gaza country and the low veldt is not so salubrious, and, generally speaking, the Zambesi valley is malarious. Agriculture at present is in a depressed state. There is plenty of arable land on the high veldt, and excellent wheat, as all English vegetables alongside the banana and orange can be grown. The high and middle veldts are more suitable for stock-farming. Facilities for irrigation abound. The tzetze does not exist on the high veldt. The mineral wealth of the country still awaits development. The Tati gold-field is now being worked by an English company, but a nod from the Matabele king may at any time put an end to this.

From a scientific point of view Monday was the most interesting day in this Section, the greater part of the time having been devoted to what was practically a discussion of the legitimate field of geography. The first paper, which was more of the nature of a lecture, was by Prof. Boyd Dawkins, on *The Beginning of the Geography of Great Britain*. He said that the surface of the earth was being given up to geographers, and in a few years it would all have been explored. Besides the geography in space, however, there was a geography in time, a field hardly yet touched. It seemed to him that in the field which was open to geographers in recording those ancient changes by which the earth's surface had come to be what it is, in bringing before us boldly and clearly those various geographical outlines at various geological periods presented by the science of geology, they would do as good and as true geographical work as any of those facts which were brought from the interior of Africa or from the inclement regions of the poles. He wished to put before them in outline the method he hoped to follow out in some detail in the course of a few years—a method opened by the results mainly of the various deep-sea exploring parties. The stratified rocks forming the crust of this earth as we know them were all of them deposited in waters which were none of them very deep, and were formed along the margin of a land which was in every degree of the same general sort as the margins of the present ocean. After describing the maps put before the Section, and stating the reasons for supposing Great Britain was originally part of a continent not Europe, which he called Archaia, Prof. Dawkins said that when he looked at the distribution of land and water in the British Isles, from the infinitely remote Upper Silurian period up to the present time, he was bound to believe that some part of the highlands remained dry lands, while the various rocks which occupied the greater part of England, and especially south-eastern England, were accumulated as represented on the map. In conclusion he said he considered that hitherto geologists had devoted themselves so much to the study of fossils and the construction of rocks, and the coining of names which shocked them all, that they had hardly seen that if the knowledge of the ancient life of the earth was to be of any practical use it must be in terms of geographical expression.

Mr. H. J. Mackinder, the recently appointed Reader in Geography at Oxford, opened a discussion on the teaching of geography as applicable to the Universities. To give a practical value to the discussion, he expounded his programme for the coming academical year. There will be two courses of lectures: Course A, on the principles of geography; Course B, on the geography of Central Europe. In these lectures no definition of geography will for the present be attempted. But, to prevent geography becoming a discussion of things in general, a distinct line of argument will be kept steadily in view. This we may indicate thus: the basis—a descriptive analysis of the earth's surface, including in that term the atmosphere, the hydrosphere, the form of the lithosphere, and the material of its surface. From this we shall reason backwards to the causes, and forwards to the effects—the causes largely geological, the effects mainly on man; in other words, in the former stage we answer the question "Why?" for physical, in the latter for political geography. Course A is intended to be annually repeated, subject of course to improvements. It will deal with the methods and principles of geographical observation, reasoning, and exposition, with the great circulations in air and water, with the various types of features, with the broad facts of distribution of animals and plants, and, lastly, with the dependence of man on geographical surroundings and the distribution of his social attributes. The classification will not be topographical, and the examples will be drawn from all parts of the world. Course B will vary in subject from year to year, but will always be an analysis of a particular region. Mr. Mackinder selects Central Europe to begin with, because it best fulfils the necessary conditions: good topographical surveys give us with precision the form of the earth; geological surveys are available for causal reasoning; and a long history gives us abundant scope for the exhibition of effects. It is impossible to foretell the nature of the classes, but he trusts to see at Course B historical students, at Course A those who intend becoming masters in our great public schools, and at both a few who intend being geographical professors, politicians, &c. As regards examinations, Mr. Mackinder is inclined to doubt the ultimate advantage of the too speedy introduction of examinations. We shall lose perhaps in the number of our students at first, but on the other hand we require time to train teachers, time to begin the tradition of a school, and as in this time we are bound to make experiments and mistakes, let us at least make them with our hands untied by a syllabus. One method of stimulating exertion is, however, not open to the same objection. Let us have a prize, but a prize under special conditions. Provisionally, he would suggest the following:—Make a list of, say, twenty small regions, carefully selected, not too distant from England, regions of historical and physical interest. Let the student select one of them at will: let him read up the literature on the subject, and then write an essay. Award the prize by the essays, and then let the winner use the money in visiting the region he has treated theoretically. There let him revise his essay on the spot, or, as he will more probably do, re-write it. Then let it be published. Thus he hopes we might help high training, and at the same time produce a valuable set of monographs. He would also add as a preliminary qualification attendance at the Reader's lectures. As regards diagram-maps, Mr. Mackinder advocates many similar outline-maps, each coloured to represent one set of features, hung side by side. Lastly, as to the relation of physiography to geography. It is impossible to teach rational geography without postulating an elementary but sound knowledge of certain physical and chemical laws and facts, chiefly relating to air and water. This training, it is true, is required for other scientific studies, and even for the intelligent newspaper reader, but it is indispensable to the geographer, and until the schools send us boys so trained, or until the Universities supply such a course for their students generally, the geographical lecturer will have to deal much with physiography. But physiography is not geography; it lacks the topography, which is the essential element in geography.

In the discussion which took place on Prof. Boyd Dawkin's paper it was maintained that he dealt with what was pure geology, or the geography of the past, which was not geography at all in the rational acceptance of the term. Geography should deal strictly with the present, and use only so much of geology as will enable it to understand the conditions of the existing surface. Geography begins where geology ends. In spite of a somewhat humorous diatribe by Canon Tristram on what seemed to him the all-comprehensiveness of the new geography, the meeting was distinctly in favour of Mr. Mackinder's interpretation of the subject.

A paper on *The Ruby Mines of Burmah* was read by Mr. G. Skelton Streeter. The ruby mine district which lies to the north of Mandalay, between the Irrawaddy and the Shan States, bordering on Yunnan. The ruby mine tract, he said, was a large valley some twelve miles long by eight broad, composed of several small valleys, or rather basins. It lay on the slope of the Sibwee Doung, dividing the Irrawaddy and Salween Rivers. The valley bore signs of volcanic origin. The ruby mines were of three distinct kinds. The first was furnished by the metamorphic rock, whose mass was traversed in all directions by huge fissures, caused probably in the past by shrinkage. These fissures were filled with a soft reddish clayey earth, generally containing rubies. At present they were worked in a superficial manner. The second variety of mine was on the sides of these rocky hills where diversified strata of clayey consistency had been upheaved. This earth the natives washed away slowly by hydraulic mining. The last system of mining was by sinking pits in the lower or plain parts of the valley and washing the earth extracted by hand.

The Valley of the Rio Dôce (Brazil), by Wm. J. Steains.—The author in 1881 left England for Brazil for the construction of a railway in the flourishing little province of Alagôs. On the completion of this railway, the author, at his own expense, undertook an exploration of the Rio Dôce and of its northern tributaries. The valley of the Rio Dôce is one of the most fertile regions of the empire. Virgin forests cover nearly the whole of it. Gold is found in Cuithé, a district of Minas Geraes, close to the right bank of the Dôce, as also on the head-waters of the Rio Tambaquary, a tributary of the Sussuhy Grande. Most of the basin of the Rio Dôce is inhabited by wild Botocudo Indians, who possess an inborn hatred of the white man, who, on his side, looks upon these "Bugres" with feelings of intense horror and dread. Until these wild Indians shall at least have been partially civilized, the valley of the Rio Dôce must necessarily remain a sealed paradise. The few attempts made hitherto in this direction have hopelessly failed, perhaps because of the gross mismanagement on the part of those to whom the task was intrusted. The author's arduous explorations have resulted in a carefully plotted map of the Rio Dôce and of its tributaries, based upon over 4000 magnetic bearings and careful dead reckonings.

On some Defects in the Ordnance Survey, by Mr. H. S. Wilkinson.—The author gave a brief sketch of the various methods used by cartographers in the delineation of the irregularities of the ground, illustrating the several styles by specimen sheets of foreign topographical surveys, and he explained that while the English 1-inch hill-shaded Ordnance map is for the mountainous districts unsurpassed by anything published in the world, the same map utterly fails in the representation of the less elevated and therefore less sloping ground. Mr. Wilkinson complained that the contours given on both the 6-inch and the 1-inch map are not sufficiently numerous and strongly-drawn to be of practical value. He suggested that the Ordnance Survey might produce a physical map of Great Britain on some such scale as 1 : 500,000 or 1 : 300,000, so as to relieve English students from the necessity of buying their maps abroad. In conclusion, Mr. Wilkinson urged that the Ordnance Survey should form a high ideal of the scope of its work, and should aim at assisting the eye and imagination of the student to realize "the nature of the earth's surface as the arena of the development of mankind."

Sir Charles Wilson said that the reader of the paper appeared to be under some misconception with regard to the nature and character of the Ordnance Survey. It differed in some respects from those of foreign countries, which were made for purely military purposes. It was true that our Ordnance Survey in its conception was military in character, but its military character was soon lost, and it was now a cadastral survey. The reader of the paper had complained of the crowded detail on the Ordnance maps, but it was to be borne in mind that England was much more crowded than any foreign country. He was acquainted with most of the gentlemen who superintended the foreign surveys, and he knew that our 1-inch map was looked upon as one of the most beautiful pieces of work that had been published. With regard to contours, he said they were tied down by Parliament, but he would like to say that the contours on the Ordnance Survey were instrumental contours, and all strictly accurate. The Ordnance Survey maps indeed were acknowledged to be the most mathematically accurate maps in Europe.

A paper on *The Utilization of the Ordnance Survey* was read by Sir Charles Wilson, who showed a number of Ordnance maps on various scales. He contended that in England as in Ireland the unit of area should be the same for all local purposes. The Ordnance maps had not been much used so far for educational purposes, though they were admirably adapted therefor. He suggested that in the elementary schools a commencement should be made with the immediate neighbourhood around them, which could be done with the 25-inch map. That showed roads, ditches, houses, and isolated trees, and conveyed to the child an idea of how objects were represented in plan. From the place with which they were familiar an advance might be made to the county, then to the whole country, the Continent, and so on. This plan was already in use in France, and he would like to see elementary schools supplied with maps such as he had suggested.

Dr. H. R. Mill gave some account of a new bathy-orographical map of the Clyde basin, embodying the results of the researches which he and others have been carrying on for some time. He described the peculiarities of some of the lochs on the west coast of Scotland, and pointing out the Wyville Thomson ridge, off the north-west coast of Scotland, stated that it was owing to this ridge that the Arctic waters did not descend to our shores, and give us a semi-Arctic climate.

A Plea for the Metre, by E. G. Ravenstein.—The author pointed out the great advantages of the metre as a universal international standard of length. There were at present in use three international measures of length, viz. the English foot, in countries covering 18,188,112 square miles, with 471,000,000 inhabitants, [the metre (12,671,200 square miles, 347,091,000 inhabita ts), and the Castilian foot (752,901 square miles, 5,905,000 inhabitants). The English foot, at present in use throughout the British and Russian Empires, in the United States, and in some other countries, appeared to gain no new adherents, while the metre was still engaged upon a career of conquest. Denmark and Russia were the only countries in Europe which had not as yet adopted it. The metrical system appeared to him to present great advantages to business men, and in 1885 nearly one-half the commercial transactions of the country were carried on with countries using the metre. The time at present expended in our schools upon acquiring a knowledge of an absurdly complicated system of weights and measures might be devoted to more useful objects. To geographers and statisticians the universal acceptance of the metre would prove an immense boon. Scientific men in other departments had freely adopted the metre, and geographers should follow this laudable example. Owing, however, to the intimate connexion of geography with the common affairs of life, he despaired of the general acceptance of the metre until it should have become the legal standard of length.

SECTION H—ANTHROPOLOGY.

The Primitive Seat of the Aryans, by Canon Isaac Taylor.—The author discussed recent theories as to the region in which the Aryan race originated. The pre-scientific Japhetian theory and the Caucasian theory of Blumenbach have long been abandoned. A few years ago the theory advocated by Pott, Lassen, and Max Müller, which made the highlands of Central Asia the cradle of the Aryans, was received with general acquiesence, the only protest of note coming from Dr. Latham, who urged that the Asiatic hypothesis was mere assumption based on no shadow of proof. The recent investigations of Geiger, Cuno, Penka, and Schrader have brought about an increasing conviction that the origin of the Aryan race must be sought not in Central Asia, but in Northern Europe. These writers have urged that the evidence of language shows that the primitive Aryans must have inhabited a forest-clad country in the neighbourhood of the sea, covered during a prolonged winter with snow, the vegetation consisting largely of the fir, the birch, the beech, the oak, the elm, the willow, and the hazel; while the fauna comprised the beaver, the wolf, the fox, the hare, the deer, the eel, and the salmon—conditions which restrict us to a region north of the Alps and west of a line drawn from Dantzic to the Black Sea.

It has also been urged that the primitive Aryan type was that of the Scandinavian and North German peoples—dolichocephalic, tall, with white skin, fair hair, and blue eyes—and that those darker and shorter races of Eastern and Southern Europe who speak Aryan languages are mainly of Iberian or Turanian blood, having acquired their Aryan speech from Aryan conquerors. It has been urged that the tendency in historic times has been to migration from north to south, and that in Central Asia we find no vestiges of any people of the pure Aryan type, while the primitive Aryan vocabulary points to the fauna and flora of Northern Europe rather than to that of Central Asia.

But the Aryans must have had forefathers from whom they were developed; and the inquiry suggests itself, What could have been the race from which the Aryans might have been evolved? A Semitic, an Iberian, an Egyptian, a Chinese, a Turkic, or a Mongolian parentage is out of the question; and the author proposed to show that both from the anthropological and the linguistic point of view the Finnic people come closest to the Aryans, and are the only existing family of mankind from which the Aryans could have been evolved. The Tchudic branch of the Finnic family approaches very nearly to what we must assume to have been the primitive Aryan type.

The only argument for deriving the proto-Aryan from Central Asia was the belief that Sanskrit comes the nearest to the primitive Aryan speech. It is now believed that the Lithuanian, a Baltic language, represents a more primitive form of Aryan speech than Sanskrit, and hence the argument formerly adduced in support of the hypothesis that the Aryans originated in Central Asia becomes an argument in favour of Northern Europe.

If this hypothesis as to the primitive identity of the Aryan and Finnic races be established, a world of light is thrown upon many difficulties as to the primitive significances of many Aryan roots, and the nature of the primitive Aryan grammar. We are furnished, in fact, with a new and powerful instrument of philological investigation, which can hardly fail to yield important results. Comparative philology must henceforward take account of the Finnic languages as affording the oldest materials which are available for comparison.

The Non-Aryan and Non-Semitic White Races, and their place in the History of Civilization, by J. S. Stuart Glennie.—The general thesis of this paper may be thus stated. The first civilizations of Chaldea and of Egypt appear to have been founded by the action on dark races of white races, neither Aryan nor Semitic. The combined results of a great variety of recent researches show that such white races are an important, and hitherto quite inadequately recognized, element in the ethnology of Asia, and of Oceania, of Africa, of Europe, and of America; and not only in Chaldea and in Egypt, but throughout the world, the civilizations of Semites and of Aryans have been founded on civilizations initiated by some one of these non-Aryan and non-Semitic, or, as in one word they may, perhaps, fitly be called, Archaian white races.

The three great divisions of this paper are indicated by this statement of its thesis:—

First, classification and summary of the facts which seem to lead to the conclusion that the imitators of the Chaldean and Egyptian civilizations belonged to a white stock different from both the Aryan and the Semitic white stock.

Secondly, an endeavour to give an approximately complete indication at least, if not statement, of the facts only partially stated by Quatrefages ("Hommes fossils et hommes sauvages") with respect to the white races which he names *Allophyllian*, and for which the term *Archaian* is proposed.

Thirdly, classification and summary of the facts which—the wide dispersion of an Archaian stock of white races being established—seem to indicate that the vexed questions with respect to the Hittites, the Pelasgians, the Tyrrhenians, the Iberians, the Picts, &c., and with respect also, in part, to the origin of the Chinese, the Mexican, and the Peruvian civilizations; the facts which indicate that these questions may be solved by reference to the general facts with regard to the migrations and characteristics of the Archaian white races.

The bearing of these results on the questions raised by the essential identity of the varying forms of folk-lore tales all over the world were also pointed out.

On the Picture Origin of the Characters of the Assyrian Syllabary, by the Rev. W. Houghton. — All written language probably originated in pictures representing objects or ideas, as in Chinese and Egyptian. At first the characters were rude figures of animal or other objects. In time the resemblance would be fainter, till at length all similarity between the character and the object represented would disappear. The process may be expressed by the term "pictorial evanescence." Of the 522 characters of the Assyrian syllabary, as given in Prof. Sayce's "Grammar," very few of the simple characters exhibit their primitive form, but the composite characters often clearly reveal themselves. We must look to the older forms of the characters for evidences of their pictorial origin. Thus, the character for a "fish" in the modern Assyrian may be traced back through the hieratic Assyrian, the hieratic Babylonian, and the linear Babylonian, to a figure of a fish, with head, body, fins, and tail. The ideograph for a "month" is, in its ancient form, a figure of a square with 3×10 inside it—*i.e.* thirty days within the sun's circle. The ancient forms of the character denoting a "man" are rude figures of a man with head, neck, shoulders, body, and legs—such a picture as a schoolboy would draw on his slate, or the North American Indians depict.

Mr. George St. Clair contributed a paper on *Boat-shaped Graves in Syria.*—In passing through the Anti-Lebanon lately, from Damascus to Baalbec, the writer noticed that the graves at the hamlet of El Fijeh have the form of a flat-bottomed boat; those at Ain Hawar are formed like long narrow boats, with an ark or house occupying the middle part; and the graves at the village of Yafufeh are built in three tiers, of which the upper one may be representative of the ark, while the head- and foot-stones are almost certainly the conventional reproduction of the head and stern of the boat. The author asks the question, What led these people in the mountains to build their graves in the model of a boat? Authors are quoted to show that arks or ships were carried in procession by the Phœnicians, as also were sacred boats in the funeral processions of the ancient Egyptians. The Egyptians conveyed the body across a lake, and both the lake and the boat were symbolical, typifying the voyage of the soul in the under-world. The system passed into Greece, where we have Charon and his boat. Charon's boat is sculptured on a funeral monument in the Ceramicon at Athens—a recently uncovered cemetery; and the bas-relief of a ship appears on a tomb at Pompeii. From these facts and others the writer of the paper would infer that the boat-shaped graves of Syria are fashioned by traditional custom in perpetuation of a practice which appears to have originated with the ancient Egyptians. As a supplementary conclusion, it is suggested that the head stones and foot-stones of modern graves may be the surviving representatives of the prow and poop of the sacred boat of the dead.

The Effect of Town Life upon the Human Body, by J. Milner Fothergill, M.D.—It is generally recognized that the effect of town life upon the physique is not beneficial, and as the population of boroughs has now exceeded that of the country, the fact becomes one worthy of our attention. The great and rapid increase of large towns at the present time adds to the importance of the subject and deepens its gravity. Of old there were but few large towns, in our modern sense of a "large" town; but Lugol, the great French authority on "scrofula," noted how the population of Paris deteriorated, and how scrofulous were the third generations of persons who came in from the country perfectly healthy. Other observers have noticed the bad effect of town life elsewhere. And the recent researches of Mr. James Cantlie have demonstrated the rarity of a pure-bred Cockney of the fourth generation. Of old the baron lived in his castle, while the populace lived around in villages of limited size. For men of all conditions of life the one thing to be coveted above all others was physical prowess. For work, for war, for games which were largely mimic war, bodily strength was essential. No courage, no skill, could effectually compensate for the want of thews and sinews. Work, war, sports, revels, all too were conducted in the open air. But civilization brought about changes profoundly influencing the life of the individual. The development of commerce entailed the growth of towns; and then it was found that in the new struggle for existence the battle went rather to the man with the active brain than to the man with a massive framework. The active brain became now the one great thing to be coveted rather than physical prowess. The tendency of town populations is to dwindle, and this dwindling is seen markedly in the feeble digestive capacity of town dwellers. They cannot eat the pastry, the pie-crust, the cakes, which form so large a portion of the dietary of their country cousins. If they attempt these articles of food they give themselves the stomach-ache. Consequently they live on such food as they can digest without suffering—bread, and fish, and meat; above all the last—the sapid, tasty flesh of animals, which sits lightly on the stomach, and gives an acceptable feeling of satiety, so pleasant to experience. The town dweller, in his selection of food, is guided by his feelings; he avoids what is repugnant to him. Such selection is natural and intelligible, but it is fraught with danger all the same. Pulmonary phthisis and Bright's disease seem Dame Nature's means of weeding out degenerating town dwellers. The offspring of urban residents are another race from their cousins who remain in the country. The latter are large-limbed, stalwart, fair-haired Anglo-Danes, while their urban cousins are smaller, slighter, darker beings, of an earlier and lowlier ethnic form, and resembling the Celto-Iberian race. And amidst this general reversion we can recognize a distinct liver-reversion to the early primitive uric acid formation of the bird and reptile. A recognition of these facts must lead to such modifications of the food customs of town dwellers as are indicated. The spread of teetotalism and vegetarianism tells of a dark groping in the right direction in blind obedience to the law of self-preservation. There must also follow some modification of the existing system of education, for it is by the imperfectly nourished town child that the weight of the burden of education is most acutely felt.

On the Bosjes Pelvis, by Prof. Cleland, F.R.S.—The embroadened brim found in certain savage tribes is a retention of a feature of adolescence. This is seen well in the Bosjes, and the peculiarity may be correlated with others which have escaped attention. There is feeble development of the iliac blades, especially at the back part, probably owing to early anchylosis of the epiphysis of the crest. Connected with this the post-auricular levers of the ilia are very feeble, as they also are in early life in Europeans, causing shallowness of the post-sacral fossa occupied by the strongest part of the multifidus spinæ muscle, a most important muscle for erecting the lumbar part of the column on the pelvis. The action of the iliac levers in broadening the brim in the European is recognized. Their shortness, and the lightness of the superincumbent weight of the body, are circumstances which account for the brim failing to broaden out in the Bosjes.

The Experimental Production of Chest types in Man, by G. W. Hambleton.—The author contended that the type of man after birth was produced solely by the conditions to which he is subjected, and that hence a wide and most important field is open for our investigation, for if we can ascertain what the conditions are that produce those changes in each part of man that together form a class or type, we may produce the type that is most suitable for different places and occupations, and then we shall have a true Science of Man.

The Scientific Treatment of Consumption, by G. W. Hambleton.—At the last meeting of the Association the author read a paper on that part of his research that referred to the prevention of consumption, and he now completed the subject by giving an explanation of the mode in which the disease is produced, and by laying down the principles that must guide us in its successful treatment. These principles are four in number, and may be stated as follows: to establish an equilibrium between the amount of interchange required to be effected and that effected; to enable the other organs of the body to perform their ordinary functions; to restore to the lungs the power of adjustment to their external conditions; and to effect the above without producing indications of friction. The effect of this method of treatment is to arrest the process of irritation, to gradually restore the general health, and to develop the lungs. This is shown by a gradual cessation of chest symptoms, a healthy appearance, and a greatly increased vital capacity, range of expansion and size of chest-girth. The author has invariably obtained these results in his experiments, and also in the few cases he has had an opportunity of treating. Sydenham undoubtedly cured consumption by ordering continuous horse exercise in the country till the patient recovered. And the author is satisfied that if we carefully treat consumption—before the disease has been permitted to become too extensive—on the principles advocated in this paper we shall be able to secure complete recovery.

Tattooing, by Miss A. W. Buckland.—The object of this paper was to show that although tattooing seems to have been almost universal among savages, yet the mode of performing the operation varies so much, and the various methods in use seem to have such definite limits, as to make them anthropologically valuable, as showing either racial connexion or some intercourse formerly subsisting between races long isolated.

The Early Ages of Metal in South-East Spain, by Henri and Louis Siret.—The authors explored a coast region, about 75 kilometres in length, between Cartagena and Almeria. They investigated some forty stations belonging to three prehistoric epochs: (1) the Neolithic, (2) Transition between Stone and Metal, (3) the Metal Age. In the Neolithic period man employed instruments of bone, stone, and flint; and ornaments made of bone, stone, and shells were used. The dead were buried in polygonal spaces, surrounded by stones. In the Transition period bronze bracelets and beads were used; and cremation of the dead was practised. These new customs were probably introduced by some foreign people; but at the same time there is evidence of the first attempts at a native metallurgy, utilizing the ores of the country; arms and utensils are found cast in metal, and imitating the form of those in bone and stone. During the Metal epoch, copper and bronze were employed simultaneously, as in the preceding age; but copper predominates, and stone implements are still common. MM. Siret also found several silver ornaments, and this is a new fact in the early Bronze Age; in this region prehistoric man found and utilized the native silver gathered on the surface of the soil. No less than 1300 sepultures were explored by the authors; all the bodies were interred, and not cremated, the bodies being usually placed, doubled up, in large terra-cotta vases. An enormous number of copper and bronze arms and utensils were found, together with vases in pottery; also bracelets, rings, and ear-rings in copper, bronze, gold, and silver; and necklace beads in bone, ivory, serpentine, bronze, copper, silver, and gold.

The Origin of Totemism, by C. Staniland Wake.—The fundamental basis of totemism is to be found in the phase of human thought, which supposes spirits "to inhabit trees and groves, and to move in the winds and stars," and which personifies almost every phase of Nature. The problem of totemism receives its solution in the fact that the totem is the *re-incarnated form of the legendary ancestor of the gens or family group allied to the totem.* The totem is thus something more than a "badge of fraternity" or "device of a gens." It is regarded as having actual vitality, as the embodiment of an ancestral spirit. Any object is fitted for this spirit re-incarnation, and therefore totemism may be looked upon as the expression of Nature-worship and ancestor-worship in combination.

Certain Degenerations of Design in Papuan Art, by S. J. Hickson.—(1) On a prau figure-head is a design which, although considerably modified, can readily be recognized as a design of the human figure. The long crimpled hair of the Papuan, two tufts of which are coloured red, in imitation of the red mud with which the Papuans complete their *coiffure*, the eyes, nose, and mouth of the face are clearly indicated, but the rest of the body is degenerated into a mere conventional sign. (2) Upon the same prau figure-head, as in (1), there is a figure of an animal (probably a gecko), fairly good and complete as a work of art, but upon the same is a design, evidently degenerated, of this, in which all that remains of this unconventionalized is the anterior pair of legs. The designs are wrought by the old men or priests of the villages, and are made for the purpose of keeping off spirits of storm, sickness, &c. Modifications are produced by the artist by want of time, ability, or inclination, and these modifications become permanent by being copied by subsequent artists, and thus in some cases mere conventional signs take the place of figures of men, birds, and other animals.

Gypsies, and an Ancient Hebrew Race in Sus and the Sahara, by R. G. Haliburton.—The province of Sus, as respects the customs of its people, is, and always has been, a *terra incognita.* Excepting a few lines by Herodotus on the subject of these people, nothing has yet been written, and this paper is the first attempt to describe them. The people of Morocco are divided into the Riffs and the Susis: the first light-haired, and large men, living in the mountains; the latter smaller, darker, and generally nomadic. The Susis speak a dialect of the Berber, and are most of them Gypsies of different descriptions. They are famous for their skill as artificers. Most of them tell fortunes—some by sand, others by beads; others, again, by a flower, and some by watching a fowl after its head has been struck off. The women, in some tribes, tell fortunes by the hand, but the men never do so. These people have been for many centuries connected with the Timbuctoo gold trade; and have secret signs and passes, called the words of the Kafila (tent or lodge), which is probably the same word as the well-known "Cabala" of the Jews. The author showed that there are vestiges of the Osirian cult lingering among these people. The author described an ancient Hebrew race inhabiting the Sahara, and pointed out that the Jews and the Gypsies must have been cast in the same mould, but must have been made of very different material. That mould, he believed, was the life in common in North Africa for thousands of years, in connexion with the gold trade and the caravans of that country.

Colour-Names amongst the English Gypsies, by W. E. A. Axon.—Considerable discussion has taken place as to the development of the colour-sense within the historic period. The colour-vocabulary of the English gypsies is limited to "green," "black," "red," and "white," so that we have the notable fact that "blue," on which so much stress has been laid in the discussion of the colour-sense, is entirely absent from the English gypsy vocabulary. This is emphasized by the fact that the gypsies sometimes use the word *blue-asar*, the suffix being that which is generally added in Romany to disguise a borrowed word. So their word for "toadstools" is *blue-leggi*, because the *Agaricus personata*, which they regard as a delicacy, has blue stalks. Clearly, if they had now in Romany a word for "blue," they would not appropriate that of *Gaujo.* And if any evidence were needed that the Romanies are not colour-blind, it is afforded by their appropriation of the English word for "blue." It only remains to add that *Yack* and *Erescare* are both given by Pott as gypsy equivalents for "blue." If these words are genuine—which may be open to doubt—it is apparently possible for a race to possess and to lose a colour-name. This brief investigation of the English gypsy colour-vocabulary will show the danger of accepting the negative testimony of philology as conclusive. The positive evidence of linguistics no one need doubt. It is clear that there is no relation between the colour-perception and the colour-nomenclature of the English gypsies.

On the Migrations of Pre-glacial Man, by Henry Hicks, F.R.S.—Referring to the further researches carried on this summer at Cae-Gwyn Cave, North Wales, the author stated that the additional evidence obtained proved most conclusively that the flint implement found there last year in association with the remains of Pleistocene animals was under entirely undisturbed glacial deposits. He maintained also that the evidence is equally clear in regard to the implements found within the caverns, which he said must have been introduced before the glacial deposits blocked up and covered over the caverns. The question as to the direction from which pre-glacial man reached this country is an exceedingly interesting one, and seems now to be fairly open to discussion. It is admittedly fraught with difficulties, but the facts recently obtained seem to require that an attempt should be made. The evidence, so far as it goes, points to a migration to this country from some northern source, as the human relics found in the caverns, and also in the older river gravels (which Prof. Prestwich is now disposed to assign also to the early part of the Glacial epoch, when the ice-sheet was advancing), occur in association with the remains of animals of northern origin, such as the mammoth, rhinoceros, and reindeer. Up to the present no human relics have been found in this country (and it is very doubtful whether they have been found in any other part of Europe) in deposits older than those containing the remains of these northern animals. If man arrived in this country from some eastern area, it is but natural to think that he would have arrived when the genial Pliocene climate tempted numerous species of deer of southern origin, and other animals suitable as food for man, to roam about in the south-east of England. Hitherto, however, not a relic has been found to show that man had arrived in this country at that time. But in the immediately succeeding period, with the advent of cold conditions and of the northern animals, evidences of the presence of man become abundant. Whether man at an earlier period migrated northward from some tropical or sub-tropical area, and that he then lived on fruit and such-like food, there is no evidence at present to show; but it seems certain that the man of the Glacial period in this country had to live mainly on animal food, and that he found the reindeer to be the most suitable to supply his wants. He followed the reindeer in their compulsory migrations during the gradually increasing glacial conditions, and kept mainly with them near the edge of the advancing ice.

Observations on Recent Explorations made by General Pitt-Rivers at Rushmore, by J. G. Garson.—Dr. Garson began his paper by defining the early British races; he then proceeded to describe the discoveries of General Pitt-Rivers at Rushmore, near Salisbury, where he has found the remains of no less than four British villages of the Roman period, besides many tumuli and cists. The human remains are extremely interesting, and throw much light on the characters of the people to whom they belonged. The chief point of interest which they show is the small stature of the people, the average of the males being 5 feet 4 inches, and of the females 4 feet 11·8 inches, in the village of Woodcuts; while in that of Rotherly, the other village excavated this year, the heights are 5 feet 1 inch and 4 feet 10 inches respectively. The skulls are of a long, narrow, oval form, with one or two exceptions, when they are of rounder form; these were found associated with longer limb bones, showing them to be of different race from the majority of the inhabitants. Two forms of skull are frequently met with in long barrows, both of a long narrow shape, but differing from each other in one having a regular oval outline, while the other broadens out from a narrow forehead, and, having attained its greatest width, terminates rapidly behind. The skulls found in these villages correspond exactly to the first type. It is therefore probable that there were two distinct races of the long-headed people which will have to be distinguished in future.

SOCIETIES AND ACADEMIES.

Paris.

Academy of Sciences, October 10.—M. Hervé Mangon in the chair.—On the theory of outflow between narrow walls at a low or a high level, by M. J. Boussinesq. The problem here discussed deals with the discharge of water into a basin, which is subjected from below to a constant pressure either less or greater than that exercised by the atmosphere from above.—On the grading of tubes intended for gasometric measurements, by M. Berthelot. The author here studies some of the difficult problems presented by the different forms of graduation in tubes employed for the measurement of gaseous volumes.—On the mechanical labour expended by the gull in its horizontal flight, by M. Marey.—On M. G. A. Zanon's memoir entitled "La Cinetica combattuta e vinta da G. A. Hirn," by M. H. Faye. M. Zanon, Professor of Naval Construction at Venice, here intervenes on the side of M. Hirn in the controversy between that physicist and M. Clausius on the subject of the modern theory of kinetics.—Remarks accompanying the presentation of M. Rouvier's seventeen charts of the Congo region, by M. Bouquet de la Grye. These charts, prepared with the co operation of Capt. Pleigneur, of the French Marines, comprise a general map of the French possessions in the Congo basin, and special maps of the lower course of the main stream and of its affluents on the right bank. They embody the results of the first exact surveys made in this extensive region over which the French protectorate has recently been extended.—Observations of Palisa's new planet, No. 269, made at the Observatory of Algiers with the 0·50 m. telescope, by MM. Rambaud and Sy. The observations include the positions of the comparison-stars and the apparent positions of the planet on September 23 and 24.—Apparent positions of Olbers's comet (Brooks's, August 24, 1887), measured with the 8-inch equatorial at the Observatory of Besançon, by M. Gruey. The observations cover the period from September 14 to October 1.—A new solar eruption, by M. E. L. Trouvelot. A description is given of a protuberance of unusual size and brilliancy observed by the author on June 24, 1887, at 267° on the western edge of the solar disk.—Action of carbonic acid on some alkalies, by M. A. Ditte. It is shown that under pressure carbonic acid and aniline unite at equal equivalents, yielding a crystallized carbonate below + 8° C., liquid, or at least in permanent superfusion, at 10° C. This carbonate, soluble in the aniline, does not dissolve the carbonic acid, but dissociates when the pressure is lowered.—On a new source of capric acid, by MM. A. and P. Buisine.—The tactile rays of *Bathypteroïs*, Günther, by M. Léon Vaillant. The specialized organs of touch resulting in certain fishes from a modification of the pectoral and ventral fins, are shown to acquire quite an unusual degree of perfection in the *Bathypteroïs* captured during the *Talisman* Expedition, from depths of 400 to 1000 fathoms.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Sailor's Sky Interpreter: S. R. Elson (Thacker).—Class-Book of Algebra Examples, part 2: John Cook (Madras).—Dix Années dans L'Histoire d'une Théorie: J. H. Van 't Hoff (Bazendijk, Rotterdam).—Report of the Voyage of H.M.S. *Challenger*: Zoology, vol. xxii. (Eyre and Spottiswoode).—Other Suns than Ours: R. A. Proctor (Allen).—Madras Journal of Literature and Science.—Journal and Proceedings of the Royal Society of New South Wales, vol. xxi. (Trübner).—Transactions of the Edinburgh Geological Society, vol. v. part 3 (Maclachlan and Stewart).—Boletin de la Academia Nacional de Ciencias en Cordoba (Buenos Aires).

CONTENTS.

THURSDAY, OCTOBER 27, 1887.

THE STUDY OF EMBRYOLOGY.

An Introduction to the Study of Embryology. By Prof. A. C. Haddon, M.A. (London: C. Griffin and Co., 1887.)

THE publication of this volume supplies a long-felt want. Many will remember the pleasure with which the first appearance of an English volume solely devoted to embryology was hailed. But there was a good deal of the "lively anticipation of future favours" in the gratitude with which the public received Part I. of the "Embryology" by Foster and Balfour. The first edition of this text-book contained, it will be remembered, a clearly written, well illustrated account of what was then known of the embryology of the chick. Although the details were far in advance of anything previously published on the subject in our language, yet the explanations were so clear and the style so lucid that the book was in every way suitable for the beginner. The public was informed, in the introduction, that the work was to be extended so that it would become a text-book of general embryology. Although this promise was more than fulfilled on the appearance of Balfour's classical work, the young morphologist was to a certain extent the loser by the immense benefit thus conferred upon the more advanced student. The second edition of the former book brought the account of the development of the chick up to date, and a comparatively short description of the essential features of embryological development in Mammalia was added, but the work still remains an enlarged Part I. But in the meantime the immense and ever-increasing development of invertebrate embryology, and the magnitude of the theoretical considerations raised by its advance, rendered it more necessary than ever that the beginner should be in possession of some introduction to this part of the subject; and that the student whom time and opportunity prevented from mastering Balfour's great work should, nevertheless, be permitted to gain some insight into the subject as a whole.

Both these conditions are fulfilled, and corresponding benefits will be conferred upon morphological teaching, by the publication of Prof. Haddon's text-book. In one respect, indeed, it appears to be probable that the "Embryology" of Foster and Balfour will always remain pre-eminent as a preliminary text-book for the young embryologist. The limited scope of that work renders possible a comparatively detailed description of the growth and arrangement of the embryonic membranes and appendages, so that the relative positions of these to each other, to the embryo, and to the yolk-sac, are brought before the student with a clearness and force which could not be attained in a more condensed statement. And the great difficulty with which this part of the subject is grasped by the beginner is apt to be undervalued by the advanced student and by the teacher. Again, the selection of a single type for detailed description renders possible and indeed suggests that the succeeding stages of development should be described after the manner of a diary; and while this method strongly impresses the imagination of the reader, it gives continuity as well as reality to the shifting scenes of embryonic development. It is also probable that such a method, with its constantly repeated recapitulations, and references to events which have been previously described, must afford to the memory an assistance which cannot be given by any other system. Of course the full development of this method is totally impracticable in a volume which deals with general embryology, and the subject is not at present in a condition such that it could be adequately represented by the selection of a few types for detailed description. A book which treats of the development of those mature organisms which are usually selected as types of the morphological series is much to be desired and would be extremely useful, but in the present state of embryological knowledge it is doubtful whether such a volume would represent general embryology as completely as general morphology is represented by the types themselves.

For the reasons given above, an ideal course of embryology will begin wtth Foster and Balfour, continue with Haddon, and end, as far as text-books are concerned, with Balfour. The complete mastery in the first-named work of a single easily accessible and readily investigated type, of considerable complexity and difficulty, will enable the student to grasp the shorter descriptions in Prof. Haddon's volume of all important embryological discoveries hitherto made. Finally, a rapid survey of general embryology being thus gained, the student will pass to the longer descriptions and further details of Balfour's classical work. A course of embryological teaching so complete as this, or so well suited to the needs of the student, does not exist in any other language.

It may be objected that in any such succession of text-books there must be a great deal of waste, from the description of the same developmental histories in rather different language. For the advanced student or the investigator the objection is valid, but I believe that every experienced teacher will agree in the opinion that the young student—for whom alone such a course is necessary—gains immensely by this very fact, and learns far more by reading a second text-book which puts the same facts in a slightly different way, than by reading the first text-book a second time.

It is very high praise of Prof. Haddon's volume to place it in the middle position of such a series. It is well and clearly written, while the adoption of smaller type for theoretical questions or less important details, is a great benefit to the beginner. The figures are drawn so that the primary layers and the organs derived from them can be respectively tinted in uniform colours throughout the book. While great additional clearness will be thus conferred, the student will gain much during the process of colouring. The illustrations are very numerous, and although many of them are roughly executed, and open to exception from an artistic point of view, their meaning is sufficiently clear. In the vast preponderance of the representations of sections among the illustrations, the work only follows the example of all books on the subject in all languages, but it is to be doubted whether the course of the young student is not somewhat impeded by this universal custom. There is no doubt that, as Prof. Weismann remarked to me the other day, the art of section-cutting is a weapon for morphological research equal in importance to the discovery of the microscope itself. And embryology, far more than any other department of morphology, depends upon this art; indeed, the study may be almost said to date from the introduction of this method of inquiry. Hence there is a strong bias in favour of representing structures in section; and in original papers and advanced treatises this custom is not to be deplored, for the reader knows exactly what is meant by the figures. But even in such works I think that the reader, and the author also, would benefit by the introduction of a few additional illustrations representing the organism, organ, or structure, as a solid object. But there can hardly be two opinions on this subject in the case of an introductory text-book. The beginner cannot readily or correctly reconstruct in imagination the solid structure from a representation of a section, and he must infallibly lose considerably in time by the prevalent custom of representing in only two dimensions objects which really exist in three. Long descriptions might be curtailed, and great additional clearness conferred by the frequent illustration of solid objects, out of which a small portion is represented as cut, on one side only, so as to show the internal structure. But this necessity is not fully recognized in any embryological text-book, although some attempt is made to deal with it in this and in other works. It is to be hoped that in future editions considerable attention may be paid to this mode of illustration, which will be more than repaid by the advantage conferred upon the young student.

In conclusion, the author seems to have included everything of importance in his subject up to the date at which the book was written; so that many important discoveries or theories are described which are necessarily absent from Balfour's work. When from the necessities of space these are only briefly touched upon, the reference to the original papers is to be found in Appendix B., containing a bibliography of recently published works on embryology Hence there is reason to hope that the volume may be found useful to the student who is familiar with Balfour's work.

E. B. P.

SOME MATHEMATICAL BOOKS.

The Conic Sections, with Solutions of Questions in London University and other Examination Papers. By G. Heppel, M.A. (London: Baillière, Tindall, and Cox, 1887.)

A New Mode of Geometrical Demonstration, with Examples showing its Application to Lines and Angles, Surfaces, and the Products of Three or more Straight Lines, &c. By D. Maver. (Aberdeen: A. Brown, 1887.)

Easy Lessons in the Differential Calculus: indicating from the Outset the Utility of the Processes called Differentiation and Integration. By R. A. Proctor. (London: Longmans, 1887.)

First Steps in Geometry: a Series of Hints for the Solution of Geometrical Problems, with Notes on Euclid, useful working Propositions, and many Examples. By R. A. Proctor. (London: Longmans, 1887.)

MR. HEPPEL'S little hand-book is not a complete treatise on elementary analytical geometry as usually presented to junior students, but it is a sequel to a previous small work in the same series ("Students' Aid Series"), "On the Geometry of the Straight Line and Circle." The object aimed at in the two works is to fully equip readers for the B.A. and B.Sc. examinations of the London University and similar examinations. Hence a limited portion only is discussed, viz. the equations to the conics; tangents, polars, normals, and curvature; sections of a cone, harmonic pencils, and miscellaneous theorems. Though Mr. Heppel has treated his subject concisely, he has not done his work in a perfunctory manner, for there is much originality exhibited in his mode of treatment, and he has discussed the general equation, not only for rectangular axes, but generally, in a very clear manner. If we mistake not, this clear exposition of a somewhat difficult part of the subject—difficult, that is, to junior students—is the outcome of some years' experience in tuition. In an appendix are given "hints to students" founded on this experience, and solutions to questions, illustrative of the text, which have been taken from the London University papers. There are a few errors in the printing, but they are not of a character to seriously inconvenience the student. We could have wished for a larger page, for then more justice would have been done to the author in the presentment of some of the formulæ. A student who carefully reads the text and transfers the formulæ for the separate conics to larger pages, ought to require no other text-book than this small one for the examinations named above.

Mr. Maver claims for his method the recommendation that it is new. One can hardly expect such to be the case, but we certainly do not remember to have come across it as here applied. The nearest approximation we can lay hands upon is the method of parallel transformation, given in Petersen's "Methods and Theories for the Solution of Problems of Geometrical Constructions" (pp. 46–47); but Mr. Maver has worked out the idea at considerable length and in an elegant manner An illustration from the principles and from the body o the work will sufficiently explain the scope of the method "Let AB and CD be two parallel straight lines, and AC GK, and GM any lines whatever drawn from the line AB to the line CD. If these lines, AC, GK, GM, move in the direction of the parallels AB, CD, so that CF = KL = MD, then we have the space CE = KB = GL (Euc. i. 36). Let the space CE be represented by *s*AC which may be read space generated by AC, and so on then *s*AC = *s*GK = *s*GM." Assuming results which readily flow from the above and which are given in th "Principles," let us now take Example II. The side AB, BC of a triangle are bisected by CE, AF, cutting i G, to prove CG = 2EG, AG = 2FG; and if BG pro duced cuts AC in D, then AD = CD. "Let AF be th direction of motion, then *s*CG = *s*CF = *s*BF = *s*B = 2*s*EA = 2*s*EG; ∴ since *s*CG = 2*s*EG, CG = 2EG In the same way AG = 2FG. Again, if DB be th direction of the motion we have *s*AD = *s*AB = 2*s*EB = 2*s*EG = *s*CG = *s*CD; ∴ since *s*AD = *s*CD, AD = CD. There are five chapters, viz. one, containing the principles two, applications to lines and angles; three, to square and rectangles; four, the products of three or mor straight lines; five, to lines that are in the same straigh line. In an appendix the author "proves Euc. i. 3

by the doctrine of motion without any reference to the propositions of Euclid which precede it." In all, the author applies his method to forty-one examples, the treatment of which will present no difficulty to the student. If the method is not thoroughly new, it is true, and offers an interesting field for investigation.

Many of our readers have no doubt made an acquaintance with Mr. Proctor's pages already, as they originally appeared in *Knowledge*. In his preface the author states as his experience:—"I could find no interest in the differential calculus, till, after wading through 200 pages of matter having no apparent use (and for the most part really useless), I found the calculus available for the ready solution of problems in maxima and minima. This little work has been planned with direct reference to my own experience at school and college." In 114 small pages Mr. Proctor very luminously, we think, unfolds the *raison d'être* of the calculus, and by easy yet sure stages carries his reader over a good deal of ground, sufficient for a large class of students. The proofs are clear, and apparently quite level to the comprehension of a student who has a solid, but not necessarily extended, knowledge of elementary mathematics. It is a good introduction to a great subject, and is calculated to entice readers to go on further. Several well-chosen problems are fully worked out, and there are a few others for the student to tackle himself. There are not many errata, but there are slips on pp. 52, 73, 95, 109, and quite a crop on p. 110.

"First Steps in Geometry" is a reprint; it certainly well deserves an extended circulation, especially amongst intelligent mechanics and others who cannot command the assistance of teachers, for they will appreciate the way in which the writer goes about his task. His "method of showing why such and such paths should be tried, even though some may have to be given up, in searching for the solution of problems," is likely to be very suggestive to the thoughtful student. There is a vast quantity of good work in the little book, and the way in which the second book of Euclid is treated ought to find a place in our school text-books.

OUR BOOK SHELF.

The Photographer's Indispensable Hand-book. Compiled by W. D. Welford. Edited by H. Sturmey. (London: Iliffe and Son, 1887.)

THIS book is practically a complete cyclopædia on the subject of photographic apparatus, materials, and processes, &c. Those who intend purchasing articles pertaining to photography cannot do better than look through these pages, where they will find a great amount of useful knowledge and information massed together in one volume.

It would be impossible to describe the various kinds of cameras and processes, &c., which are dealt with here, but we need only add that they are profusely illustrated and well classified.

One of the latest novelties in the way of secret cameras is shown in "the watch," which, when closed, is exactly like an ordinary watch. It is opened by a spring, when a series of about half-a-dozen tubes shoot into position.

A great assortment of the different make and kinds of drop-shutters which at the present time are so largely used for instantaneous work is added.

Ueber Gemüthsbewegungen. Von Dr. G. Lange. Autorisirte Uebersetzung von Dr. H. Kurella. (Leipzig: Theodor Thomas, 1887.)

THE original essay of which this is a German translation is in Danish, and was published in 1885. The author is a Professor of Pathological Anatomy in Copenhagen, and is well known both as a practical physician and as a man of science. He does not pretend to deal fully with the complicated and difficult questions connected with the expression of the emotions. He examines, however, with much care the physical accompaniments of sorrow, joy, terror, and anger; and he offers important suggestions as to the point of view from which the entire subject can be most successfully studied. That emotions are not, in any sense which can be recognized by science, the causes of the physical phenomena associated with them is a proposition on which he lays great stress; and in support of his opinion he presents a number of arguments which deserve the attention of all who are interested in psycho-physiological studies. The German translation is very clear, and will no doubt find readers in England as well as in Germany.

Three Lectures on the Forms of Nasal Obstruction in relation to Throat and Ear Disease. By Greville Macdonald, M.D. (London: A. P. Watt, 1887.)

THESE lectures were originally delivered at the Throat Hospital, Golden Square. They do not constitute a text-book, but the author has embodied in them the results of much inquiry as to various forms of nasal obstruction. The diseases of which he treats are all of common occurrence, yet some of them have hitherto been but inadequately described, and Dr. Macdonald holds that their pathology is often totally misunderstood. His exposition, therefore, should be of service both to the student and to the general practitioner.

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. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

"The Scenery of Scotland."

THE review by Mr. A. H. Green, of Geikie's "Scenery of Scotland," published in your paper of October 13 (p. 553), does not, I think, show any accurate appreciation of the disputable points in that work. The fundamental proposition ascribed to Hutton is "that the surface features of the land are, in the main, due to the carving and sculpturing action of denudation." Mr. Green does not seem to be aware that the truth of this doctrine may entirely depend on the definition of the words "surface features," and of the subsequent words "in the main."

As there is probably no actual "surface" existing in the world which has not been weathered more or less (except the very freshest lavas), the doctrine of Hutton, when so stated, is not only true, but it is a truism. Indeed the words "in the main" might be omitted; because it would be substantially true that all "surface features" in this literal sense are due entirely to denudation.

But if the words "surface features" be understood not literally, as confined to any mere visible surface, but as applying to all forms and shapes underlying mere surfaces, then the doctrine is open to great debate, and the truth of it turns entirely on the breadth of interpretation given to the words "in the main."

Living as I do in the Highlands, I maintain that the forms of our mountains have been largely determined by their geological structure, and by faults, contortions, and subsidences in the strata of which they are composed.

I cannot argue this question here. Suffice it to say that the "Great Gutter Theory," as I venture to call it, does not, in my opinion, explain our hills or our glens. There has been, no doubt, enormous denudation. But "in the main" the forms express structure, and the effects of subterranean force.

Mr. Green refers to the "graphic illustrations" of Mr. Geikie's book. But unfortunately those illustrations are sometimes very incorrect. For example, the general view given of the south-western termination of the Highland ranges, as seen from above Gourock on the Clyde, is a view as defective and incorrect as it is possible for a geological landscape to be. I know that range of hills well, and have seen it since my childhood in every variety of light and shadow. I have also drawn it frequently, and know almost every line of it by heart. It presents a section across a great anticlinal, as was first pointed out to me by Murchison; and it is full of surface markings which reveal its structure. Not one line of these is given in Mr. Geikie's drawing. If he had been sketching a set of mole-hills he could not have made them more featureless—more utterly devoid of their distinctive forms.

Let us have facts before theories. Let us have our hills so drawn as to express the lines of structure as they are seen in Nature, and in their relation to outline. But very often the eye sees nothing except what the brain behind it has preconceived; and a geologist who draws a mountain with a theory of guttering in his head, is pretty sure to make a mess of it.

There is really nothing in the argument about an average level along the tops, as any sure indication of an original "table-land," with all its hollows due to guttering. All sedimentary materials having an average composition, when subjected to strains, pressures, or fractures, would, and must, exhibit average resulting forms. This general fact is equally consistent with more than one explanation.

I believe Mr. Geikie has modified his former views as to the action of ice. A closer inspection of the Highlands will, I am convinced, modify greatly in other ways his teaching as to the small share which structure, and subterranean force, have had in determining the physical geography of the country.

October 15. ARGYLL.

In your last issue Prof. A. H. Green, reviewing Dr. A. Geikie's "The Scenery of Scotland viewed in Connexion with its Physical Geology," described the alleged resemblance between the Durness fossils and certain North American types as "an announcement of the greatest interest." The fact is certainly of the "greatest interest," but the "announcement" was made nearly thirty years ago by the late J. W. Salter in the *Quarterly Journal* of the Geological Society, 1858, p. 381. Mr. Salter refers to the fauna as "this truly North American assemblage," and compares the species one by one with Prof. Hall's types.

CH. CALLAWAY.

Wellington, Shropshire, October 16.

[We have referred these letters to Mr. Green, who has sent us the following reply.—Ed.]

It is well known that the Duke of Argyll has long been a strenuous and consistent opponent of the views as to the origin of the surface features of the earth which are accepted by the majority of geologists. Indeed, if I had been disposed to be personal, I do not think that I could have quoted a more pertinent illustration than his Grace of a fact in the history of opinion to which I drew attention in the opening part of my review of the "Scenery of Scotland." He hears not Moses and the prophets, and I fear he will not be persuaded by the pleadings of one of their humbler followers; but if he will let me have my small say, I will first point out that his objection to the expression "surface features" seems to me to savour a little of quibbling. It is a general rule of criticism to interpret any ambiguous words by the context. The whole tenor of my article shows that I did not use the words in the first of the two meanings which the Duke says they may bear. Again, I am quite prepared to admit that geological structure has had a large share in determining the form of the ground; and I cannot find that either Dr. Geikie, or any other upholder of the Gutter Theory (I thank thee, Duke, for teaching me that word: no happier designation could be found), denies that subterranean force has played an important part in determining the physical geology of a country. Rather the contrary, for hear Dr. Geikie himself. He avows himself wishful that his reader should "recognize that a belief in the paramount efficacy of superficial denudation in the origin of the features of the land is compatible with the fullest admission of the existence and potency of subterranean disturbance. Inability to make this recognition," he says, "has led to absurd misconceptions and misrepresentations of the views of those who hold that the topography of the land is essentially the result of a process of sculpture" ("Scenery of Scotland," pp. 95, 96).

I will leave Dr. Geikie to take care of himself and defend the drawing the accuracy of which is impugned by his critic. I do not know the special landscape of Fig. 19, but I have enjoyed a few panoramic views of Highland scenery, and I can honestly say thus much: I have everywhere recognized those surface *markings* (may I again congratulate his Grace on the happiness of this phrase?) which indicate the geological structure of the ground beneath, but I have in every case been still more struck by that general flat-toppedness on which special stress is laid by Dr. Geikie. The comparatively slight prominence given to these surface markings in Fig. 19 will be easily understood if w bear in mind the one point which that cut was intended to illustrate.

I may add that I am extremely sorry if any words of mine seem to imply that I grudge my old friend Salter the credit due to him with regard to the Durness fossils. The expression I have used could be made to bear this meaning, and I am much obliged to Dr. Callaway for giving me an opportunity of disavowing any such intention. A. H. GREEN.

Leeds, October 20.

A Hydroid Parasitic on a Fish.

During my studies the past summer at the Newport Marine Laboratory I captured a single specimen of an osseous fish, *Seriola zonata*, Cuv., which exhibits a most interesting example of parasitism or possibly commensalism. Upon the outer wall of its body an extraordinary hydroid was found to have attached itself. As this mode of life is unique for a hydroid, it is thought that a mention of it, and a statement of the peculiar modifications which the hydroid has suffered, may be not without interest to others besides special students of the jelly-fishes. The hydroid is new to science, and on that account the name Hydrichthys is suggested to designate it. The hydroid will later be described and figured under the name *Hydrichthys mirus*, gen. et sp. nov.

The colony of Hydrichthys is found on the side of the body and near to the anal fin of the fish, Seriola. It forms a reddish cluster or patch of bodies, and was at first mistaken for a fungoid growth. When it was examined by means of a microscope its animal nature was easily seen and its hydroid affinities clearly made out. The fish was kept alive in an aquarium and medusæ raised from the attached hydroid. The hydroid colony is composed of two sets of individuals. These two kinds of individuals arise from a flat plate formed of branching tubes, by which the colony is attached to the body of the fish. The two kinds of individuals noticed in the cluster are the sexual bodies (gonosomes), and the "filiform bodies" (structures of unknown function).

The sexual bodies have the form of grape-like clusters of buds mounted on small contractile peduncles, which branch from a central axis or stalk. The filiform bodies are simple, elongated, flask-shaped structures, destitute of appendages, with a central cavity and terminal orifice. Neither of these two kinds of individuals have tentacles around or near a *mouth opening*, nor any structures which can be compared with these bodies, which are almost universal among fixed hydroids.

The first kind of individuals are the gonosomes or sexual bodies. They arise from the flat basal plate of branching tubes, by which the union of the colony with the outer wall of the fish is effected. Each hydroid gonosome consists of a main stem with lateral branches. At the end of each lateral branch there is a crowded cluster of small buds, which are immature jelly-fishes in all stages of growth. Each gonosome resembles a bunch of reddish and orange-coloured grapes.

The filiform bodies are simpler in structure than the sexual clusters or gonosomes. They are destitute of tentacles and are flask-like, with a cavity and terminal orifice. They are very sensitive, and move about with freedom, never, however, being detached. The fish, Seriola, was kept alive until the larger buds of the grape-like gonosomes separated from the hydroids. These buds are medusæ, different from any which I have ever seen, but with close affinities to common and well-known genera. A large glass aquarium containing several gallons of water was found to be swarming with these medusæ two days after the capture of the Seriola.

Each fully-grown medusa closely resembles the genus Sarsia. It has an oval bell, four broad unbranched radial tubes, and four long simple tentacles. There are no octocysts on the margin of the bell.

If the strange form of the hydroid was not known to me, it would have been very easy to call this medusa a near relative of Sarsia. The medusa belongs to a group, called by Agassiz the Tubularians, but its hydroid is different from that of any other member of the group.

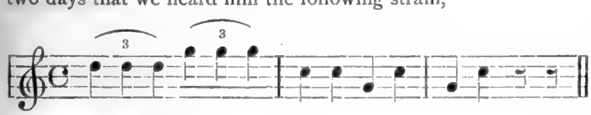
One other parasitic hydroid may be thought to be related to Hydrichthys. I refer to the Polypodium, described from the ova of the sturgeon. A description of Hydrichthys with figures of the fish (Seriola) to which it is attached, and of the hydroid with its medusa, will soon be published by me. As a discussion of its relation to other hydroids has little interest except to a specialist in the study of medusæ, a comparison of Hydrichthys with Polypodium and other genera is reserved until my complete diagnosis of the genus and species. J. WALTER FEWKES.

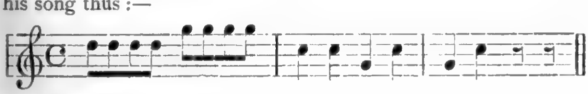
Cambridge, Mass., U.S.A.

Music in Nature.

IN NATURE for August 11 (p. 343) there is an interesting article on music in Nature; the writer evidently being inclined to deny that true musical notes, and especially several notes in succession having a musical relation to one another, can be found in bird songs. However this may be in the Old World, we have in the New at least one example of a bird which not only sings, or rather whistles, pure and well-sustained musical notes, but has a succession of notes with such intervals as to form a simple melody. I refer to the scarlet tanager.

While we were at The Thousand Islands early in the summer of 1886, one of these brilliant fellows carried on a courtship among the trees close to our cottage, repeating incessantly during the first two days that we heard him the following strain,



in a clear, bright whistle. After the first two days he changed his song thus:—

and during the three weeks that we heard him he made no other variation, except that he occasionally repeated the last two notes a third time, thus filling out the bar. The notes were taken down by a trained musician, and if whistled give the tanager's song exactly.

It may be mentioned that, though perhaps the most brilliant in plumage of our Canadian birds, the male tanager referred to made no attempt at concealment, but swept like a living flame from tree to tree close to the cottage, and when singing preferred to sit on the topmost bough of a pine near by.

A. P. COLEMAN.

Faraday Hall, Victoria University, Coburg, Ontario, October 8.

Swifts.

THE following facts relating to the habits of the swifts were observed by paying close attention to these remarkable birds during the past summer. For more than a month, *i.e.* from June 1 to July 12, we watched them here. On the fine evenings about forty of them (the males I believe), ascended high into the air at about 9 o'clock, and after wheeling about for a minute or two, screaming loudly, fled straight away, sometimes in one direction, sometimes in another. White, in the "Natural History of Selborne," notices that: "Just before they retire whole groups of them assemble high in the air, and squeak and shoot about with wonderful rapidity." But the most wonderful part of the proceeding is that they do not come down again that night. At all events I can show that they do not come down again before 10.30, at which time I do not think they would be able to find their nests under the eaves of the church. Between the dates above-mentioned there were only six days during which I did not see or hear the swifts ascend and fly off. Three of these days were rainy, and the swifts stayed at home, and on three other days I was not able to watch them. The churchyard adjoins the garden of this house, and numbers of swifts build in the church, which is but a few feet from where we sit out and walk about in the summer evenings.

After seeing the high-flying swifts safely off to the south-west at 9.10 one night, I sat on a tombstone under the north eaves where most of them build, until 10.30. Two swifts hawking low for flies entered their nests after 9.10, but one of them was flying low while the high-flyers were in sight, and the other came out of its nest after they had gone, and both had retired before 9.20. On the other side of the church my father (the vicar) and my brother, who both took a keen interest in the doings of the swifts, were keeping watch alternately, and only two low-flyers were out there after the others had gone. The high-flyers did not return. On several other nights we watched until 11 o'clock, though not quite continuously, but quite closely enough to make certain that none returned. I think it most probable that owing to the darkness they do not return until the break of day, and further, that they *remain on the wing all night.* This last feat, though sufficiently startling, will, I am convinced, not be deemed impossible by those who have had good opportunities (and made use of them) for studying the ways of swifts and their wonderful powers of flight. As far as my observation goes, the swift settles nowhere except at its own nesting-place.

I shall be very glad of any information tending to throw light upon the question, and I shall be very pleased to give any of your correspondents any further information within my knowledge concerning this curious habit of the swifts, and the proofs thereof, to set out which in this letter would take up too much of your valuable space.

White also says (p. 180, original edition) he has never seen the swift carrying materials to its nest, and suggests that it usurps that of the sparrow. This does not accord with my own observation here. I have repeatedly seen swifts taking bents of grass in their beaks to their nests, and I have again and again scattered feathers on the wind from the sound-holes in the steeple, and from the steps of the cross in the churchyard, and seen them eagerly seized within a few feet of my head by numerous swifts. Their nests are neat, small, and shallow, and very firm, the materials being glued together by the viscous saliva of the builders. AUBREY EDWARDS.

The Vicarage, Orleton R.S.O., Herefordshire, October 13.

Hughes's Induction Balance.

HAVING just made a Hughes's induction balance, I have, in the course of some experiments with it, observed what was new to me, for I have not seen it mentioned in any account of the balance. I take the liberty, therefore, of asking through your columns whether the explanation resolves itself into the difference between paramagnetic and diamagnetic substances. The apertures of my bobbins are 1½ inch in diameter; my primary current is from three Daniell's, and the break is a bent steel spring whose free point just grazes the surface of a mercury cup, so that the merest touch with a finger causes a series of regular breaks. Now, if I place an iron or steel disk, or ring, such as a key-ring, inside the aperture, the telephone sounds loudly if the plane of the disk or ring is at *right angles* to the plane of the coils; but very very faintly if it is *parallel* to the plane of the coils. On the other hand, if a disk, or ring, or coil of wire, of any of the diamagnetic metals—copper, brass, zinc, silver, gold, aluminium, lead—be used, the telephone sounds loudly if the plane of the disk or ring be *parallel* to the plane of the coils; but very faintly, if at all, when it is perpendicular to the plane of the coils. Further, if a short *bar* of soft iron, or of nickel, be inserted so that the length of the bar is parallel to the plane of the coils, almost no sound is heard; but if it be turned through a right angle so as to be perpendicular to the plane of the coils, the sound is a maximum. Have we in this simple instrument the ready means of distinguishing paramagnetic from diamagnetic substances? J. COOK.

Central College, Bangalore, S. India, September 26.

PROF. KIRCHHOFF.

GEHEIMRATH GUSTAV ROBERT KIRCHHOFF was born at Königsberg on the 12th of March, 1824. He commenced his professorial career at Berlin University as Privat Docent; became Extra-ordinary Professor in Breslau from 1850 to 1854, thereafter till 1874 Professor of Physics in Heidelberg, whence he was finally transferred (in a somewhat similar capacity) to Berlin. His health was seriously and permanently affected by an accident which befell him in Heidelberg many years ago, and he had been unable to lecture for some time before his death.

It is not easy, in a brief notice, to give an adequate idea of Kirchhoff's numerous and important contributions to physical science. Fortunately all his writings are easily accessible. Five years ago his collected papers (*Gesammelte Abhandlungen* von G. Kirchhoff, Leipzig, 1882) were published in a single volume. His lectures on Dynamics (*Vorlesungen über Mathematische Physik*, Leipzig, 1876) have reached at least a third edition; and his greatest work (*Untersuchungen über das Sonnenspectrum*, Berlin, 1862) was, almost immediately after its appearance, republished in an English translation (London, Macmillan). To these he has added, so far as we can discover, only three or four more recent papers; among which are, however, the following, published in the *Berlin Abhandlungen*:—

Über die Formänderung die ein fester elastischer Körper erfährt, wenn er magnetisch oder diëlectrisch polarisirt wird. (1884.)

A subsequent paper gives applications of the results (1884).

Additions to his paper (presently to be mentioned) on the Distribution of Electricity on two Influencing Spheres. (1885.)

While there are nowadays hundreds of men thoroughly qualified to work out, to its details, a problem already couched in symbols, there are but few who have the gift of putting an entirely new physical question into such a form. The names of Stokes, Thomson, and Clerk-Maxwell will at once occur to British readers as instances of men possessing such power in a marked degree. Kirchhoff had in this respect no superior in Germany, except his life-long friend and colleague v. Helmholtz.

His first published paper, *On electric conduction in a thin plate, and specially in a circular one* (Pogg. Ann. 1845), gives an instance. The extremely elegant results he obtained are now well known, and have of course (once the start was given, or the key-note struck) been widely extended from the point of view of the pure mathematicians. The simpler results of this investigation, it must be mentioned, were fully verified by the author's experimental tracing of the equipotential lines, and by his measurements of their differences of potential. A remark appended to this paper contains two simple but important theorems which enable us to solve, by a perfectly definite process, any problem concerning the distribution of currents in a network of wires. This application forms the subject of a paper of date 1847.

Kirchhoff published subsequently several very valuable papers on electrical questions, among which may be noted those on conduction in curved sheets, on Ohm's Law, on the distribution of electricity on two influencing spheres, on the discharge of the Leyden Jar, on the motion of electricity in submarine cables, &c. Among these is a short, but important, paper on the *Determination of the constant on which depends the Intensity of induced currents* (Pogg. 1849). This involves the absolute measurement of electric resistance in a definite wire. Kirchhoff was also the inventor of a valuable addition to the Wheatstone Bridge. To the above class of papers may be added two elaborate memoirs on Induced Magnetism (*Crelle*, 1853; *Pogg. Ergänzungsband*, 1870).

Another series of valuable investigations deals with the equilibrium and motion of elastic solids, especially in the form of plates, and of rods. The British reader will find part of the substance of these papers reproduced in Thomson and Tait's *Natural Philosophy*. There are among them careful experimental determinations of the value of Poisson's Ratio (that of the lateral contraction to the axial extension of a rod under traction) for different substances. These results fully bear out the conclusions of Stokes, who was the first to point out the fallacy involved in the statement that the ratio in question is *necessarily* 1/4.

Kirchhoff's *Lectures on Dynamics* are pretty well known in this country, so that we need not describe them in detail. Like the majority of his separate papers they are somewhat tough reading, but the labour of following them is certainly recompensed. They form rather a collection of short treatises on special branches of the subject, than a systematic digest of it. One of the most noteworthy features of the earlier chapters is the mode in which dynamical principles (*e.g.* the *Laws of Motion*) are introduced. While recognizing the great simplification in processes and in verbal expression which is made possible by the use of the term Force, Kirchhoff altogether objects to the introduction of the notion of *Cause*, as a step leading only to confusion and obscurity in many fundamental questions. In fact he roundly asserts that the introduction of systems of Forces renders it impossible to give a complete definition of Force. And this, he says, depends on the result of experience that in natural motions the separate forces are always more easily specified than is their resultant. He prefers to speak of the motions which are observed to take place, and by the help of these (with the fundamental conceptions of Time, Space, and Matter) to form the general dynamical equations. Once these are obtained, their application may be much facilitated by the introduction of the *Name* Force; and we may thus express in simple terms what it would otherwise be difficult to formulate in words. So long as the motion of a single particle of matter only is concerned we can, from proper data, investigate its velocity and its acceleration, as directed quantities of definite magnitude. Thus we proceed from Kepler's Laws to find the acceleration of a planet's motion. This is discovered to be directed towards the sun, and to be in magnitude inversely as the square of the distance. We may call it by the name Force if we please, but we are not to imagine it as an active agent. Something quite analogous appears in the equations of motion when we introduce the idea of Constraint. The mode in which the idea of Mass is introduced by Kirchhoff is peculiar. It is really equivalent to a proof (ultimately based on experiments) of Newton's *Third Law*. Once, however, it is introduced, the same species of reasoning (which differs but slightly from what we should call Kinematical) leads to the establishment of D'Alembert's and Hamilton's *Principles*, with the definition of the Potential Function, the establishment of Lagrange's Generalized Equations, and the proof of Conservation of Energy, &c. The observational and experimental warrant for this mode of treatment is, according to Kirchhoff, the fact that the components of acceleration are in general found to be functions of *position*. [Kirchhoff's view of Force has some resemblance to, but is not identical with either of, the views previously published by Peirce and by the writer.] This is the chief *peculiarity* of the book, and very different opinions may naturally be held as to its value, especially as regards the strange admixture of Kinematics and Dynamics.

Of the rest, however, all who have read it must speak in the highest terms. A great deal of very valuable and original matter, sometimes dealing with extremely recondite subjects, is to be found in almost every chapter. Among these we may specially mention the investigation of surface conditions in the distortion of an elastic solid, with the treatment of capillarity, of vortex-motion, and of discontinuous fluid motion (*Flüssigkeitsstrahlen*).

Besides these definite classes of papers, there is a number of noteworthy memoirs of a more miscellaneous character:—on important propositions in the Thermodynamics of solution and vaporization, on crystalline reflection and refraction, on the influence of heat conduction in a special case of propagation of sound, on the optical constants of Aragonite, and on the Thermal Conductivity of Iron.

Finally we have the series of papers on Radiation, partly mathematical partly experimental, which, in 1859 and 1860, produced such a profound impression in the world of science, and which culminated in the great work on the solar spectrum whose title is given above. The history of Spectrum Analysis has, from that date, been one of unbroken progress. Light from the most distant of visible bodies has been ascertained to convey a species of telegraphic message which, when we have learned to interpret it, gives us information alike of a chemical and of a purely physical character. We can analyze the atmosphere of a star, comet, or nebula, and tell (approximately at least) the temperature and pressure of the glowing gas. But, at the present time, the fact that such information is attainable is matter of common knowledge.

This is not an occasion on which we can speak of questions of priority, even though we might be specially attracted to them by finding v. Helmholtz and Sir W. Thomson publicly taking (in full knowledge of *all* the facts) almost absolutely antagonistic views. However these points may ultimately be settled, it is certain that Kirchhoff was (in 1859) entirely unaware of what Stokes and Balfour Stewart had previously done, and that he, with the powerful assistance of Bunsen, MADE what is now called Spectrum Analysis: Kirchhoff, by his elaborate comparison of the solar spectrum with the spectra of various elements, and by his artificial production of a new line whose *relative* darkness or brightness he could vary at pleasure; Bunsen by his success in discovering by the aid of the prism two new metallic elements.

P. G. TAIT.

ON THE SIGNIFICATION OF THE POLAR GLOBULES.[1]

IT has long been known that the egg of some animals, after becoming mature and before undergoing its embryonic development, throws out certain bodies of globular form, which take no part in the embryonic development, but perish sooner or later. These polar globules have been found on the eggs of nearly all classes of animals, and it has been proved that they are real cells, composed of nucleus and cell-body.

Several theoretical opinions have been expressed in regard to their signification. Some naturalists believe them to be only a kind of excretion of the egg; others even think them to be of no functional importance, and perceive in them only a remnant of some ancestral process, a recapitulation of some ancient part of the phylogenetic development.

Now it is true that, in many animals, structures occur without any physiological value, but it is also known that such structures—as, for instance, the hind-legs of whales—disappear more and more in the lapse of phylogenetic development. Furthermore, such rudimentary organs never disappear in all species and genera of a large group simultaneously, but in one genus or species they persist longer than in another. Thus, some whales possess certain of the bones of their hind-legs lying between the muscles of the trunk, whilst others have preserved only one bone of the pelvis. Now the polar globules might have been regarded as insignificant and rudimentary as long as they were only known in a few groups of the animal kingdom. But as their existence is now proved in nearly all classes of animals, and as they appear in all of them in the same manner, we are compelled to assume that they possess at least some physiological significance.

Mr. Sedgwick Minot and your illustrious Balfour made a great step forward in attempting—each independently of the other—to attribute a high importance to the expulsion of the polar globules. As you know, they suggested that the egg-cell was originally hermaphrodite, and that the polar globules were the male portion, which had to be thrown off. They based their idea upon the generally accepted view, according to which fecundation is the union of a specific male with a specific female substance. If this is true, then the fecundated ovum contains both these substances in equal quantities; and the observations upon the segmentation of the egg lead further to the conclusion of E. Van Beneden, that all cells of the body contain these two substances, and that they are all hermaphrodite. The throwing out of polar globules was, according to these views, the means of preventing parthenogenesis, which must have occurred if the male substance had remained in the egg. This was Balfour's opinion, and he formulated the same with all precaution, putting it forward as a supposition, which might prove true or not. He himself even pointed out the way by which a decision could be obtained, in his statement, that, if his theory was true, polar globules would not be found in parthenogenetic eggs. Certainly, if polar globules represent the male substance, they cannot be thrown out in an egg that is not destined to be fertilized, and which therefore would not receive the male substance from another cell

Now, I have tried to decide this question by observing whether parthenogenetic eggs throw out polar globules or not, and I discovered several years ago that polar globules certainly exist in parthenogenetic eggs. I have found them in the summer eggs of Daphnidæ, and later, assisted by my pupil Mr. Ishikava, of Tokio, I have also found them in the parthenogenetic eggs of Cypridæ and of Rotatoria.

Now it is impossible that these polar globules can contain the male part of the egg, and the question arises, What other significance can be attributed to them?

When I ascertained the facts which I have just described, I was not at the time aware of another fact that I am about to lay before you, and which seems to me to possess an important bearing upon the meaning of polar globules, and of sexual propagation in general. This fact is a very simple one: Parthenogenetic eggs throw out only *one* polar body, whilst sexual eggs throw out two of them.

The importance of this fact lies in the significance of the substance that is thrown out in the polar globules or polar cells. You know well that it is a true cell-division which leads to the formation of polar globules, and that the first polar cell takes away from the egg-cell one-half of the nuclear substance. You are also aware that the second polar cell again takes away half of the nuclear substance remaining in the egg. Hence in sexual eggs three-quarters of the nuclear substance originally contained in the egg-cell are taken away by the two polar cells. In parthenogenetic eggs only one polar cell is formed, and consequently only one-half of the original mass of nuclear substance is removed from the egg-cell.

Now you know that nuclear substance is a very important thing. The experiences and reflexions of the last ten years have led to the general conviction that nuclear substance is the part that controls the whole cell, and that the entire structure as well as the functions of the cell depend upon its minute structure. The nuclear substance is the idioplasma of the botanist Nägeli. Upon the molecular structure of it the form and function of every

[1] Paper read by Prof. August Weismann before the British Association at Manchester.

cell in the body depend, and consequently the form and function of the whole body are determined by the nuclear matter or idioplasma of the first cell, the egg-cell—parthenogenetic or fertilized.

If this theoretical view is correct, then we must be astonished that so much of this important nuclear substance is lost to the egg-cell—namely, one-half by the parthenogenetic ovum, and half as much again by the sexual one. What can be the cause that renders it necessary for this to happen before the egg-cell is able to develop into an embryo?

I will give a short account of my ideas upon the subject.

(1) The nuclear substance or idioplasma of the first polar body must be detrimental to the further development of the egg, for it is thrown out in all kinds of eggs, parthenogenetic as well as sexual, and the embryonic development never begins before the first polar cell has been expelled. Now, if the nuclear substance truly controls the cell and compels it to take a certain shape and a certain histological structure, there must be such a substance, such an idioplasma, also in the youngest egg-cells. This idioplasma causes the egg to develop a yolk possessing a certain colour and structure, it causes the egg to form a shell of a certain thickness and structure; briefly, it compels the young egg-cell to attain a degree of histological differentiation which it did not previously possess. For the youngest egg-cells are essentially similar in most animals, whilst mature egg-cells are very different and can often be very well distinguished in different species. The specific idioplasma of the growing egg-cell—I call it ovogenetic idioplasma—cannot be the same as that contained in the nucleus of the mature egg, and which controls the development of the embryo. It cannot be that idioplasma which determines the development of a certain egg-cell into a duck and not into a swan; it cannot be that kind of idioplasma which I have called *germ-idioplasma*, or simply *germ-plasma*.

Of course there must also be germ-plasma in the *young* egg-cell; I believe that in the youngest germ-cells there is no other idioplasma than germ-plasma, and that this germ-plasma changes into ovogenetic plasma, only a very small part of germ-plasma remaining unaltered.

This remaining part grows with the growth of the egg, and finally attains the same volume as the ovogenetic idioplasma. Then the division of the nuclear substance takes place, and the superfluous ovogenetic substance is removed in the first polar globule, whereupon the egg-cell contains only germ-plasma.

This is my explanation of the removal of the *first* polar cell.

(2) In regard to the second it is clear that an egg that contains only germ-plasma should be capable of undergoing embryonic development, unless the quantity of germ-plasma should prove to be too small. But this is not the case. Parthenogenetic eggs enter upon embryonic development immediately after the expulsion of the first polar globule. Sexual eggs do not thus develop, and we have to inquire into the reason for this. I believe it is because they throw out a second polar cell, which takes away one-half of the germ-plasma left within the egg-cell. After this the quantity of germ-plasma is too small for entering upon embryonic development, and therefore the egg-cell remains undeveloped, unless the lost quantity of germ-plasma be replaced in the process of fertilization. Embryonic development takes place immediately after the union of the germ-plasma of a spermatozoon with the remaining germ-plasma of the ovum. Consequent upon this the quantity of germ-plasma in a fertilized egg again becomes equal to that which was present after the separation of the first polar globule, and also equal to that which enters upon embryonic development in the parthenogenetic egg.

This is perfectly simple, but a great difficulty remains. Why is it necessary that the sexual egg should throw out half of its germ-plasma; why does it not retain the whole quantity of this important substance?

You would perhaps answer, Because the quantities of male and of female germ-plasma, that are united by fecundation, must be equal. Indeed, the facts of heredity lead to the opinion that these two kinds of germ-plasma must be equal in quantity, and we have microscopical observations recorded by Van Beneden, Carnoy, and others, which further support this conclusion. But if the quantity of germ-plasma must be equal in both, why should the germ-plasma of the egg increase so largely as to attain twice the volume of the germ-plasma of a spermatic cell? Nature is not so wasteful as to throw away so important a substance for nothing. There must be an adequate cause why in sexual eggs the germ-plasma must be halved before fecundation can take place.

I believe I can point out the reason why this is necessary, but before I do so I must beg you to first enter with me upon a few theoretical considerations on the subject of heredity.

Heredity depends upon the germ-plasma, as I have said before; the minute molecular structure of the germ-plasma causes the egg-cell to develop into a duck or a swan, it also causes the egg to develop into a Negro or into a European, into a Mr. Smith or into a Mr. Jones; in short, all qualities of the developed individual depend upon the constitution of this germ-plasma. In my opinion sexual propagation implies the union of two different germ-plasmas to form the single nucleus of the egg-cell; and the two substances that are united in the process of fertilization I believe to be equal in size and quantity.

Now let us suppose that we lived at a time when sexual propagation had not yet existed, and that we were present at its origin. We should then observe the union of two different germ-plasmas, both of the same size and quantity, but of a slightly different molecular constitution, one coming from one parent and the other coming from another. Both substances must be thoroughly homogeneous—that is to say, they must be composed of particles that are equal in their chemical, molecular, and morphological constitution. Let us illustrate this by a diagram, in which we represent each germ-plasma as a thread or a loop, which we know to be the microscopical form of germ-plasma and of nuclear plasma in general. For simplicity's sake we will represent only one loop for the germ-plasma of each parent. We have then two loops, the first representing the peculiarities of the germ-plasma of one parent, and the second representing the peculiarities of the other parent, and we will discriminate between them by making the first green and the second red.

These two individual kinds of germ-plasma unite and form together the nucleus of the fertilized egg, which develops into a new individual of the second generation. This individual will form again germ-cells, and each of these germ-cells will contain a germ-plasma, which is not homogeneous, as before, but composed of two halves, derived respectively from the two parents. In each succeeding generation the germ-plasma must attain to a more complicated constitution, it must contain twice as many different kinds of germ-plasma as were contained in the germ-plasma of the preceding generation. If we follow this development of the germ-plasma for a few generations, we shall find that union will take place by sexual propagation between the germ-plasmas of two individuals of the second generation, each containing two different kinds of germ-plasma. In this way the individuals of the third generation will be formed possessing germ-cells which contain four different kinds of germ-plasma. I have called these different kinds of germ-plasma *Ahnenplasma*, a word that can be rendered in English by the term *ancestral plasma*. By sexual propagation the individuals of the third generation would give rise to individuals of the fourth generation, and the germ-cells of these last individuals must contain eight different ancestral plasmas; similarly the germ-cells of the fifth generation must contain sixteen ancestral plasmas, and so on. It is thus clear that in a very small number of generations the composition of the germ-plasma must become extremely complicated: by the tenth generation it would already contain 1024 different ancestral plasmas.

We do not know how far this may go, because we do not know how small are the primary elements of germ-plasma, nor do we know how many of these elements may be indispensable for the youngest and smallest germ-cells. But if we imagine these elements to be excessively small, this process of doubling the number of ancestral plasmas in each generation must have come to an end after a certain number of generations, whether they were 10, 20, 100, or 1000!

From the time at which the germ-plasma first attained its utmost complexity further sexual propagation was only possible by halving the number of ancestral plasms contained in the germ-plasma. Clearly, this process of halving ought to take place in male germ-cells as well as in female ones, but at this moment we are only sure of its existence in the latter. We have seen that one-half of the germ-plasma contained in the nucleus of the egg-cell is expelled in the second polar cell. That the nuclear substance thus expelled is true germ-plasma, is not a mere supposition, but a certainty. We know of developing eggs which are either fertilized or unfertilized, and in the latter case they develop by parthenogenesis. Such are the eggs of the honey-bee. We may assume that if these eggs remain unfertilized they will expel only one polar globule, but that if, on the other hand, they are penetrated by a spermatozoon they will also expel the second globule. Thus the same idioplasma that is expelled from the fertilized egg remains, and forms half of the first segmentation-nucleus in the parthenogenetic egg. It must therefore be true germ-plasma.

I do not doubt that this is the true significance of the formation of a second polar globule. We can see the necessity on theoretical grounds for the removal of half the number of ancestral germ-plasmas; and we *actually* find that half of the germ-plasma *is* removed in every sexual egg.

If this reasoning be correct, our views on sexual propagation must undergo a total change. Fertilization is no longer an unknown impulse given to the egg-cell by the entrance of a spermatozoon, but it is simply the union of the germ-plasmas of two individuals. The spermatozoon is no longer the spark which kindles the powder, or the relatively small force which converts potential into actual energy, but it is merely the carrier of germ-plasma of a certain individual, possessing the necessary qualities for reaching, penetrating, and fusing with the bearer of germ-plasma from another individual. There are no *essential*, but merely individual, differences between the nuclear substance of the spermatozoon and that of the ovum. There are no such things as male or female nuclear substances, but merely male and female cells, carriers of the immortal germ-plasma. The differences are wholly individual and of merely secondary importance, and nothing corresponding to the ordinary notions implied by the terms male and female exists in germ-plasma.

If this be so, then it is clear that the fact of sexual propagation demands a new explanation. We must attempt to explain the reason why Nature has insisted upon the rise and progress of sexual propagation. If we bear in mind that in sexual propagation twice as many individuals are required in order to produce any number of descendants, and if we further remember the important morphological differentiations which must take place in order to render sexual propagation possible, we are led to the conviction that sexual propagation must confer immense benefits upon organic life. I believe that such beneficial results will be found in the fact that sexual propagation may be regarded as a source of individual variability, furnishing material for the operation of natural selection. I believe that sexual propagation has become prevalent among the higher organisms for the purpose of conserving and multiplying that individual variability which owes its first origin to the Protozoon condition of such higher organisms. But it is not now my purpose to speak further upon this subject: I have already discussed it elsewhere ("Die Bedeutung der sexuellen Fortpflanzung für die Selections-Theorie," Jena, 1886).

Whatever is to be said for the above hypotheses, the facts I have the honour of bringing before you to-day seem at least to prove that sexual propagation depends on the removal of half of the germ-plasma of the egg and the replacement of it by the same quantity of germ-plasma of another individual. This is now a fact which may be regarded as indisputable; and, further, the existence of true parthenogenesis is now proved beyond doubt. For we know now that an egg which expels only one polar globule enters without delay into embryonic development, inasmuch as it has retained the whole of its germ-plasma.

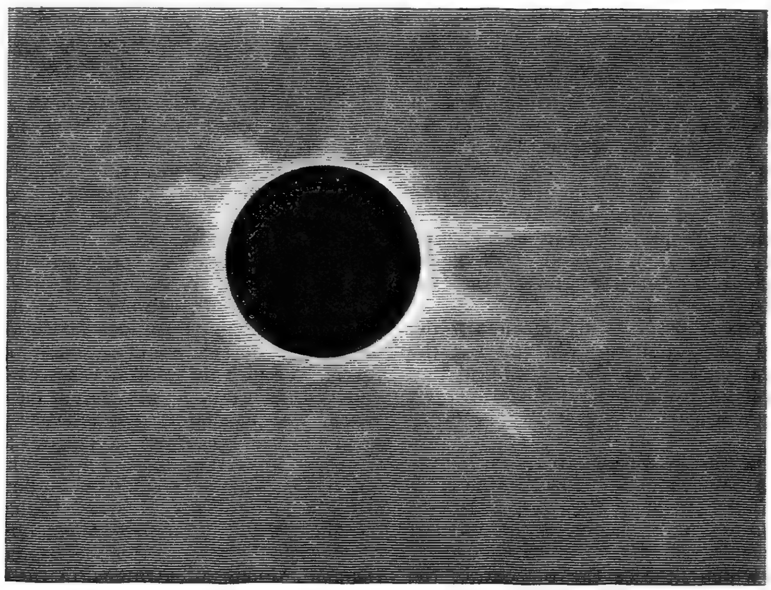
THE TOTAL ECLIPSE OF LAST AUGUST IN JAPAN.

THE eclipse has come and gone, and our little party is on its way home with a few papers and a small box of glass plates—a rather meagre showing for the hard work of our summer months. Although we were so unfortunate as to have uninterrupted cloud throughout the entire duration of the eclipse, our expedition to Japan has not been so dismal a failure all told. Apart from sundry observations of minor importance contributed by volunteer observers at scattering stations for whom I had prepared instructions, Dr. W. J. Holland, who joined the Expedition at my invitation as naturalist, has been actively engaged in botanical and entomological research in fruitful fields, and has a good harvest to report. He has also valuable notes upon his ascent of Nantaisan, Asama-yama, and Nasutake (which latter he appears to have been the first foreigner to ascend); while the separate expedition to the summit of Fuji-san (12,400 feet), which I had the pleasure to carry out under the auspices of the Boyden Fund of the Harvard College Observatory, and on which I had the highly-valued co-operation of Dr. E. Knipping, Meteorologist of the Japanese Weather Service, resulted, among other things, in the determination of its rare fitness as a site for astronomical observation—of which more elsewhere.

With reference to the preliminaries of the Eclipse Expedition it is necessary to state that early in the present year the trustees of the Bache Fund of the National Academy of Sciences, Washington, made a grant to Prof. Newcomb, the Superintendent of the *Nautical Almanac*, for observing the total solar eclipse of August 19, 1887. Prof. Newcomb determined the general lines of research to be undertaken, decided upon locating the observing-station in Japan, and placed me in charge of the Expedition. After some weeks of preparation in Washington and elsewhere, I set out for Japan on June 9, and arrived in Yokohama a month later. There was no small difficulty attending the definitive location of the instruments for observing the eclipse, owing to the deficiency of precise meteorological information regarding the region of the shadow-path. All existing data were kindly placed at my disposal by the officers of the Japanese Government, and several of the departments contributed in other ways to the assistance of an expedition which, had the skies been favourable, could not have failed of entire success.

Going northward from Tokio along the line of the Japanese Railway Company, to the courtesy of whose president, Mr. Narabara, our Expedition is deeply indebted, we pass through Utsunomiya at the distance of 65 miles, and at 113 miles reach Shirakawa, a town of about 10,000 inhabitants, and situate 10 or 12 miles north of the centre of the shadow-path. Here I found a spot forming, in many respects, an ideal location for an eclipse-station. Within a quarter of a mile of the telegraph-office and railway-station, and in an unfrequented part of the town, was the ruin of the celebrated old castle erected some 300 years ago, and occupied by the Abe family until the revolution of 1868. Permission to establish my station on the castle walls was given by Count Oyama, the Minister of State for the Army, under whose control this and similar castles elsewhere in Japan, formerly possessed by the Daimios, now are. The massive walls rise to a height of about 80 feet above the surrounding plain, and afford a capital foundation for the instrumental equipment—not to say the seclusion so desirable in the mounting and adjustment of delicate apparatus. Of the mountain-range 25 or 30 miles to the west and north-west, and its cloud-creating propensity, we had apparently little need for fear—in fact, a month's residence in the castle gave us a large proportion of afternoons on which an entirely satisfactory record of the eclipse in all its stages could have been secured.

Our main instrument was a horizontal photoheliograph of nearly 40 feet focal length, with which we hoped to expose 100 plates during the partial phases of the eclipse; but I had determined also to attempt coronal photography with the same instrument, hoping to obtain eight or ten negatives of the corona of such size that subsequent enlargement would be undesirable. At the focus of this telescope the sun's image has a diameter of 4½ inches, and dry plates 17 × 20 inches had been provided for this work. Also an extra mirror, finely silvered by Brashear, was taken along for the heliostat, to replace the unsilvered mirror ordinarily employed, shortly before totality came on. After the special modifications of the exposing-shutters and plate-holders had been made, and a light-proof tube or camera the whole length of the telescope had been constructed, periodic drill for the work of eclipse-day was at once begun. For some minutes immediately before the beginning and after the end of totality, the partial phase exposures were to be made every fifteen



seconds, while the large plates for the corona, with exposures varying from one second to sixty-four seconds, were to be handled as rapidly as possible: we found that there was a loss of about five seconds between the plates, or something like one-sixth the entire duration of totality. With a very efficient photographic corps, and the drill which we all underwent, I had the best of reasons for anticipating complete success.

As was foreseen, too, we found in photographing artificial crescents—very slender ones—that no image of the plumb-line appeared on the plate; there was thus no initial line of reference for the measurement of position-angles. Mr. Hitchcock, whom I appointed photographer of the Expedition, undertook a variety of experiments to overcome this difficulty, and with entire success; the form of apparatus finally adopted will be detailed in the report of the Expedition. To assist in the operations of the photographic house, we were fortunate in securing the services of Mr. Ogawa, of Tokio, a Japanese photographer of wide experience, and Dr. Y. May King, of Amoy, also a highly-skilled manipulator.

As wet plates appeared to me preferable in many ways to dry ones for the partial phases, Mr. Hitchcock and Mr. Ogawa instituted a thorough series of experiments in the preservation of sensitized films, at first with glycerine, and subsequently more successfully with sugar. The results of this work made the wet plate, with its fine-grained film, as available for rapid manipulation in the photography of celestial phenomena as the dry plate has hitherto been found to be. It was shown that the plates might with entire safety be removed from the sensitizing bath from two to four hours before exposure and development, if treated with the sugar preservatives, and proper precaution was taken to keep the films from drying. The details of this process will be embodied in the report of the Expedition. As an extreme test we exposed, on the day after the eclipse, a box of the plates which had been sensitized and preserved for eclipse-work some twenty-six hours previously, and found that they gave sun-pictures photographically perfect.

In compliance with orders issued by the Secretary of the Navy, two officers from the Asiatic squadron, Lieut. Southerland and Chief-Engineer Pemberton, of the U.S.S. *Monocacy*, reported to me for eclipse-duty on my arrival in Yokohama, and their services were zealously and most effectively rendered. In addition to his work as executive officer, I placed Lieut. Southerland in charge of the 9-foot coronagraph, sent out by the Pickerings from the Harvard College Observatory. The objective of this instrument was the 7¼-inch Clark glass of the equatorial of my own Observatory at Amherst, while the dry plates, with the instructions for their manipulation, were identical with those furnished by the Pickerings to Prof. Young, who carried to his Russian station a 6⅜-inch Merz glass, also lent from the larger transit-instrument of Amherst College Observatory. I have not yet been able to learn whether Prof. Young was favoured with a clear sky during totality; but, if he was, it is the more regrettable that clouds covered the sun at Shirakawa, as the first serious attempt to obtain trustworthy evidence of rapid changes in the corona has thereby come to nought. It will be many a year before another eclipse occurs with two stations geographically so well placed for this special research as were Russia and Japan. Prof. Pickering desired me, if practicable, to place all or a portion of the corona-apparatus provided by this Observatory on the summit of one of the mountain-peaks of which there are several adjacent to the centre of the shadow-path, notably Nantaisan, 8500 feet high. Dr. Holland made the ascent of this mountain about the middle of July; but his report of its difficulties, together with the highly probable cloudy condition of the summit during the eclipse, led me to abandon farther consideration of this mountain; while the other peaks were too far removed from Shirakawa to permit of occupation with the time and assistance at my disposal. The remainder of Prof. Pickering's apparatus was therefore mounted alongside the photoheliograph at the central station; the double coronagraph, two 5-inch lenses of about 3 feet focus, being operated by Dr. Ames, U.S. Navy, while Dr. D. B. McCartee attended to the exposures with the 4-inch short-focus camera, and Mr. C. R. Greathouse to the exposures of plate-holders for determining the actinic effect of the coronal light.

The valued service of Mr. Pemberton is worthy of special mention here in rendering the photoheliograph less unwieldy for rapid work than I had found it formerly. By means of an ingeniously-devised system of cords and pulleys, led from the heliostat into the photographic house, the reflecting mirror was placed under the immediate and constant control of the chief astronomer making the exposures: it was thus possible to dispense with the customary assistant at the heliostat pier for adjusting the mirror in right ascension and declination. A very simple device made it possible to see the bright reflected image of the sun while at my post in the dark room, and adjust it accurately on the plate without opening the exposing-slide.

The importance of Newcomb's and Langley's observations of the outer corona in 1878, and attempted by Lockyer in 1886, had not escaped me, and I had an occulting-disk mounted on a rod attached firmly to the gable of the photographic house, so that its shadow as cast by the eclipsed sun would fall about 50 feet away, in the area inclosed by the upper castle wall. Here I stationed Mrs. Todd, provided with all the paraphernalia for seeing and sketching in their correct relations the faint outlying streamers of the corona.

Of two 3½-inch telescopes lent by Admiral Yanagi, Hydrographer of the Imperial Japanese Navy, one was reserved for the optical observation of first and fourth contacts, and the search for intra-Mercurial planets; while the other was committed to Dr. Holland, a skilled artist, with instructions to sketch as far as possible all the details of the corona adjacent to the solar poles.

Mr. Nakagawa, the Director of the Naval Observatory, with his assistant, made a thorough series of meteorological observations throughout the eclipse period, following the system elaborated by Von Bezold and recommended by the German Meteorological Conference for the observers in Russia. On the north-west corner of the castle wall I stationed Mr. K. Açino, a student of astronomy in the University, to make detailed and precise observations of the diffraction bands, and to observe if possible the sweep of the lunar shadow across the extensive rice-fields below.

The purely eclipse results of the work at Shirakawa were disheartening in the extreme. The forenoon gave us a perfect sky, with no indication whatever of approaching cloud: all were confident of entire success. But about an hour before the time of first contact, a slender finger of cloud began to rise from the west, coming at first directly above the summit of Nasutake, a volcano about 25 miles away, and which had sprung into unwonted activity during the past night, belching forth for hours enormous volumes of smoke and steam. The sun was entirely invisible during the first half-hour of the eclipse, when a brief interval of partly clear sky gave time for adjusting the heliostat and making ten or twelve exposures. The sun being very faint, only five of these photographs are available for measurement; and these were the only pictures that could be taken with the photoheliograph. The dense clouds, leaving a large clear area most of the time about the zenith, lay over the sun until the eclipse was past, save only a moment shortly after totality, when there was a partial clearing, but too brief, and the sun too faint, to allow of the necessary adjustment of the reflecting mirror.

As totality drew near, it suddenly occurred to me that a good observation of second contact might be possible by watching for the approach of the moon's shadow among the clouds; but my attempt to do this failed, the light appearing to me too much diffused to permit of anything better than a rough approximation to the time of contact. I found subsequently among Mrs. Todd's notes of the eclipse that totality appeared to her to come on, not evenly, but as if by jerks—a phenomenon which may, I think, have been due to the extinction of the sun's light from one cloud after another, as the lunar shadow advanced over the north-western sky. The weather-map for August 19, which came to our station from Tokio the day after the eclipse, gave us some idea of the odds we had been labouring against: the sheet for 2 p.m. showed clouds at all stations of the Meteorological Service except one, and that far removed from the belt of totality. In general, the whole of the main island was obscured on the eventful afternoon, and a view of the eclipse was permitted only to those so fortunate as to be located in the line of the small apertures, here and there, through the general cloud area. These were numerous enough to enable voluntary observers, scattered over the central portion of the belt of totality, and for whom I had prepared instructions, to obtain a goodly number of drawings of the corona. These instructions had been translated into Japanese, and printed and distributed through the co-operation of the Department of Education and the Bureau of Geography of the Department of the Interior. Altogether there are something like a hundred such drawings; but their value is uncertain until they are properly collated. Much the best drawing which I saw was made by Mr. Shuji Isawa, Chief of the Bureau of Compilation of the Department of Education, who was fortunate enough to be located in a spot in Western Japan, where totality was seen in a nearly cloudless sky. He has kindly furnished me with photographs of his drawing, one of which is inclosed.

Other Expeditions in Japan fared ill also—some of them worse than my own. That sent out from the University in charge of Prof. Terao, and located a few miles south of Shirakawa, at Kuroiso, experienced not only heavy clouds, but much rain during the eclipse, and no observations could be made. At Sanjo, on the central line and south-east of Niigata, Prof. Arai, Director of the Meteorological

Observatory, was able to make successful exposures for the corona with a small telescope. It was reported clear during the whole eclipse at Choshi, a point on the eastern coast near the southern limit of total obscuration, but there were no observers or instruments there for scientific work. It was reported cloudy throughout the whole eclipse at Niigata; while a party of observers who had ambitiously climbed to the top of Nantaisan brought down a record of nothing but clouds and fog. On the whole, Japan appears to have been an uncanny spot to lead an eclipse-track across. DAVID P. TODD.

s.s. *Port Victor*, September 20.

THE MÄRJALEN SEE.

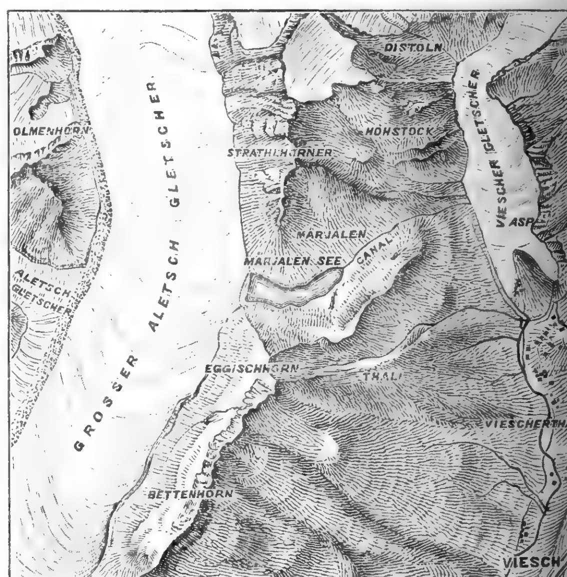
LAKELETS, in which the ice-crags of a glacier are mirrored, in which miniature bergs may be seen to float, are of occasional, though of rare, occurrence in the Alps—as for example the Lac de Ste. Marguerite, at the foot of the Ruitor glacier; but the Märjalen See, so far as I know, is unique of its kind. It is not formed at the foot of a glacier, either by partial occupation of a shallow basin worn by the ice-stream in its days of greater strength, or by the pounding back of the glacier torrent by an old terminal moraine; but it is on one side of a glacier, which makes a dam across an upland glen. This barrier at times yields to the pressure of the accumulated water sufficiently to allow of its escape beneath the great ice-stream, and it is a recent incident of the kind, noticed in the *Times* of September 30, which has suggested the present article.

The Great Aletsch glacier, as is well known, is the largest ice-stream not only in the Oberland group, but also in the Alpine chain. Its upper basin is fed by the snows of an almost complete ring of grand peaks, the most conspicuous of which, enumerated from west to east, are the Aletschhorn, the Jungfrau, the Mönch, and the Viescherhörner. All these are considerably above 13,000 feet, and there are several others, less familiar to the ordinary tourist, which either rise slightly above that elevation, or are only a very few hundred feet below it. The great *eismeer* thus formed passes out as a single stream through a "gate in the hills," between the crags of the Faulberg on the east and the base of the Dreieckhorn on the west. This gap is rather more than a mile across, and the glacier for several miles is not less, and is generally rather more, than its breadth at this place. It flows at first slightly to the east of south, then runs almost due south, and finally sweeps gradually round to the south-west. This deflexion is caused by the Eggischhorn, which rises like a great pyramid full in face of the upper course of the glacier to a height of 9649 feet above the sea, or nearly 2000 feet above the surface of the ice. At this spot the Märjalen See is situated, at a height of 7710 feet above the sea, rather less than five miles below the "gate," and rather more than that distance above the end of the Aletsch glacier. This sweeps on along the west flank of the Eggischhorn, until it terminates in the grand gorge of the Massa, at no great distance from the Bel Alp Hotel, a worthy rival in beauty of situation to that on the Eggischhorn.

The Märjalen See is thus formed: the range of the Eggischhorn is continuous with that which makes the left bank of the Aletsch glacier, and divides its comparatively unbroken surface from the narrower and more shattered mass of the Viesch glacier. But this range to the north of the Eggischhorn is deeply notched, so that it is possible to quit the Aletsch glacier and without ascending to reach a depression, barely so high as the surface of the ice, from which one looks down a steep slope on to the surface of the Viesch glacier. From this depression a shallow valley descends towards the west, and is barred, as mentioned above, by the great glacier of the Aletsch. Thus a lake is formed, fed by various streamlets from the slopes on either side and by the melting of the glacier ice. Though now the shrunken stream of the Aletsch glacier does not diverge towards the lake, it was no doubt formerly divided by the opposing mass of the Eggischhorn; then one of its arms occupied the bed of the lake, and after passing over the depression joined itself to the Viesch glacier. Some geologists regard the basin of the Märjalen See as wholly due to the excavatory action of this offshoot of the Aletsch. To myself it appears to be the upper part of a valley, produced in the ordinary way, but subsequently modified in its outlines by the rasping action of the glacier.

At somewhat irregular intervals, but according to popular belief once in seven years, the ice-dam yields sufficiently to allow the pent-up waters to escape beneath the glacier, when the contents of the lake are discharged rapidly down the gorge of the Massa, and, after devastating the fields below, are poured into the Rhone, near Brieg.

In the summer of 1858 I had the good fortune to see the Märjalen See both full and empty. On my first visit I found a lake full 300 yards across at the lower end, and



about three times as long. From the rocky margin on either side the ice arched up in a low flattened curve until its edge at the highest point was about 60 feet[1] above the level of the water. From this it rose in a vertical cliff of almost unbroken ice, of purest white, which was mirrored in the still blue water below. Here and there miniature icebergs were floating, in colour if possible yet purer than the parent glacier, and above the water at the foot of the cliff was a band of turquoise blue. This was produced by the fresh surface of the ice, for the cliff is under-cut by the action of the water of the lake, and to this under-cutting the bergs are no doubt partly due.

Next evening I revisited the spot. To my surprise all was changed: the lake had almost wholly disappeared; the glacier cliff which the day before had been doubled by reflexion, was now doubled in reality. Below the upper zone of white ice was now a zone of more than

[1] Probably the height at the present time is not so great. Since 1858 there has been a considerable shrinking in the glaciers of the Alps. When I visited the Märjalen See in 1881, the greatest height of the cliff did not appear to me to exceed 30 feet. In 1858 Prof. Ramsay found by measurement that the greatest height of the cliff above water was 60 feet, and the greatest depth of the lake at its foot 97 feet ("Peaks, Passes, and Glaciers," 1st series, p. 461).

equal thickness of the most exquisite blue; on the dry bed of the lake were stranded the bergs which the day before had floated in its waters, and we could now appreciate their true size. One whose shape we had greatly admired now appeared even more beautiful with its fantastic pinnacles and blue recesses. It was, I estimated, from 30 to 40 feet high, nearly as wide, and considerably longer. Two cubical masses on the opposite shore were in colour the most lovely turquoise blue that I have ever seen. These, no doubt, on the previous day had appeared as mere slabs on the surface. The bed of the lake was covered with a fine mud, on which were numerous tracks or castings, which I attributed to a worm. I did not see any shells, so that probably no mollusks live in the chilly waters of the Märjalen See.[1]

The ice in this part of the Aletsch glacier is comparatively little crevassed. This permits the glacier to act as a dam; the drainage of the lake is no doubt due to some accidental rupture which opens a communication, quickly enlarged by the running water, with the sub-glacial drainage of the glacier.

A traveller in August 1872 was so fortunate as to see the actual escape of the water at the lower part of the glacier. He describes it as follows[2]:—"It was 4.50 p.m. when we arrived (at the Bel Alp Hotel). The domestics drew our attention to a sound like the roar of a cataract, which seemed to descend the Aletsch. For a time the sound was sub-glacial, but a yellow torrent at length appeared on the opposite side of the glacier, smoking and roaring as it tumbled down the declivities of ice. The front of the torrent soon appeared opposite to the Bel Alp, carrying every movable thing along with it. Wishing to get near the torrent, I descended rapidly to the glacier, crossed it, and succeeded in getting quite close to the rushing water. Everywhere impetuous, it was divided into spaces of tolerably uniform slope, separated from each other by steep and broken declivities, down which the water plunged with tremendous fury. At the base of one of these falls it was met by a kind of reflecting surface, by which the rhythmic character of the motion was finely revealed. The water here was tossed upwards in a series of vast parallel fans, carrying with them ice-blocks and stones, and breaking above into a spray as fine as smoke. A bend of the glacier came in for the lateral portion of this spray, and over it the rounded blocks of ice and the stones were showered like projectiles. The sound of the torrent had not abated at bed-time, but this morning all is quiet, and no water is to be seen in the temporary channel."

This sudden discharge of so great a body of water, in addition to damaging the fields immediately below, very considerably raises the level of the Rhone. On the last occasion, September 4, the writer quoted at the beginning of this article states that "the level of the Rhone rose at Brieg 5½ feet, from about 3½ feet to 9 feet, and at Sitten 4 feet, from 6½ feet to 10½ feet. The greatest rise observed since the regulation of the Rhone from the same cause took place on July 19, 1878, and although it was then at Brieg only 5 feet, and at Sitten only 3 feet, it was considered a very fortunate circumstance that the event took place at a very low level of the Rhone for the season."[3] He adds that to avoid such a danger in future it is proposed to enlarge greatly a channel which many years since was cut through the moraine stuff overlying the rock east of the lake, and so provide an outlet towards the Viesch glacier. By this "the volume of its waters will be reduced to about half what it is at present (10,000,000 cubic metres)." So that future travellers will not see the Märjalen See in its full beauty. The lake formed by the advance of the Gétroz glacier, in the upper part of the Dranse valley, the bursting of which in 1818 wrought such fearful devastation, may be regarded as to some extent a parallel case with the Märjalen See, of a more temporary nature, but on a grander scale.

In Sir Charles Lyell's "Principles of Geology" (chapter xvi.), and again in his "Antiquity of Man" (chapter xiv.), are notices of the Märjalen See, and of some beach terraces formed by its waters. He regards it as illustrative of the celebrated parallel roads of Glenroy, but, though this explanation has found very general favour with geologists, I must confess myself unable to accept it. But into this thistle-bed of controversy I must not permit myself to wander. T. G. BONNEY.

[1] Ramsay found the temperature of the water near the ice-cliff to be 3° C.

[2] J. T. quoted from the *Times* in *Alpine Journal*, vol. vi. p. 100.

[3] An account of this is given by F. V. Salis (*Jahrb. Schweiz. Alpencl.* 1878-79, p. 549. The discharge on this occasion was at first slow. It began at 8 a.m. July 18; by 4 p.m. the lake had sunk 1 metre; during the darkness it sank 4 metres, and by 3 p.m. most of the contents were gone. It was estimated that only 700,000 cubic metres of water out of 10,000,000 remained.

THE BACILLUS OF MALARIA.

A PAPER of unusual interest in relation to the question of the agency of microphytes in the production of disease will shortly appear in Prof. Cohn's botanical *Beiträge* (vol. v. part 2). For many years the efforts of pathologists have been directed in this relation to the subject of malaria. The local conditions which determine the "endemic" prevalence of ague have been studied with considerable exactitude. They are such as to indicate very clearly that the material cause of intermittent fever, although it is generated in the soil, acts through the air. The fact that its influence is restricted within very narrow limits of distance from its source indicates that it is not diffusible like a gas or vapour, but consists of particles which, on various grounds, are surmised to be living organisms of extreme minuteness. Can this be established on evidence which will bear criticism?

All will remember that in 1879 Tommasi Crudeli published (in conjunction with Prof. Klebs) observations which tended to show that in malarious districts a Bacillus inhabits the soil which can be cultivated so as to yield a product capable, when inoculated, of producing in animals a fever of intermittent type, accompanied by the anatomical characteristics of malarious infection. Subsequently it was found by several observers that, during the cold stage of ague, spore-containing Bacilli, conjectured to be identical with those of Crudeli, are to be found in the blood.

These results have been received by pathologists with much misgiving, partly because the experimental proofs appeared inadequate, partly because other observers failed in their endeavours to verify them. Dr. Schiavuzzi, a medical practitioner at Pola, on the Adriatic, appears to have been more fortunate. Following the methods of Dr. Koch, he has sought for organisms in the air of the malarious district near the town in which he resides, and with such success that he is able, in repeated observations, to obtain without fail pure cultivations of a Bacillus which is not only indistinguishable as regards its structure from that of Crudeli, but also produces in animals the characteristic symptoms and pathological changes which belong to ague. The first communication of Dr. Schiavuzzi's results was made to the Accademia dei Lincei more than a year ago (see *Rendiconti*, vol. ii. 1886, April 4), but excited very little attention. It so happened that in the course of the past summer Prof. Cohn visited Pola, and so became acquainted with Dr. Schiavuzzi, who, during the present year, has been pursuing his investigations. In consequence, Prof. Cohn has been able to repeat the Pola experiments in his own laboratory at Breslau, and, so far as possible, to confirm the discovery. The writer had the opportunity, a short while ago, when Prof. Cohn was in England, of reading the proofs of Schiavuzzi's paper, and of seeing the very perfect photographs of the Bacillus which have been made of it at Breslau.

Although it may be admitted that evidence of a more conclusive kind than any which has been offered by Dr. Schiavuzzi is required to establish the truth of his inference, yet there seems to be good reason for thinking that he has approached much nearer to a solution of the question than any of his predecessors. J. B. S.

NOTES.

AT the meeting of the Academy of Sciences, Paris, on the 17th inst., Admiral Mouchez spoke of the preparations which are being made for executing the photographic charts of the heavens. Ten of the photographic telescopes, seven by MM. Henry and Gautier, of Paris, and three by Sir H. Grubb, of Dublin, are expected to be finished by the end of 1888 and forwarded to various observatories in France, Spain, South America, and Australia. With the promised co-operation of England, the United States, and Russia, it is hoped that a good beginning will be made in 1889, and that the vast undertaking will be completed within the time anticipated by the International Congress of last April.

PROFS. CAYLEY, F.R.S., AND M. J. M. HILL retire from the Council of the London Mathematical Society. The new names selected by the Council for submission to the Society at its annual meeting (November 10), are those of Mr. A. Buchheim and Dr. J. Larmor. The De Morgan Medal, which we have already announced as awarded to Prof. Sylvester, F.R.S., will be presented to him at the aforesaid meeting.

ON December 8 next, Herr Friedrich Traugott Kützing will be eighty years of age, and a good many men of science in Germany are anxious to give emphatic expression on the occasion to their respect for his character and their appreciation of his labours. Herr Kützing was one of the first to recognize that the best material for the study of cells and their life is provided by the simplest plants; and the results of his researches are well known to all biologists. It is proposed that a gift of some kind shall be presented to him on his eightieth birthday, and an influential Committee has been appointed to make the necessary arrangements. If any English students of biology would like to associate themselves with their German colleagues in this matter, they should communicate with the secretary of the Committee, Herr Otto Müller, 44 Köthenerstrasse, Berlin, W.

M. NOLAN, student at the Geological Laboratory of the Sorbonne, has been intrusted by the French Ministry of Public Instruction with a mission to study the geology of the Balearic Islands.

A NOTIFICATION in the *Calcutta Gazette* states that the Maharanee of Cossimbazar has given twenty thousand rupees for the promotion of technical education in the Moorshedabad district. The donor of this munificent sum proposes that five thousand rupees shall be spent in purchasing the necessary apparatus and instruments. The interest on the remainder is to be devoted to endowing a class in the Berhampore Collegiate School, and establishing classes in connexion with the college class in some of the neighbouring elementary schools. The Lieutenant-Governor of Bengal accepts the gift, and approves of the scheme proposed by the Maharanee.

AN interesting address on English and foreign technical education was delivered last Saturday by Prof. Silvanus Thompson, at the Aldingham Institute, a school of technical education in Goldington Crescent, St. Pancras. In the course of his address Prof. Thompson drew attention to the fact that in Berlin there is a great State-aided institution in which every known industry is taught. This institution he described as a large building—as big as Buckingham Palace, if not quite so beautiful—standing on a site of about 12 acres. It had something like 500 rooms for technical teaching, and, in fact, was a perfect college, with a good library. He believed that the entire cost of that establishment had been about £960,000. It was a building which had cost about the same amount as one of our ironclads. The entire maintenance of the institution was about £38,000 per annum; but that expenditure and the original outlay were recouped by the well-to-do character of those who had passed through its teaching, and thus had become useful members of society instead of drags upon the country. For such a country as England a site not of 12 acres but of 40 acres would be required. It might cost £4,000,000 to build and £100,000 a year to maintain, and from the results which would follow it would be cheap at the price, for it would enable us to obtain every possible requirement of life from our own handicraftsmen, instead of having to go to foreign countries for what we need. It would do more, for the superiority of English workmanship would cause fresh demands to be made from us, not only throughout our own colonies, but from foreign countries also.

THE Geodynamical Committee of the Italian Meteorological Society held meetings at Aquila from September 6 to 8. A preliminary meeting had been held at Florence in May last to prepare the work of the present one. The object was to formulate practical and uniform directions for the seismological researches undertaken by the Society, and to deduce from our actual knowledge rules to be followed in the construction of houses, so as to diminish the risk of damage in earthquakes and undulatory motions. M. Bertelli, of Florence, explained the theory of his new bifilar instrument for determining the least tremors of the earth, and he was invited to prepare directions for its construction, erection, and use, to be added to the report of the discussion. He also described his apparatus for the protection of telephones from lightning. The choice of a type of seismograph was the subject of a long discussion, and a Committee was nominated for the study of this very important question. The consideration of the best mode of collecting, discussing, and publishing the seismic and micro-seismic observations now being made in Italy was referred to the same Committee. On the consideration of the rules to be followed in the construction of buildings, M. de Rossi, of Rome, indicated the further observations which ought to be made. MM. Denza, De Giorgi, Roberto, Bertelli, and Galli also took part in the discussion. The resolutions adopted will be printed, and distributed to the various municipalities for the instruction of the persons concerned. This meeting of students of seismology is the first that has been held in Italy. The Secretary of this enterprising Society is Dr. O. L. Bianco.

THE other day the Committee of the Chester Society of Natural Science passed a resolution, which was entered in their minutes, expressing their deep sense of the loss the Society had sustained through the death of Mr. John Price, which took place on Friday, the 14th inst. He had reached the ripe age of eighty-four. Upwards of forty years ago, when residing at Birkenhead, Mr. Price occupied himself with the fauna of the Birkenhead shore, and the value of his researches was recognized by various scientific investigators of acknowledged eminence. The last of his observations on the shore were embodied in a paper on the pluteus of the starfish, read before Section D of the British Association meeting at Liverpool in 1852. To the Proceedings of Section D of the British Association he contributed papers from time to time at subsequent meetings. Having settled at Chester in 1858, Mr. Price was soon the means of establishing in the city a Society for the study of natural history. Ten years later, after the appointment of the late Charles Kingsley to a canonry in the Chester Cathedral, this Association was merged in the present Society, founded by Mr. Kingsley, who was one of Mr. Price's intimate friends. Mr. Price accepted, and retained until his death, the position of Chairman of the Botanical Section of the Society. The resolution to which we have referred concludes as follows: "We shall miss from our meetings and excursions his venerable form, his familiar voice, and his wise counsels, but the name of 'Old Price' is one which will ever live in the Society as that of one of our revered fathers, and one of Nature's truest disciples and humblest and most loyal children."

THE last number of the Journal of the Royal Asiatic Society (N.S. vol. xix.) contains a short paper by Prof. de Lacouperie on the Miryeks, or stone men of Corea. These are huge half-length human figures, carved in stone, and looked upon as relics of a religion of former times. Those described by Mr. Carles in his paper on Corea, read before the Royal Geogaphical Society last year, are about 25 feet high, cut out of some large boulders in the middle of a fir wood in a hill-side. The largest hitherto known is at Unjin, and is shown in a plate prefixed to the paper. It stands about 62 feet high; and the body and head resemble those of the idols in Buddhist temples. A column about 10 feet high runs up from the head, giving support to an oblong slab about the same length; on this stands a smaller column supporting another slab, and from the corners of the two bells are pendent by chains. Prof. de Lacouperie points out some peculiarities about the word Miryek, and suggests that perhaps it is not Corean at all; it may have existed in Corea in its special adaptation to the huge stone statues, without having preserved its original meaning previous to the adoption of Chinese characters. If this be correct, it implies that the religion which produced the erection of the statues was then forgotten or in the shade. They might, he suggests, be due to an early spread of Buddhism in Corea. But it is evident that we must know more of that country before the origin of these curious survivals is clearly explained.

ANOTHER contribution to our knowledge of the group of beautiful dye colouring matters known as safranines has just been published in the current number of the *Comptes rendus* by MM. Barbier and Vignon. It has been known for some time that the nitroso-derivatives of the tertiary aromatic monamines in acting upon the primary monamines give rise to colouring matters, but their nature has hitherto remained undetermined. MM. Barbier and Vignon, however, in the light of their previous work, set out to explore this interesting side group of substituted safranines, with the following successful result. Starting with para-nitroso-dimethyl aniline, $C_6H_4NON(CH_3)_2$, they studied the action of one equivalent of its hydrochloride upon one equivalent of aniline in alcoholic solution, and found that a reaction occurred sufficiently violent to boil the alcohol. Eventually the product dissolved, forming a solution at first yellow, afterwards gradually changing to brown, and finally to bright violet-red, which on cooling deposited a solid. After filtration, washing, and repeated recrystallization this solid was obtained pure in brilliant brown crystalline spangles. Analysis showed that its composition was $C_{16}H_{20}N_4$, and from its reactions there can be little doubt that it is tetra-methyl diamido-azo-benzene. It is not very soluble in water, but, as is characteristic of all the safranines, is soluble in concentrated acids, forming deep-red or violet solutions. This, however, is not the only substance formed during the above reaction, for sodium chloride precipitated from the violet mother-liquor a second crystalline colouring matter, which turned out to be identical with the well-known dimethyl phenylene safranine, $C_{20}H_{18}N_4$. The formation of this latter body helps to explain the course of the reaction, which probably runs as follows: $3[C_6H_4NON(CH_3)_2HCl] + 2C_6H_5NH_2 = C_{16}H_{20}N_4 + C_{20}H_{18}N_4HCl + 3H_2O + 2HCl$. The work of MM. Barbier and Vignon, and of all other workers in this direction, is the more interesting inasmuch as it combines industrial utility with the advancement of pure chemistry, on the one side handing over new materials to the manufacturer, and on the other new facts to the already immense number which stand to the credit of the last few years.

THE *Times* reprints, from a document issued by the Berlin Bureau of Statistics, some interesting information about what is called "the motive force of the world." It appears that four-fifths of the engines now working in the world have been constructed during the last twenty-five years. France owns 49,590 stationary or locomotive boilers, 7000 locomotives, and 1850 boats' boilers; Germany has 59,000 boilers, 10,000 locomotives, and 1700 ships' boilers; Austria 12,000 boilers, and 2800 locomotives. The force equivalent to the working steam-engines represents—in the United States, 7,500,000 horse-power; in England, 7,000,000; in Germany, 4,500,000; in France, 3,000,000; and in Austria, 1,500,000. In these figures the motive power of the locomotives is not included, whose number in all the world amounts to 105,000, representing a total of 3,000,000 horse-power. Adding this amount to the other powers we obtain the total of 46,000,000 horse-power. A steam horse-power is equal to three actual horses' power; and a living horse is equal to seven men. The steam-engines of the world represent, therefore, approximately the work of 1,000,000,000 men, or more than double the working population of the earth, whose total population is supposed to amount to about 1,455,923,000 inhabitants. Steam has accordingly trebled man's working power.

IN the report presented at the eighteenth meeting of the Sunday Lecture Society it is stated that the attendance at the lectures during the last session was less than in the previous year. The Committee also announce that the accounts show an increased balance against the Society. On the other hand, they note with pleasure that good progress is being made by kindred associations in various parts of the country.

A SERIES of elaborate "geological studies," relating to the Dutch West Indies, by Prof. K. Martin, of the University of Leyden, is being issued by E. J. Brill, of Leyden. These "studies" embody the results of researches which Prof. Martin himself has carried on. In the first instalment, which has just been issued, he deals with the geology of Curaçao, Aruba, and Bonaire. This instalment consists of 140 handsome, well-printed pages, and is illustrated by three coloured geological maps, two plates, and thirty-six woodcuts.

A VALUABLE "Statistical Atlas of India," prepared for the Colonial and Indian Exhibition, 1886, and printed at Calcutta, may now be obtained at Mr. Edward Stanford's, Charing Cross. The object of the work is to give a general idea of the character of the country, its inhabitants and agriculture, with the addition of such statistics as may serve to illustrate commercial and educational progress. The maps have been prepared and printed in the office of the Surveyor-General of India at Calcutta, under the special supervision of Colonel Waterhouse and Major Strahan; and the chapters are by writers specially conversant with the subjects with which they deal.

MESSRS. CASSELL AND CO. have ready a new and enlarged edition of "Colour," a scientific and technical manual treating of the optical principles, artistic laws, and technical details governing the use of colours in various arts, by Prof. A. H. Church; and cheap editions of "The Fresh-water Fishes of Europe," by Prof. H. G. Seeley, F.R.S., and "Short Studies from Nature," a series of familiar papers, by eminent authors, on interesting natural phenomena, with full-page illustrations and diagrams.

NEW catalogues of scientific books have just been issued by Messrs. Macmillan and Bowes, Cambridge, Messrs. Dulau and Co., London, and Mr. W. P. Collins, London.

The Cambridge Scientific Instrument Company have published a descriptive list of "anthropometric apparatus," consisting of instruments for measuring and testing the chief physical characteristics of the human body. This list cancels those previously issued. The instruments have been designed under the direction of Mr. Francis Galton.

A Biological and Microscopical Section has been formed in connection with the Cardiff Naturalists' Society, with Dr. C. T. Vachell, M.D., as President, and Prof. W. N. Parker as Hon. Sec. The inaugural meeting was held on Thursday evening, October 20, in the Biological Department of the University College, when papers were read on the work of the Section by the President and Hon. Sec. The objects of the Section are stated to be the promotion of the study of biology generally, but more especially of the local flora and fauna, including marine as well as land forms.

It is announced that the following lectures will be delivered at the Royal Victoria Hall, Waterloo Bridge Road. November 1, Mr. A. H. Gilkes, "The First Napoleon;" 8th, Mr. W. L. Carpenter, "Heat" (experiments by means of the projection lantern); 15th, Dr. H. W. Crosskey, "Early Changes in the Earth's Surface and how we get our Knowledge of them;" 22nd, Sir John Lubbock, M.P., "The Habits and Ideas of Savages;" 29th, Mr. W. F. Donkin, "Mountain Climbing in Switzerland and the Caucasus;" December 6, Prof. Boyd Dawkins, F.R.S., "A Bit of Coal;" 13th, Dr. W. D. Halliburton, "The Eye, and how we See."

The additions to the Zoological Society's Gardens during the past week include two Nisnas Monkeys (*Cercopithecus pyrrhonotus*) from West Africa, presented by Mrs. Benett Stanford; a Brown Capuchin (*Cebus fatuellus*) from Guiana, presented by Mr. Edward A. B. Pitman; a Dusky Ichneumon (*Herpestes pulverulentus*) from South Africa, presented by Mr. L. G. Morrell; a Three-striped Paradoxure (*Paradoxurus trivirgatus*) from India, presented by Mr. J. Millar; a Buzzard (*Buteo vulgaris*), British, presented by Mr. F. Austen; a Pennant's Broadtail (*Platycercus pennanti*) from New South Wales, presented by Mrs. Brooks; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. Absell; two Burrowing Owls (*Speotyto cunicularia*) from South America, presented by Mr. John Clarke Hawkshawe, F.Z.S.

OUR ASTRONOMICAL COLUMN.

The Parallax of Σ 2398.—In the *Astronomische Nachrichten*, No. 2676, Dr. E. Lamp, of Kiel, has published a determination of the parallax of the brighter component of this pair (to which his attention was attracted by their large proper motion) referred to two neighbouring stars derived from a series of differences of declination observed with the refractor and filar micrometer of the Kiel Observatory between February 1883 and April 1885; the value of the parallax deduced from this discussion being 0″·34. Wishing to verify this result, Dr. Lamp has made with the same instrument, between May 1885 and March 1887, a further series of measures of differences of declination of each component of Σ 2398 (= D.M. + 59°, No. 1915, mags. 8·2 and 9·0 respectively) referred to three comparison-stars: viz. D.M. + 59°, No. 1911, mag. 7·0; D.M. + 59°, No. 1913, mag. 9·4; and D.M. + 59°, No. 1918, mag. 7·8; and has published the results of his discussion of these measures in Nos. 2807–8 of the above-mentioned periodical. The following tabular statement gives for each series of observations the resulting parallax deduced from the differences of declination between each component of the double star and each of the above-mentioned comparison-stars:—

Period.	Component.	π_1	π_2	π_3	No. of Measures.
		″	″		
1883–84 ...	Σ_1	... —	... +0·2958	... +0·3801	46
1884–85 ...	Σ_1	... —	... +0·2517	... +0·4628	44
1885–87 ...	Σ_1	... +0·3601	... +0·2656	... +0·4303	73
1885–87 ...	Σ_2	... +0·3808	... +0·2636	... +0·4199	73

It would appear from this that Comparison-star No. 2 has a sensible parallax relative to Comparison-stars Nos. 1 and 3, and we hope that Dr. Lamp will proceed to investigate it independently. Meanwhile, combining the results obtained from the three stars, the mean parallax is—

$$\text{for } \Sigma_1 = 0''\!\cdot\!3520 \pm 0''\!\cdot\!0140$$
$$\text{and for } \Sigma_2 = 0''\!\cdot\!3548 \pm 0''\!\cdot\!0131,$$

or the mean parallax of the system Σ 2398 = 0″·353 ± 0″·014.

New Minor Planet.—A new minor planet, No. 271, was discovered on October 13 by Dr. Knorre, of Berlin.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 OCTOBER 30—NOVEMBER 5.

(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 30

Sun rises, 6h. 52m.; souths, 11h. 43m. 46·2s.; sets, 16h. 36m.: right asc. on meridian, 14h. 17·7m.; decl. 13° 47′ S. Sidereal Time at Sunset, 19h. 11m.

Moon (Full on October 31, 22h.) rises, 16h. 38m.; souths, 23h. 12m.; sets, 5h. 58m.*: right asc. on meridian, 1h. 47·3m.; decl. 6° 0′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury	9 21	13 16	17 11	15 49·9	23 5 S.
Venus	3 2	9 8	15 14	11 41·4	0 23 N.
Mars	1 21	8 13	15 5	10 46·2	9 29 N.
Jupiter	7 31	12 14	16 57	14 48·3	15 17 S.
Saturn	22 15*	6 2	13 49	8 34·7	19 2 N.
Uranus	4 42	10 20	15 58	12 53·9	5 5 S.
Neptune	17 36*	1 18	9 0	3 49·8	18 18 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. ° °
30	33 Ceti	6	0 58	1 44	193 275
30	35 Ceti	6½	1 54	3 3	149 325
Nov.					
1	μ Ceti	4	2 34	3 38	109 355
3	B.A.C. 1351	6½	4 19	4 30	217 235
3	75 Tauri	6	6 41	7 21	89 5

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Nov. 2,	3 10	*m*
o Ceti	2 13·6	3 29 S.	,, 5,		*M*
T Arietis	2 42·0	17 2 N.	,, 4,		*m*
η Geminorum	6 8·1	22 32 N.	Oct. 30,		*M*
ζ Geminorum	6 57·4	20 44 N.	,, 30,	4 0	*m*
			Nov. 3,	4 0	*M*
U Hydræ	10 32·0	12 48 S.	,, 3,		*m*
S Coronæ	15 16·8	31 47 N.	,, 3,		*m*
V Ophiuchi	16 20·5	12 10 S.	Oct. 31,		*M*
W Herculis	16 31·2	37 34 N.	Nov. 4,		*m*
U Ophiuchi	17 10·8	1 20 N.	,, 2,	2 23	*m*
			and at intervals of	20 8	
β Lyræ	18 45·9	33 14 N.	Nov. 2,	22 0	m_2
η Aquilæ	19 46·7	0 43 N.	,, 1,	0 0	*m*
W Cygni	21 31·8	44 52 N.	,, 1,		*m*
δ Cephei	22 25·0	57 50 N.	,, 3,	3 0	*m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

GEOGRAPHICAL NOTES.

Lieut. Wissmann, the well-known African explorer, has arrived at Brussels from his journey across Africa. He was accompanied as far as Nyangwé by Lieut. Le Marinel, the route followed being different from that traversed by Wissmann on his first journey. He did not, however, succeed in penetrating the region to the north of the Sankuru, nor in reaching the somewhat mysterious Lake Lundi.

IN the *Bulletin* of the Hungarian Geographical Society, vol. xv. fasc. 7, will be found an interesting paper (abstracted in French) on the struggle for existence among plants in the Hungarian Pusztas (steppes).

To the *Bulletin* of the Belgian Geographical Society, No. 4, 1887, M. Louis Navez contributes an instructive paper on the influence of the various geological formations in Belgium, especially upon the people. M. Navez points out that Belgium is specially favourable for such an investigation. In France and Germany the great differences of altitude and latitude, and the diversity of climate, determine phenomena often difficult to distinguish from those which are really due to geology, and are therefore causes of error. In Belgium the differences of altitude, of latitude, and of climate are not of so much importance. The influence of the character of the soil is almost always preponderant, and is easily distinguished. One instance may be given. The great quantity of lime which the Cretaceous soil of the Geer valley contains gives to the straw of the cereals a special suppleness, strength, and whiteness. From this straw are manufactured plaits which have a large sale, and in Paris are ranked next to the straw of Italy for ladies' bonnets. This manufacture is worth from four to five million francs yearly. On the other hand, the absence of calcareous salts in the ground traversed by the feeders of the Lys, render that river eminently suited for the cleansing of flax; hence the fame of the cloths of Flanders. M. Navez believes that the facts he brings together prove that the geological construction of the ground is one of the factors that limit the free will of man and have an active influence on communities.

IN Dr. Oscar Baumann's paper on Fernando Po, in No. ix. of *Petermann's Mitteilungen*, the author states that the volcanic group of which the island is a member, forms a line running south-west from the Cameroons, and may be regarded as the result of an eruptive fissure, which on the one side extends from the Cameroons to the island of Annobon, and on the other appears to find in the Rumbi Mountains a continuation into the heart of Africa. The northern half of the island is covered almost entirely by the huge volcanic peak of O-Wassa (Clarence Peak). After careful estimation he gives the summit a height of 9350 feet. The volcano may be regarded as extinct, the fire and clouds of smoke seen at times on the summit being easily explained by the annual burning of the grass. The crater on the top of the mountain is 515 feet in depth, and is inclosed by gray disintegrated walls of basalt. On the thickly wooded and almost impassable slopes are many subsidiary craters. The basaltic rock of the east coast is being gradually crumbled away by the sea, while along the west coast the land is gaining on the sea. The physical geography of the southern half of the island is determined by the mountain range of the "Cordillera of Fernando Po," which in two chains connected by a pass runs practically east and west. These chains culminate in several summits, which have a volcanic character only on account of their basaltic composition. In the south of the island, and apparently quite independent of the Cordillera, there rises a lofty volcanic mass. On the top of one cone-shaped peak, precipitous on all sides, there extends a flat basin surrounded by a circle of hills. This the author supposes to be the remains of a large crater.

M. A. J. WAUTERS writes a long article in the new number of the *Mouvement Géographique* to prove that Lake Muta-Nzigé, the somewhat problematical lake to the south of Albert Nyanza, belongs to the Congo system, and not to that of the Nile. The altitudes and other data which M. Wauters has on which to base his conjecture are of the scantiest, and extremely doubtful. It seems to us that such conjectural arm-chair geography is a useless waste of time and space. The question of the relation between the Muta-Nzigé and the Albert Nyanza can only be solved by actual exploration. By this time no doubt it has been solved either by Emin Pasha or Stanley, and M. Wauters might therefore put the valuable space of his small journal to a much more profitable use.

METEOROLOGICAL NOTES.

THE Hydrographer to the Admiralty has issued a circular stating that the United States Government has given notice that from September 1, 1887, the following storm signals (consisting of day signals of two kinds, also night signals), would be shown on the shores of the Atlantic Ocean, Gulf of Mexico, and Great Lakes, as storm conditions may demand, taking into consideration the fact that westerly winds of high velocity with clearing weather are less dangerous than those from easterly quarters with freezing weather: (1) cautionary signal, a *yellow flag with white centre*, will indicate the winds expected are not so severe but that well-found and seaworthy vessels can meet them without great danger; (2) storm signal (now in use), a *red flag with black centre*, will indicate that the storm is expected to be of more marked violence. In order to afford as exact information as possible regarding the relative positions of the storm and the winds expected, two pendants will be displayed: a *red pendant* will indicate easterly winds, from north-east to south inclusive, and that the storm centre is approaching; a *white pendant* will indicate westerly winds, from north to south-west inclusive, and that the storm centre has passed. Whilst it is intended that the pendant shall indicate positively only whether the winds will be easterly or westerly, yet in order to give still more definite information, the *red* or *easterly pendant* will be hoisted above the cautionary or storm signal, for winds from the north-east quadrant, and below for winds from the south-east quadrant. Also, the *white* or *westerly pendant* will be hoisted above the cautionary or storm signal for winds from the north-west quadrant, and below for winds from the south-west quadrant. Because of the difficulty of varying night signals, they will not distinctly show the force, but indicate the direction of the wind only; a *red light* for easterly winds, and a *red and white light* for westerly winds.

THE Pilot Chart of the North Atlantic Ocean, issued by the Hydrographic Office at Washington for the month of September, includes a valuable article on the law of storms, considered with special reference to the approaching season of West Indian hurricanes. The article is accompanied by two diagrams illustrating (1) the circulation of the wind in a tropical cyclone in the northern hemisphere, showing clearly how the wind is drawn in towards the centre of low barometer, and its direction at any point; and (2) a diagram for practical use in finding a ship's position relatively to the centre of the hurricane, by means of the direction of the wind and fall of the barometer. The circles in this diagram are normal isobars, and represent the rate at which the barometer falls as the centre of the storm is approached, so far as our latest knowledge of cyclones can be safely used as a general guide. Full directions are given for the practical use of these diagrams. The Pilot Charts contain frequent extracts from ships' logs, as to the great value of the use of oil in heavy seas, and the Hydrographic Office considers that the testimony is so conclusive that its use is now recognized by every commercial nation. The Charts contain information about winds, currents, fog, and the position of dangerous derelicts, and their speedy dissemination must prove to be of great utility to the maritime community generally.

THE *Annalen der Hydrographie und maritimen Meteorologie* for August contains a graphical representation of the distribution of rainfall in the Atlantic and Indian Oceans, compiled by Dr. W. Köppen from materials at the disposal of the Deutsche Seewarte and all other available sources. The latitude is shown in the vertical, and the months in the horizontal direction; each point of the area of the diagram therefore indicates a definite time of the year, and a definite distance from the equator, and the curves show the equal percentages of days of rainfall. The diagrams are explained in the text, and show the relatively large rainfall frequency in the higher latitudes of the North Atlantic in all seasons, but especially during winter; further southwards, towards the region of the trade-winds, all months become drier, but north of the tropics rain falls on an average once in every 5–10 days. In the South Atlantic the conditions are more complicated. From 0°–5° S. autumn rains are prevalent, and from 5°–14° S. the winter rainfall maximum is prominent. In the Indian Ocean the equatorial rain-belt is considerably widened, especially on the southern side. From 2° N. to 12° S. the rainfall frequency does not fall below 50 per cent. in any month, and from June to October it exceeds 70 per cent. Between 10° and 12° S., and also from 25°–30° S. the spring-time is driest; while from 33° S. the rainfall increases rapidly in winter and spring. South of 40° S. it rains eight days out of ten during July and August.

Symons's Monthly Meteorological Magazine for September contains interesting articles on the deficiency of rainfall this year and on the definition of drought. From a comparison of the rainfall at 27 stations from January 1 to August 31, 1870–79, and for the same period in 1887, and expressing the difference of 1887 from the average as a percentage, it appears that the mean percentage this year has been: for England 59·8, for Wales 60·0, for Scotland 68·5, and for Ireland 60·3. Out of 11 stations in England the drought is unprecedented at 6, in Scotland at 6 stations out of 8, and in Ireland at 5 stations out of 6. Scotland has suffered least, and in parts of England the deficiency has reached the extreme limit that may be expected. There appears to be no good definition of a drought. In 1880 Mr. Symons adopted a classification for his numerous observers, which has generally been used up to the present time, viz. (1) absolute drought, being a period of 14 or more consecutive days without rain; (2) partial drought, being a period of 28 or more consecutive days in which the total rainfall did not exceed 0·25 inch. But engineers speak of droughts varying from 140 even to 240 days; these can bear no comparison with the above definitions, and Mr. Symons suggests a third term, viz. long drought, being a period of not less than 60 days with a total rainfall of not less than 2 inches. Opinions are wanted as to how the records of rainfall observers may be best utilized in the form most useful to engineers.

MR. J. W. OLIVER has contributed an article to *Longman's Magazine* for October on the moon and the weather, in which he discusses some of the most important of the popular predictions in which the moon is concerned. He deals (1) with the lunar notions that are utterly absurd, and (2) with those that are explicable by the aid of physical principles. The conclusion at which he arrives is that there is more nonsense than sense in lunar predictions, but that it is unfair to consider the whole subject as unworthy of serious treatment. For instance, atmospheric tides due to the moon's attraction must exist, although generally obliterated by disturbances due to other causes. The author apparently favours Sir John Herschel's statement of the tendency of the full moon to clear the sky, and in support of that theory he quotes the experiments of Melloni and others, showing that moonlight contains a minute proportion of dark heat rays, the effect of which may in a certain measure cause the dispersion of the clouds. The lunar halo is also referred to as an old sign of bad weather. Of 61 halos observed near London 34 were followed by rain within 24 hours and 19 within four days. In the *Mittheilungen aus dem Gebiete des Seewesens* for June, published by the Hydrographic Office of Pola, Capt. C. von Bermann has an article upon the same subject. He deals chiefly with Herr Falb's attempt to reinstate the moon's influence in his work "Das Wetter und der Mond." The result arrived at is that, although the moon has an influence on the weather, it is too infinitesimal, compared with other influences, to be appreciable.

THE Annual Report of the Meteorological Observer for Tasmania for the year 1886 gives the results of observations taken at 11 stations and rainfall reports from 37 stations.

THE SIXTH INTERNATIONAL CONGRESS OF HYGIENE AND DEMOGRAPHY IN VIENNA.

(FROM OUR OWN CORRESPONDENT).

THE most important questions dealt with at the Vienna Congress were those relating to preventive medicine, a branch of medical science which originated with Edward Jenner's discovery of immunity from small-pox by means of vaccination. The high value of vaccination and re-vaccination was clearly shown in the Demographic Section of the Congress by statistical tables exhibited by T. Körösi, the Director of the Statistical Office in Buda-Pesth. According to these tables the mortality of the not-vaccinated patients treated in nineteen Hungarian small-pox hospitals was 800 per cent. larger than that of the vaccinated patients, while the receptivity for getting the disease was three and a half times larger in the not-vaccinated as compared with the vaccinated people. In the Fourth Section the question of vaccination was also submitted for discussion by a lecture delivered by a Turkish delegate, Dr. Violi, and a resolution recommending to all Governments the introduction of compulsory vaccination was unanimously adopted.

Thanks to the valuable discoveries of Pasteur, the method of protecting the life and health of men as well as animals by vaccination has been worked out more extensively, and is now applied with the best success against various desolating and destructive diseases. The beneficial effects of the various methods of preventive inoculations, the amount of saving of human and animal life brought about by their use, will easily be perceived by a sketch of the discussions which were held at the last meetings of the Third Section of the Congress.

A special sub-section was formed where the special results obtained by the preventive inoculations in splenic fever, and erysipelas of the pig, were reported and discussed upon. Dr. Lydtin, of Carlsruhe, gave an interesting account of the development of the measures against the different plagues, alluding also to the defects of repressive measures, by which cattle-trade and cattle-breeding are so severely affected. While two centuries ago the first trials made of providing immunity against sheep-pox and other plagues were based on purely empirical views, the recent bacteriological discoveries have led to the scientific method of vaccination with an attenuated virus. Dr. Chamberland, Pasteur's assistant, reported on the results obtained by the preventive inoculations against (charbon) splenic fever in France, Hungary, Italy, and Russia. In the year 1886, 367,208 of sheep and 47,229 of cattle had been subjected to anti-charbon inoculations. Chamberland's statements were considerably supported by the results obtained with Pasteur's methods in Switzerland and at Pakisch, Prussia, which were communicated by Dr. Custer of Zurich and Dr. Lydtin (Carlsruhe). On the other side, the practical value of the preventive inoculations in splenic fever was severely contested by Dr. Löffler, of the Berlin School. An animated discussion on this subject went on for three hours and a half.

By the majority of the speakers it was pointed out that no loss by death—or only a small percentage of such loss—was caused by the inoculation itself in cattle, and that the number of cases of natural splenic fever decreased very much after the preventive inoculations; but in sheep the vaccinations against charbon were not equally favourable. As to the duration of the immunity acquired by the vaccination no positive statements could be made.

Then the usefulness of preventive inoculations against charbon symptomatique (Rauschbrand) was dealt with, and was acknowledged by all the speakers. In the course of the discussion on the preventive inoculations against erysipelas in the pig—a discussion in which Drs. Lydtin, Chamberland, and Prof. Czokor of Vienna, took part—it was stated that by the inoculations the animals get immunity against the disease, but that further improvements of the method are wanted, as there is now a considerable loss of inoculated animals by the vaccination itself, and sometimes the spreading of disease in healthy animals. In conclusion it was resolved on a vote that the experiments on preventive inoculations against splenic fever, charbon symptomatique, "erysipelas of the pig," and the other epizootics ought to be continued under the control of and assisted by Governments.

The battle-ground of the Congress from a scientific point of view was the discussion on the preventive inoculations against rabies. Dr. Chamberland gave a *résumé* of the development of Pasteur's method, and the results obtained by this treatment in France. 2682 persons were treated and the average mortality was only 7·5 per cent.; while the mortality of persons bitten by rabid animals if not inoculated varied between 5 and 30 per cent., according to the statements made by different authors. In answering the objections made by some recent writers who had expressed the opinion that the death of patients from hydrophobia was caused by the inoculations themselves, he maintained that it was proved by experiments made with the spinal cord of those persons that the death was caused by the bite of the rabid animal, not by the inoculations. If animals were inoculated with brain emulsions of those persons they became rabid, but they died on the fourteenth or fifteenth day after inoculation, while they would have succumbed on the seventh day if the death of the persons had been caused by the inoculated virus.

Dr. Bordoni-Uffreduzzi of Turin, in making experiments on dogs and animals, found that various parts of the brain of rabid animals show different degrees of virulence. The pancreatic gland is nearly as virulent as the brain, while the liver and spleen seem to act much less strongly as media of the virus. He had also treated 119 persons bitten by rabid dogs; three of them died.

Dr. Emmerich Ullmann, assistant of Prof. Albert of Vienna, has performed preventive inoculations in 122 persons. Only persons who could prove by documents the rabid condition of the biting animal were submitted to the treatment, and only the weak inoculations were used. The rabid condition of the biting animal was in all the cases proved either by experiments with the brain of the animals or by *post-mortem* examinations. Out of the 122 patients treated, whose ages varied between fourteen months and sixty-one years, three died; the rate of mortality being 2·4 per cent. In fourteen cases the wounds were on the head or in the face. To this series belonged two deaths; while in seventy-two cases the upper extremity was the seat of the wound, in which group the third death occurred. As Pasteur had laid great stress on the previous cauterization of the wounds, Dr. Ullmann was carrying out some experiments to examine the influence of cauterization. Animals were infected subcutaneously with the virus of rabies, and then treated with cauterizations by lapis infernalis or fuming nitric acid. All these animals died from rabies, even when the cauterization was applied immediately after the infection, as could be shown by experimental inoculation with their brains. Only two animals cauterized on the spot with the Paqueline (ferrum candens) remained healthy. The eminent value of the preventive inoculations could easily be ascertained by some of the cases treated by Dr. Ullmann. In *Rzeszow*, a Polish village, five persons were bitten by a rabid dog. Three of them were brought to Vienna, where they were treated by Pasteur's inoculations; they remained healthy, while the two others remaining at home not inoculated died from hydrophobia after two weeks. Also in some other cases, persons bitten by rabid dogs but not submitted to Pasteur's treatment died, while the inoculated persons bitten by the same animal were not attacked by the disease. These experiments seem to be crucial experiments, proving clearly the usefulness of Pasteur's method.

It may be also stated that Dr. Ullmann had to defray the expenses of his experiments and of the inoculations from his own pocket, as the Austrian Government refused to grant any subvention for carrying out Pasteur's experiments, "on account of the frequent mishaps"! It is impossible to say from what source the Government obtained knowledge of these mishaps, as it did not try to get any information on the matter either in Paris or in Vienna. On the other side, the Hungarian Government has acknowledged the high merits of M. Pasteur by conferring on him the Order of the Iron Crown.

Dr. von Frisch criticised the statistical data given by Pasteur and Chamberland. He argued that there should be a decrease of cases of rabies if the method were successful; but the number of cases has increased. The fixed virus is an inconstant power, the period of incubation after its inoculation being a various one, not restricted to seven or eight days. Inoculation after previous infection does not provide certain immunity. The compulsory inoculation of dogs should be introduced to prevent rabies in man.

Prof. Metschnikoff, the celebrated Russian biologist, reported on the results obtained at the Bacteriological Station of Odessa. 713 persons, bitten by rabid animals, had been treated by Dr. Gamaleia. At first the results obtained were unfavourable, too weak emulsions having been used for the inoculations, as was proved by later experiments. Since July 1886, 532 persons were treated by the intensive method. In 137 out of these 532 cases, emulsions weaker than of two-days' incubations were injected; 9 persons died, the rate of mortality being 6·5 per cent. Of 88 persons inoculated once with a two-days' virus, 2 died (*i.e.* 2·3 per cent.); while among the last 307 cases, treated at least twice with a two-days' emulsion, only 2 deaths (0·6 per cent.) occurred. The average mortality in all these 532 cases was 2·4 per cent. The value of the intensive treatment could easily be understood in the case of persons bitten by rabid wolves. Of 36 persons bitten by rabid wolves, 6 were subjected to the weak treatment, and 2 of them died; the other 30, inoculated with two-days' virus, remaining healthy. In some cases even the one-day's virus was used with the best results; *e.g.* among 5 persons bitten by rabid wolves, inoculated six times with one-day's virus, the outbreak of rabies had been prevented. Experiments on 1500 animals were carried out by Dr. Bardach, who was able to prove the correctness of Pasteur's statements regarding the seven-days' period of incubation of his fixed virus. In some cases, however, the period of incubation was found to be prolonged; but the prolongation was caused by some other parasitic diseases of the animals, which will be described by Dr. Bardach in a forthcoming paper. In concluding his report, Prof. Metschnikoff stated that "the results obtained at the Odessa Bacteriological Station are very strongly in favour of Pasteur's discoveries, which decidedly must be regarded as epoch-making."

Dr. De Renzi (Naples), described his experiments on rabbits, in which, by the injecting of brain-emulsion into the blood-vessels, rabies had been produced. But neither by this method nor by subcutaneous injections of the fixed virus was he in every case able to produce rabies in rabbits, which sometimes showed immunity against this infection. He expressed also the opinion that no gradual difference of the attenuation of the virus seems to exist.

In summing up the different views of the speakers Dr. Chamberland was able to state that the high value of Pasteur's discoveries was acknowledged by everybody.

THE MINERAL WEALTH OF THE UNITED STATES.

ON Saturday, October 8, a lecture on this subject was delivered at the American Exhibition by Dr. A. E. Foote, of Philadelphia, who, as Acting Commissioner from Pennsylvania, has exhibited a fine collection illustrating the mineral resources of that great State. The Chairman, Mr. F. W. Rudler, President of the Geologists' Association, congratulated the audience upon being able to listen to one who, from his very extensive and personal observation, was well qualified to treat such a subject.

Dr. Foote pointed out that the geological formation of a country was the basis of its mineral wealth, and illustrated this by the remarkably fine large geological map of the United States compiled by Prof. Hitchcock from the work done by the United States and State Geological Surveys. His statistics were mostly based upon Williams's "Mineral Resources," of 1885.

As a Pennsylvanian he was happy to say that of 95,000,000 of tons of coal mined in that year nearly two-thirds the tonnage, and fully two-thirds the value, was produced in Pennsylvania. Of this amount 34,000,000 of tons was anthracite. The Girard Trust, that noblest of America's educational charities, exhibits a mass of anthracite from their mines in Schuylkill County, Pennsylvania, that measures 22½ cubic feet, and weighs 2256 pounds. Coal is now found in workable quantities in thirty States and Territories. Of iron there was less in value mined in 1884 than the value of the petroleum—£4,000,000 sterling—but with the steady revival of business the production has rapidly increased, until this year it may reach a total of £6,000,000 sterling. The iron area is being developed almost as rapidly as the coal. The greatest activity has been manifested in Alabama, near Sheffield and Birmingham. The latter city is the most wonderful example of rapid, solid business development that America has ever seen. Perhaps nowhere else in the world can such a favourable combination of coal, iron, and limestone be found.

But for the remarkable development of natural gas near Pittsburg, far more of Pennsylvania's capital would have been transferred to this favoured locality. Natural gas was first used for lighting the town of Fredonia, New York, in 1825, and this well, still in operation, was pronounced by Humboldt an eighth wonder of the world. Here, however, it was used simply for lighting, but it is far better suited for heating, owing to the absence of the heavy hydro-carbons. It is principally marsh-gas, ethane, hydrogen, and nitrogen. In October of 1875 it was first used in the smelting of iron at the mills of Spang, Chalfant, and Co., at Etna, near Pittsburg. The gas was brought 17 miles from the "Harvey," the leading well of the region. When turned into the 6-inch iron pipe the pressure was so great that it travelled the entire distance in twenty-two minutes. Mr. Foote visited the works in December 1875, and it was already in full and successful operation, turning out the purest iron, equal to the best Swedish. Its success was so striking that it was almost instantly introduced into all the iron, glass, and other manufactories of this great centre of America's industry. It is also used in all the heating and lighting of the city. The cost is about one-quarter that of coal, and it is estimated that it will this year take the place of coal to the value of £6,000,000 sterling. Not the least of its advantages is its freedom from smoke, so that what was once an unbearably dirty city now has air as pure as that of a country village. The enormous waste that has been going on is illustrated by the history of the "Haymaker" Well, No. 1, which in five years wasted £400,000 worth of gas, or nearly £200 daily. There were hundreds of such wells.

Gold and silver were touched upon very briefly, as others who were present were expected to speak upon them. The production of gold was £6,500,000. Of this the larger portion was from quartz-mining, the placer production having very much fallen off owing to the unfavourable legislation in California. The speaker referred to the tellurides as true ores of gold, and predicted that while at present they were found in large quantities only in Colorado, they would be found in much greater quantities north and south. The production both of gold and silver would undoubtedly be increased as transportation facilities were increased in localities now nearly inaccessible. The placer mines of North Carolina and Georgia had, he believed, a great future before them. A successful process was required for working the enormous dry deposits of Arizona and New Mexico without water. The silver production is over £10,000,000 sterling. The rich deposits of Leadville (Colorado), the Comstock mines in Nevada, the Silver King and other localities in Arizona, Silver City and Lake Valley (New Mexico), were referred to. At the latter a chamber was found not much larger than a common room from which £100,000 worth of ore was taken. It was chloride, so rich that it would fall in drops of nearly pure silver with the heat of a lighted match.

The American copper mines are the richest in the world; the production being over £4,000,000 sterling, of which the "Calumet and Hecla" claims one-fifth. They have over seventeen years' supply in sight, and practically the supply is inexhaustible. The "Anaconda" of Montana, and neighbouring mines, also produce about one-fifth. These localities are so rich, so easily worked, and above all so accessible, that the extraordinarily rich mines of Arizona, where transportation is much more costly, have suffered severely by the competition. The mines near Clifton (Arizona) produce a charming combination of malachite and azurite, which is one of the most beautiful ornamental stones ever seen. At the Copper Queen mine malachites fully equal to those of Russia, and azurites as finely crystallized as those of Chessy, France, are found.

The lead production is of the value of £2,100,000 sterling, most of which is a by-product of the Rocky Mountain silver mines. This production is so great that it has rendered mining for lead alone unprofitable at the wonderfully rich South-West Missouri region. A very interesting fact is that the vanadates and molybdates of lead replace the phosphates in all the western region. As vanadium salts are rapidly increasing in importance in the manufacture of the aniline dyes, this will be a future source of wealth, for there are mines here that can produce more vanadium than all the rest known in the world. The molybdates from Arizona and New Mexico are the finest ever seen.

The production of zinc was £1,000,000 sterling, three-quarters of which is found in South-West Missouri and Kansas. Four railroads have been constructed within a few years to carry away the rapidly-increasing production. The zinc mines of Franklin, New Jersey, have been known for over a hundred years and are remarkable not only for their richness but for the extraordinary number of rare and beautiful species that are found there and nowhere else in the world. The localities of mercury, nickel, manganese, tin, chromium, platinum, and other metals were also spoken of.

Among non-metallic minerals the phosphates of South Carolina were the principal, over £750,000 sterling being produced annually for fertilizers. Vast deposits of gypsum or plaster-stone are found in Michigan, Ohio, New York, and other localities. Mica is principally mined in New Hampshire and North Carolina. This is the mineral popularly but erroneously known in England as talc. Talc is a very different material, also found in North Carolina, but used for the tips of gas-burners and as a lubricator. The principal use of mica is for the windows of coal-stoves. Many very interesting minerals occur in connexion with the mica, such as beryls of enormous size, emeralds of great value, garnets, and so forth.

The time was so fully taken up with the ores and economic minerals that the gems and ornamental stones were treated of in a lecture delivered on October 22.

THE EXPLORATION OF NEW GUINEA.

On August 11 (p. 351) we reprinted from the *Sydney Morning Herald* an account of an exploring expedition in New Guinea, conducted by Mr. Theodore Bevan in the steamer *Victory*. Another Australian paper, the *Daily Telegraph* of July 9, gives, with the map which we reproduce, a more detailed narrative, compiled from Mr. Bevan's notes. From this narrative we take the following passages:—

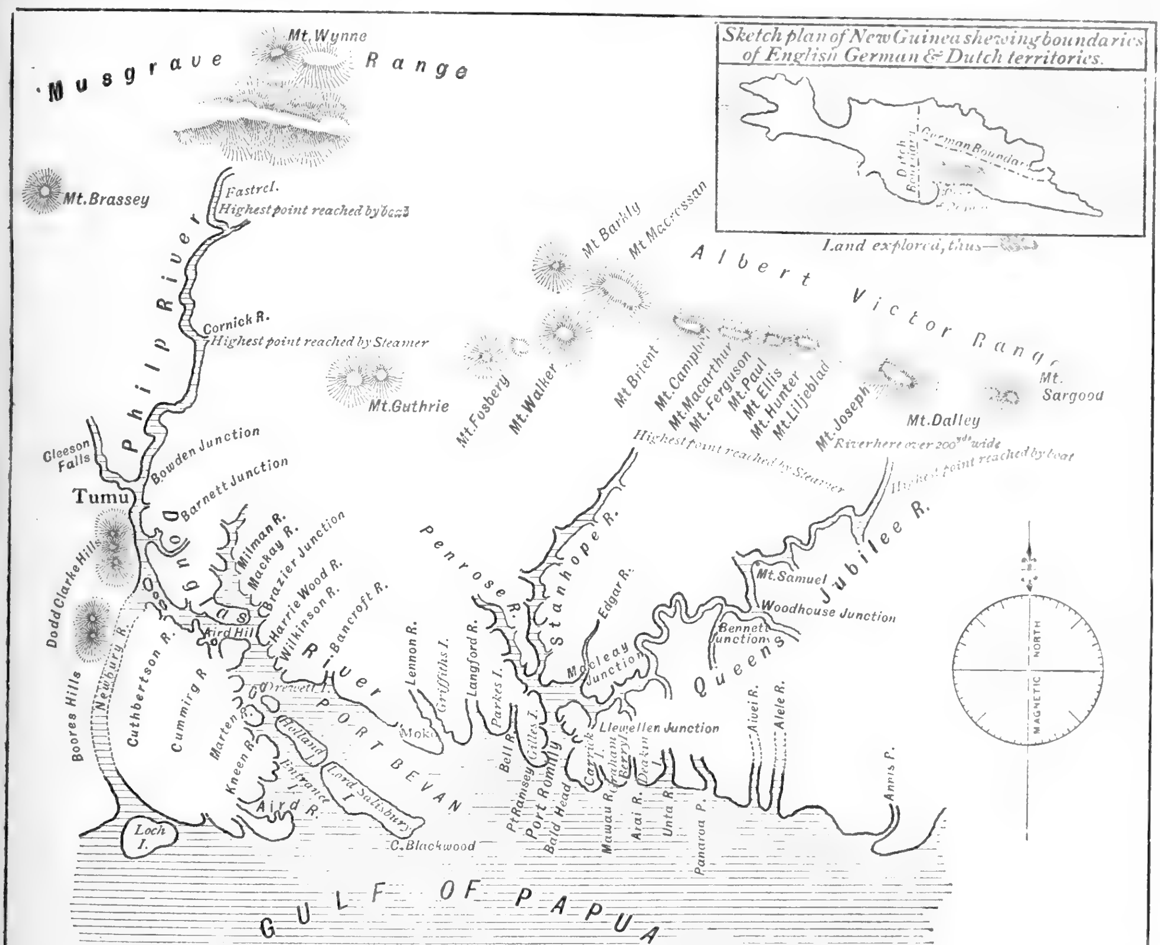
The *Victory* left Thursday Island at 5.30 a.m. of Thursday, March 17, and was headed for Cape Blackwood at the mouth of the Aird River, New Guinea, and distant 220 miles. On the following Saturday, early in the morning, the distant mountains of Papua were sighted, and at 6 o'clock Cape Blackwood was seen. Shaping a course after passing inside the cape to the north of Entrance Island and steering past it, the party found that the River Aird narrowed to about 200 yards, and after steaming from Entrance Island a distance of 5 miles, about 4 p.m. they came to a broad but seemingly shallow stream running into Deception Bay. (Deception Bay is the open space shown on the map between Cape Blackwood and Bald Head.) . . . It was found that the country between Deception Bay and the Aird River was made up of islands instead of being mainland as charted by Blackwood. Further, from a great number of water-ways and river openings on every side, it became evident that this was nothing less than the delta of a large fresh-water river, whose source was in the mountains seen from the coast. Steaming up this river, leaving alluvial scrub-covered islands to the left, and passing broad streams eachover half a mile wide on the right, the party found that deep water was carried to an important junction (named after Mr. John Brazier, of the Australian Museum) right under and to the south-east of Aird Hills. Here the river threw off two branches, one skirting the hills to the south and the other bearing north by east. Brazier Junction was found to be distant, as the crow flies, 30 miles from Cape Blackwood. . . . A portion of the party proceeded in the whale-boat for a distance in a north-westerly direction of about 6 miles, when to their surprise a wide branch of the river was opened up north by west, two important openings towards the south-west being also seen and named after Mr. Cuthbertson and Mr. Cosmo Newbery. It was becoming more and more evident that the main stream of a delta in a large fresh-water river had been reached, the southerly-running channels being divisions, and the Cuthbertson and Newbery Rivers probably discharging their vast volumes of water into Prince George's Inlet several miles westward of Cape Blackwood. This main stream was explored for some 5 miles, the depth being from 2 to 7 fathoms. The country around was of alluvial formation, and scarcely above the level of the river, but thickly covered with virgin forest, the trees reaching a height of 150 feet, and crusted with mosses, fungi, creepers, and orchids in tropical luxuriance. A magnificent view was obtained at one part of tier upon tier of ranges of hills to the northward, and behind these blue mountain peaks of from 7000 to 8000 feet in altitude, and from 40 to 50 miles distant. The river itself in the various branches looked like an immense lake studded with islands. This main channel the steamer proceeded up on the day following, and a second range of low palm-clad hills observed in front on the previous day proved to be the head of the delta. This was named after Mr. J. V. S. Barnett, of Cooktown, Queensland. This spot, as the crow flies, was 45 miles from Cape Blackwood, and only now could it be properly stated that *terra firma* had been reached, for south of them the soil was alluvial, being brought down from the mountains by this great stream, the volcanic cones of Aird Hills, rising to a height of 1620 feet to the south-east, being the solitary exception in these miles of dead-level, scrub-covered, deltaic alluvial flats. North of Barnett's Junction the river flowed between compact banks, through gradually rising country, and with an exclusive fresh-water current.

On March 25, after proceeding up the river a few miles, a third series of palm-topped conical hills were seen, and on the summit were two of their native houses, being about 200 feet in length. It was soon evident that the strange apparition of the steamer gliding into these fastnesses was visible from the shore, as the mellow sound of the conch shell was heard, warning the inhabitants of the scattered village of danger. Slowly the steamer approached, and when abreast of the village and opposite a creek some canoes full of savages were seen scuttling up the place in abject terror. The river now gradually widened out, and two large tributaries, running one from the north-west and the other from the north-east, were seen. This junction was observed to be in latitude 7° 11', and 144° east longitude. . . . The junction of these rivers was named after Mr. V. R. Bowden, of Thursday Island, the north-west and north-east branches being named after Messrs. Burns and Robert Philp, while the great river running from Bowden Junction into Deception Bay, a distance of over 60 miles, was called after the Hon. John Douglas.

The steamer was taken cautiously up Burns River, the north-west branch, and a splendid view of near mountain ranges was obtained, and apparently a splendid spot to explore. . . . Seven miles up the river an anchorage was come to, the river being over 300 yards wide, and the soundings being from 2 to 6 fathoms. The scenery was picturesque in the extreme. Hills of from 600 to 1000 feet, clothed with verdure, came down to the water's edge. There were cedars, oaks, eucalypti, fig-trees, acacias, pines, palms, and tree-ferns. Feathery bamboos, ferns and varied flora adorned the river banks. Butterflies of gaudy hue, and birds of the brightest plumage, flutter in and out amongst the trees and shrubs. The water was placid and in the deepest recesses of the gorge-like ranges was sombre and cold. . . .

A few miles further up the river a small rapid was passed, and it was then found that there was a break, one arm apparently running in a northerly direction towards ranges about 4 miles distant, and from 2000 to 3000 feet high, and the other arm stretching easterly towards high distant ranges, which closed in the horizon in that direction also, but they appeared to be about 25 miles off. This junction was named after the *Victory*, but the steamer could not pass it to any distance. The highest point reached by the steamer was ascertained to be in south latitude 6° 51′, and east longitude 144° 8′, that is to say 65 miles in a straight course from Cape Blackwood, or nearly 90 miles by river courses. This was on March 31.

A boat party was formed of eleven, and a week's provisions put in, but progress was slow, owing to the strong currents, the rapids, and the heavy rain that now came on in the evening. . . . The highest position inland reached by the whale-boat was within 25 miles of the German boundary, or 80 miles as the crow flies south-by-west of Cape Blackwood, and over 100



Walker & Boutall sc.

miles by river courses. This was in south latitude 6° 39′, and east longitude 144° 11′. It appeared to Mr. Bevan that the natural boundary or water-parting between the river systems of the two territories might be found to exist a few miles to the north of the present German boundary. . . . The nature of the coast for several miles to the westward was now known, and it remained [after return to Deception Bay] to continue explorations to the eastward, and also to settle the point as to whether from so far in the heart of Deception Bay there existed a deep water channel leading out into the Gulf of Papua. Mr. Bevan decided to test this question. Although there were the river openings between them and Bald Head, it was unlikely that there would be room for any considerable river between the newly-discovered Douglas River and the large river of which it had been for years reported that the five openings east of Bald Head were separate mouths. Mr. Bevan first proceeded to Motu Motu, situated at the mouth of the Williams River on the easternmost boundary of the gulf, and 100 miles distant, in order to send off despatches. A start was made from Motu Motu on Monday, April 11, and after calling at Karama, Silo, Namai, and Vailalla, they anchored off Orokolo, in rather dirty weather. On April 14 the steamer was within a few miles of the Alele River, or first of the five openings east of Bald Head, reported by the natives as leading into one large river. Upon superficially examining the entrances to these five rivers, namely, Alele, Aivei, Panaroa, Unta, and Arai, no safe channel for the steamer could be found, owing to the heavy break on each bar. Off the next opening, or Marwau River, the same conditions were experienced, and there now remained but the wide entrance marked on the Admiralty chart. Towards this opening the steamer was steered, and carefully proceeded in with a $2\frac{1}{2}$ fathoms channel at low water, leading half a mile to the west of Bald

Head, an anchorage being found 2 miles within in 15 feet of sheltered water. Point Ramsay (named by Mr. Bevan) was 3 miles to westward, over an unbroken stretch of water, which also ran far inland to the north. This was an important discovery, as no ship had before been within this opening, and the *Victory* had again passed the confines of the known. In a little bight under Bald Head a village was discovered partly hidden and sheltered by a grove of cocoa-nut trees. Canoes with natives came off, and though shy at first, they afterwards came near. . . . The anchorage was left at 7.45 a.m., and 2½ fathoms deep was taken into a channel 5 fathoms in depth midway between Bald Head and Point Ramsay. For over 8 miles, with a depth of from 5 to 9 fathoms, the vessel proceeded until an important junction was reached. Here land traversed the horizon, and broad arms coming in from north-west to north-east joined the river. This junction was named after the Hon. W. Macleay, of Sydney, and the sheet of water so far traversed from Bald Head was named Port Romilly.

Round the point, and at a distance of 4 miles, a second junction was met with, and named after J. Beveridge, one of the party. At this point the river was nearly half a mile wide, and an extensive mud-flat was met with. Some very fair agricultural land was now passed through, with light chocolate soil, and covered with scrub that could be cleared with ease. Freshwater springs were noticed flowing over the banks. Numerous small deserted huts were passed, and a number of alligators and flying foxes. The rule seemed to apply in these deltaic rivers that the land was making on the convex side, while the deepest channel and strongest current were found close to the concave bank. The country now passed through alluvial swampy land, in which nipa and sago palms flourished amidst a thick scrub. The river narrowed to 60 yards, and at low tide the water was quite fresh. It was found necessary to anchor here, and some of the party, getting into the whale-boat, rowed up the river, which continued to get narrow, until it broke up into several deep but very narrow creeks, and further navigation was closed. The highest point reached up this river, which was named after the Hon. Edward Stanhope, was 7° 14′ south latitude, and 144° 28′ east longitude, being 34 miles due north from Arai River on the coast, or 40 miles by river courses to Bald Head. Returning to Beveridge Junction, the *Victory* was taken up an arm coming in here from the west. Several openings into the arm were passed, but after proceeding 7 miles up it shallowed to 11 feet. The river was named the Penrose, after a gentleman of that name of Yulgilbar, in this colony. On this river a native plantation and some natives were seen. The steamer was taken to an anchorage at Macleay Junction. After exploring with boats the eastern channels of this junction, and meeting with natives who were of an extremely friendly disposition, the *Victory* was taken up the channel. The houses of the natives were raised on piles of the hog-backed shape, open in front and with protruding peaks. The village was called Piri Evorra. Continuing on its way, about midday the steamer, to the great satisfaction of all, ran at right angles into a fine new river running north, north-east, and south. This seemed to form certain proof that they were now in the one large river reported by the coast natives. This junction, which was 11 miles west by north of Macleay Junction, was named after Dr. Llewellyn Bevan, of Melbourne, a family connexion of the leader. Taking the north-east branch of the river, which is 300 yards wide, some fine-looking, well-timbered country was passed through. Several deserted dwellings were noticed. After passing several miles up this splendid river another junction was met with, where a broad stream over half a mile in length came in from the easterly direction, and bifurcated into the channel, and a wide stream flowing south-south-westerly, with a steady fresh-water current flowing seaward. This junction was named the Bennett Junction after a friend. Three miles further up, and still another junction was met with, named after Mr. William Woodhouse, of Sydney. This proved to be the head of the delta of the great fresh-water river up which they had come, and which was named the Queen's Jubilee River. At this point the river again bifurcated, throwing off one main branch half a mile wide running down to Bennett Junction, and the other flowing easterly and southerly. Past Woodhouse Junction the river maintained its width of fully half a mile, and a range of hills 2000 feet high, a few miles distant, was named after Sir Saul Samuel. Mountain peaks of great altitude were visible some 40 miles to the north. Still proceeding up the river, a rapid some miles further on was passed, and soon afterwards it was found necessary to stop, but for one day over 30 miles had been travelled. The highest position reached was south latitude 7° 18′, and east longitude 144° 59½′, and distant 45 miles from Bald Head, and over 100 miles by the remarkably tortuous courses. As the river had now become unsafe, and only two days' coal were left for river work, it was found necessary to return to Bennett's Junction, from which it was hoped that the broad channel leading to the sea would be taken; but the master of the *Victory* demurred to this step, owing to the strong current running. The same objection had to be taken to the opposite, the southerly stream, at Llewellyn's Junction. The course was therefore taken by the one whereby the *Victory* had been brought in, and on Thursday, April 28, Bald Head was passed through again, and after putting in at Orokolo, a course was steered for Motu Motu, which was reached shortly before noon on the following day. York Island was reached on May 1, and Thursday Island on the following day, and on May 3 the *Victory* once more steamed to an anchorage at Thursday Island after an eventful and most successful journey into the interior of New Guinea.

THE WHEAT CROP OF 1887.

SIR J. B. LAWES forwards to us the following information:—

"The very low prices during the last few years have, it is supposed, induced farmers to use a not inconsiderable quantity of their wheat as food for stock. The amount so withdrawn from human consumption is quite unknown. It has been estimated by some to be considerably less than one million, and by others to be even as much as two million quarters within the harvest year. Whatever the amount may be, it is evident that a new element of uncertainty is thus introduced into our estimates of the quantity of imported wheat required to supply the deficiency of the home-grown crop.

"The 'Agricultural Produce Statistics' published at the beginning of the year give, as the result of inquiries in fourteen thousand parishes in Great Britain, and many in Ireland, an average yield of 26·89 bushels per acre for the wheat crop of the United Kingdom in 1886. If we deduct from this amount 2¼ bushels per acre for seed, as we did in the case of our own estimate, it leaves only 7¼ million quarters available for consumption by the population and for stock feeding. The imports less exports for the harvest year ending August 31, 1887, amounted to 17⅓ million quarters, making altogether a total of little over 24½ million quarters. But assuming the consumption per head of the population to be 5·65 bushels, which is the figure we have adopted for the last ten years, the amount so required would, independently of the quantity consumed by stock, be 26½ million quarters, or two million quarters more than the estimated available home produce and imports taken together. By the kindness of Messrs. Beerbohm I have been furnished with a statement of the amount of wheat, and of flour reckoned as wheat, in warehouse on July 1, 1886, and July 1, 1887, from which it appears that the stocks were slightly the higher in 1887, whilst it is estimated that subsequently to that date they somewhat increased.

"Our own estimate of the yield of the wheat crop of 1886 was 29¼ bushels. This is considerably higher than that of the Government above quoted; and it is also higher than the estimates of others. According to our figure, the available supply of home produce was nearly 8 million quarters. Even with our higher estimate of the home crop, there is still a deficiency in the imports for the estimated requirements for human consumption, to say nothing of the amount consumed by stock. The evidence so far would thus seem to suggest the question whether there has not been some decline in the consumption per head of the population. At the same time it should be stated that if we take our own estimates of the available home produce and the recorded imports for the whole period of the eleven harvest years 1876–77 to 1886–87 inclusive, for which we have adopted a consumption of 5·65 bushels per head, the result shows precisely that amount available, if no allowance be made for consumption by stock. It is obviously desirable, however, that those who are engaged in forming the estimates of the yield of the wheat crop should also endeavour to ascertain the facts as to the quantity of wheat consumed by stock."

Sir John Lawes next exhibits tables proving his estimate as to the average yield of wheat at Rothamsted, and explains the peculiarities of the late season with regard to the growth of wheat.

He continues as follows:—"The Rothamsted result of 28⅝ bushels, which more probably under- than overrates the crop of the country, if calculated upon the slightly increased area this year, namely, 2,383,584 acres, gives an aggregate produce for the United Kingdom of 8,454,275 quarters. Hitherto we have always deducted 2¼ bushels per acre for seed, but this is supposed to be too high an average at the present time, and if we deduct only 2 bushels, there remain 7,858,379, or rather less than 8 million quarters available for consumption. Still estimating the consumption per head of the population at 5·65 bushels, the requirement for the harvest year would be 26,419,940, or nearly 26½ million quarters, of which about 18½ million quarters would have to be supplied by stocks and imports.

"For some reason the imports of wheat have been below the estimated requirements for the last two years. Whether, or to what extent, this is due to previous accumulations, to the home crops having been underrated, or to a reduction in the consumption of bread and flour, there is not sufficient evidence to decide conclusively. If there has been a reduced consumption, the question arises whether there has been an increased consumption of other foods. During the last few years there has been some increase in the number of both cows and other cattle kept, but there has upon the whole been a reduction in the number of both sheep and pigs. In fact, the records, neither of the home production, nor of the imports, of animal foods, afford evidence of any material increase in the consumption per head of such foods.

"Further, a careful examination of the amounts of the imports of other articles used as human food shows in the aggregate a reduction rather than an increase in proportion to the population. In such articles as rice and potatoes, for example, which would to some extent substitute wheat, the decline in the imports is very marked. Thus, whilst during the five years 1877–81, the average annual imports of potatoes amounted to 395,277 tons, during the five years 1882–86 they amounted to only 156,017 tons, or to considerably less than one-half. Nor is it probable that the amount of maize flour used has at all materially affected the consumption of wheat. The indication would thus seem to be, therefore, that if the consumption of wheat has really declined, either the total consumption of food per head of the population has also declined, or that the deficiency in the wheat imports has been compensated by increased supplies of home-grown foods. So far as potatoes are concerned, however, the 'Agricultural Produce Statistics' show a decline in area, in produce per acre, and in aggregate produce, both in 1885 and in 1886 compared with 1884. On the other hand, there has, notwithstanding an increase in the imports of other vegetables, been a considerable increase in the area devoted to market gardening during the last few years, and also an increase in the area of allotment gardens. It would obviously be a ground of satisfaction should further information and consideration show that, notwithstanding the very low prices of grain, there has been a larger consumption of some other home-produced foods.

"Whilst it is obviously of importance to the grower that his wheat crop should yield well, it has ceased to be a question of any interest to the consumer whether the yield of the home crop is a few bushels per acre more or less. Nor does such a difference, on our much reduced area, at all materially affect the supply from foreign sources. During the eight harvest years 1852-53 to 1859-60, which were the first of our estimates of the home wheat crop, nearly three-fourths of the aggregate amount consumed was of home growth, and little more than one-fourth was derived from foreign sources; but during the eight years 1878–79 to 1885–86 little more than one-third has been provided by the home crop, and nearly two-thirds by imports; and were it not for the value of the straw for bedding purposes it is probable that the reduction in the area under the crop would have been even greater than has actually been the case.

"Although greater facilities for acquiring land have been afforded by the Acts of Parliament recently passed, there is not much probability that the result will be an increase in the area under wheat, or other grain crops; or in fact that tillage on a small scale will successfully compete with arable farming as at present practised. Nor is it likely that there will be any permanent extension of peasant holdings of pasture land, excepting in localities where the soil and climate are specially favourable for permanent grass. But garden allotments, as distinguished from peasant holdings or from farm allotments, are of very great advantage to the masses of the population, and will no doubt continue to extend as they have done largely during the last quarter of a century."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

King's College, London.—A new laboratory has been fitted up in the College. It is provided with a collection of pathological material, biological, histological, and chemical apparatus, and is intended to afford every facility for obtaining a practical knowledge of bacteriology, and for prosecuting original research in all matters relating to human and comparative micro-pathology.

The laboratory is open to all gentlemen, whether students of other departments of the College or not. The practical courses and lectures are specially intended for medical officers of health, veterinary surgeons, and analysts.

A certificate of attendance will be granted to each member of these courses.

The winter course of lectures with practical work will commence on November 1. There will be about fifteen lectures, and the practical course will last for thirty days. The lectures will be delivered on Mondays, Tuesdays, Thursdays, and Fridays, at 10 a.m.

They will be illustrated with diagrams and typical preparations and followed by practical work in the laboratory for the rest of the day.

Admission will be permitted to the lectures without the practical work.

In the case of medical men in practice, medical officers, and veterinary surgeons of the army, and others whose duties may prevent their attending the laboratory daily, special arrangements will be made for extending the days of attendance over a longer period.

For further particulars apply to Prof. Crookshank, King's College, London, or to the Secretary, J. W. Cunningham.

SOCIETIES AND ACADEMIES.

Liverpool.

Astronomical Society, October 10.—Mr. W. F. Denning, of Bristol, President, in the chair.—This was the first meeting of the seventh session, and sixty-one candidates were proposed as members.—In his opening address the President referred to the last volume of their Journal as exhibiting the varied and attractive character of the work in which the members had been engaged. The angular measurements of fifty binary stars had been completed, and a valuable series of illustrated articles on lunar objects had been published. The remarkable dark patches in the "crape" ring of Saturn were observed and described by members at Bedford, and Louvain in Belgium. There had been a wide-felt regret that the objects of the Society's Eclipse Expedition of August 19 had been defeated by cloudy weather. Observations of Jupiter had been reported, and the increase in the rotation-period of his red spot fully verified. The meteoric section had made considerable progress. To the several members who had so practically aided the Society in its efforts to promote a knowledge of astronomy their warmest thanks were due. The ensuing session gave promise of increased activity, particularly in the stellar, planetary, and meteoric sections. The action of the American members in having so disinterestedly set aside national prejudices to enter into a bond of fellowship with English observers, had afforded great satisfaction, and must lead to a considerable extension in the Society's connexions and sphere of usefulness. The Society owed a debt of gratitude to Mr. W. H. Davies, F.R.A.S., the Hon. Secretary, for the untiring zeal which he had displayed in a very laborious office during several years. Undoubtedly a great future lay before the Liverpool Astronomical Society if its members continued their hitherto united policy. Individual interests and ambitions must be made subordinate to greater aims.

Paris.

Academy of Sciences, October 17.—M. Janssen in the chair.—Catalogue of the Paris Observatory, by M. Mouchez. The revision of Lalande's Catalogue, made in 1791–1800, and containing the positions of 47,390 stars, was begun in 1854 by Leverrier, continued in 1878 by M. Mouchez, and now completed far enough to begin the publication of the results. The first two volumes, which have just been issued, contain the 7245 stars comprised between 0h. and 6h. of right ascension, for which 80,000 observations are recorded. A comparison of the results shows the surprising accuracy of Lalande's observations made with instruments which would now be regarded as very defective.—On the formulas of dimensions in electricity, and on their physical significance, by M. G. Lippmann. Some of these formulas give the idea of a corresponding physical interpretation. But it is shown that no electric magnitude appears susceptible of such interpretation, except where the dimensions may be reduced to those of time, certain electric phenomena having a duration capable of being calculated.—Researches on drainage, by M. Berthelot. Numerous experiments made at Meudon in connection with the study of nitrogen in vegetable soil lead to the general conclusion that the drainage of rain-water carries off a much larger quantity of nitrogen than that supplied to the soil by the atmosphere, and especially by the rain-water itself. This result is destined profoundly to modify the views hitherto accepted regarding the conditions of natural vegetation and of husbandry.—Duality of the brain and of the spinal marrow, by M. Brown-Séquard. It is shown that anæsthesia, hyperæsthesia, paralysis, and various phases of hypothermia and hyperthermia, due to organic lesions of the cerebro-spinal centre, may be transferred from one side of the body to the other. In a word, the author undertakes to establish as the result of a prolonged series of crucial experiments that, contrary to the generally received opinion, each half of the encephalon and of the spinal marrow may equally and independently serve for all the functions of the two halves of these nervous centres. The anæsthesia and analogous affections caused by an organic lesion of the nervous centres are transferred to the opposite side under the influence of a second lesion of those centres; hence it follows that such manifestations are not necessarily effects of the destruction of certain nervous elements endowed with certain functions, but may be the results of purely dynamic actions exercised at a distance by the irritation caused by the lesion. In the same way one half of the encephalon may serve as the seat of the voluntary motions and vaso-motor actions for either half of the body; and so with the spinal marrow, at least so far as concerns sensibility and the vaso-motor actions.—Remarks accompanying the presentation of the second volume of the "Compendium Floræ Atlanticæ, &c.," (Flora of Algeria, Tunis, Morocco), by M. E. Cosson. This volume contains a supplement to the already published notice on the botanical explorations in Mauritania, together with a detailed description of the families, genera, and species from the Ranunculaceæ to the Cruciferæ inclusive.—Observations of Peters's new planet, 270, made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan. The observations extend over the period from October 14 to 16. On the 15th the planet was of the 10·5 magnitude.—A mechanical and automatic registering apparatus of signals transmitted by telegraph and by optical projectors, by M. E. Ducretet. The apparatus here described and illustrated has the advantage over others in general use of automatically recording all messages for the purposes of reference in case of doubt or error occurring in the transmission of signals. It is equally available for ordinary telegraphic service, and for optical, military, and other systems.—Isoclinous magnetic curves, second memoir, by M. C. Decharme. This paper serves as a supplement to the author's previous communication on the isogonous magnetic curves relative to the declination. It deals specially with the isoclinous curves obtained with the dipping needle.—On a new mode of formation of the substituted safranines, by MM. Ph. Barbier and Léo Vignon. It was shown some years ago that the nitrous derivatives of the aromatic tertiary monamines, by acting on the primary monamines, give rise to certain colouring substances. Here are given the results of studies undertaken to determine the true character of these substances.—Researches on the bovine origin of scarlatina, by M. Picheney. The experiments here described tend to confirm the conclusion arrived at in England that scarlatina has its origin in the milk of diseased cows consumed especially by children.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Mattie's Secret: E. Desbeaux (Routledge).—Unfinished Worlds: S. H. Parkes (Hodder and Stoughton).—Bees and Bee-keeping, vol. ii. part 13: F. R. Cheshire (Gill).—British Dogs, No. 12: H. Dalziel (Gill).—Meteorologische Beobachtungen in Deutschland, 1885 (Hamburg).—Report on the Meteorology of India in 1885: H. F. Blanford (Calcutta).—Charts of the Bay of Bengal and Adjacent Sea North of the Equator, showing the Specific Gravity, Temperature, and Currents of the Sea Surface.—Charts of the Bay of Bengal and Adjacent Sea North of the Equator, showing the Mean Pressure, Winds, and Currents in each Month of the Year.—The Vegetable Lamb of Tartary: H. Lee (S. Low).—Austral Africa, 2 vols.: J. Mackenzie (S. Low.)—Report of the Meteorological Service of the Dominion of Canada for the Year ending December 31, 1884 (Ottawa).—The Encyclopædic Dictionary, vol. vi. part 2 (Cassell)—Handbuch der Oceanographie, Band ii.: Dr. Kummell (Engelhorn, Stuttgart).—Cow-Pox and Vaccinal Syphilis: C. Creighton (Cassell).—A Manual for Steam Users: M. P. Bale (Longmans).—Universal Phonography: W. Benson (Chapman and Hall).—Studies in some New Micro-Organisms obtained from Air: Grace C. Frankland and Percy F. Frankland (Trübner).—The Preservation of Fish: J. C. Ewart (Griffin).—Report of the Entomologist, C. V. Riley, for the Year 1886 (Washington).—Modern Lessons in Dynamical Geography and Topography.—Journal of the Chemical Society, October (Gurney and Jackson).—Journal of the Royal Microscopical Society, October (Williams and Norgate).—Indian Meteorological Memoirs, vol. iv. parts 2 and 3—Sitzungsbericht der K. Academie der Wissenschaften; Math.-Naturw. Classe, Mineralogie, Botanik, Zoologie, Geologie und Paläontologie, Jahrgang 1886, April to December (Wien).—Physiologie, Anatomie und Theoretischen Medicin, 1886, January to December.—Mathematik, Physik, Chemie, Mechanik, Meteorologie und Astronomie, 1886, January to December.—Journal of Physiology, vol. viii. No. 5 (Cambridge).—Annalen der Physik und Chemie, 1887, No. 10 (Barth, Leipzig).—Proceedings of the Bristol Naturalist's Society, vol. v. part 2 (Bristol).

CONTENTS.

4, 53, 102, 196/7, 264, 317, 374/5, 221, 567/8, 606/7 | 176, 317